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Analytical and Critical Reviews.

ART. I.

1. *On the Management of the Poor in Scotland.* By WILLIAM PULTENEY ALISON, M.D., F.R.S.E. Second Edition.—*Edinburgh*, 1840. 8vo, pp. 123.
2. *Report on the Sanatory State of the Labouring Classes, as affected chiefly by the Situation and Construction of their Dwellings in and about the Metropolis.*—*London*, 1840. 8vo, pp. 30.
3. *Papers relating to the Noxious Effects of Fetid Irrigations around the City of Edinburgh.*—*Edinburgh*, 1839. 8vo, pp. 94.
4. *An Examination of the Statements contained in the Papers relating to the Fetid Irrigations around the City of Edinburgh.* By WILLIAM TAIT, Surgeon.—*Edinburgh*, 1839. 8vo, pp. 40.
5. *Vital Statistics of Glasgow.* By ROBERT COWAN, M.D.—*Glasgow*, 1838. 8vo, pp. 54.
6. *Hygiène Publique, ou Mémoires sur les Questions les plus importantes de l'Hygiène, appliquée aux professions et aux travaux d'utilité publique.* Par A. J. B. PARENT-DUCHATELET.—*Paris*, 1836. Deux Tomes. 8vo, pp. 552, 708.

Public Hygiène, or Memoirs on the most Important Questions of Hygiène, applied to professions and to works of public utility. By A. J. B. PARENT-DUCHATELET.—*Paris*, 1836. Two Vols.

NOTWITHSTANDING the confident manner in which many very respectable authorities, both new and old, have expressed themselves respecting the Causes and Propagation of Continued Fever, we believe there never was a time when the opinions of reflecting men were more generally suspended or less decided regarding them than at present. For this reason, although we are aware that enquiries are now on foot which promise to adduce much additional information from all parts of the country, we do not think it premature to examine and weigh the materials already in our possession, and to state the results which may be legitimately deduced from them. If we were to content ourselves with

mere opinions, the difficult subjects under discussion would admit of being summarily dismissed in one or other of two ways,—either by selecting from the mass those opinions which are sanctioned by the most distinguished authors, or by receiving without investigation—as seems to have been the custom—every notion that has been entertained regarding them. But we must not forget that the present may be styled as much the age of discrimination and independence as of discovery and enterprise. Men are not now satisfied with knowing an *opinion;* they must take to pieces the machinery by which it is elaborated, and inspect the materials from which it is made, before they decide upon its merits. We see every day evidences of the searching spirit of our times in the crumbling fragments of long-cherished hypotheses which now only litter the path of science; and we would hope that this genius of a better philosophy will soon add to its other triumphs in medicine substantial and intelligible doctrines on the matters in question, fit and worthy to supplant the crude and misty conceits which have so long passed current in their stead.

While it must be allowed that fevers have received their full share of accommodation in the libraries of medicine, in these as in former times, it cannot be with equal justice maintained that the voluminous records of their history have been generally composed in the exercise of a sound and discriminating judgment. Numerous, if not imperishable, are the monuments of industrious contention which have been reared in almost every European language on the track of epidemics. Their origin, diffusion, nature, treatment, have each been the ground of dispute, and still there is but little that can be considered established; men continue to think apart and to think differently. This state of matters we hardly believe to be altogether warranted by the condition of our knowledge of fever, imperfect as it confessedly is. We conceive that enough is ascertained to convince an unbiassed enquirer that our common continued or contagious fever is a disease diversified, not in *kind* but in its degree and external features only, according to the many modifying circumstances which affect it in different classes, ages, situations, and seasons; whether, as Dr. Haygarth remarks, it be "denominated the slow, low, nervous, putrid, petechial, malignant, pestilential, jail, ship, camp, hospital fever," synochus, or typhus. We believe also that enough is ascertained to prove that it is not a local disease, not an inflammation of the brain, of the bronchi, of the blood-vessels, or of the intestines; and that, moreover, there are materials in existence adequate to instruct us—if not indisputably on what the origination of fever depends, at least on what it does *not* depend—a kind of information which, though not the precise thing we most desire to know, is yet of much value in narrowing the question, and which, when fully matured, will shut up the enquiry within very small limits, if it do not enable us to ascertain positively the true causes of fever with certainty.

It would be an idle task to endeavour to ascertain the source of fever while we attach no precise notions to the disease itself, and while we remain in doubt whether it is a specific disease originating under peculiar circumstances, or a disorder of an indeterminate nature, capable of being excited indifferently by a multiplicity of external and dissimilar agents.

While certain specific diseases, such as smallpox, are conceived always to be produced by specific poisons or contagions, it has been a very common supposition, even with those who admit that fever is disseminated by contagion, that its origination may be the effect of external matters by no means fixed or definite in their nature, of a common or general infection, as they term it, by way of distinction from that which is fixed and peculiar. In so far as this doctrine maintains that noxious effluvia of different kinds may produce febrile disorders, of a continued and even typhoid kind, there can be no reasonable doubt of its truth; yet he who admits that the manifold emanations from putrefying carcasses and decaying vegetation, the corrupted exhalations of overcrowded apartments, the steams of the many obscene mixtures which pollute the hovels of the poor, and such other malarial productions as have enjoyed the evil reputation of generating typhus, are capable of producing a fever—may believe that they have no more power to produce our common *contagious* fever than they have to produce measles or smallpox, which may frequent the same localities, and yet cannot be referred to a malarial cause unsettled in its origin and indeterminate in its constitution. It may indeed be maintained that the analogy of certain fevers of definite characters and career, originating under very different conditions, gives support to the supposition that our common fever may be produced by different agents. The yellow fever at one time rages in the low marshes of Fort Augusta, at another on the dry elevation of Stoney Hill; agues abound in the fens of Holland and in the arid and elevated province of Quito, as at Guaillabamba and Salinas de Mira; but it does not follow that the essential causes of these fevers are different, merely because the external features, and more familiar qualities of soil and atmosphere are unlike in those places. The argument that might be founded on the dissimilarity of the local conditions in either case, as tending to prove a dissimilarity in the causes of those fevers, has far less weight than the opposite argument has—of the similarity of the real, though unknown causes being implied by the sameness of the effects. We shall by and bye endeavour to ascertain what grounds there are for supposing, not simply that contagious fever is capable of being produced by several products of a malarial kind, but that it can be produced by any malarial substance whatsoever.

While, on the one hand, the supposed multiplicity of causes from which contagious fever might originate, has tended to prevent the disease from being looked on as of a specific nature, the fact, on the other hand, of typhoid symptoms being, under particular circumstances, common to it with many other disorders, has still farther countenanced the idea of its being devoid of a specific character. There seems also in the minds of some to be a vague notion of the peculiarity of typhus fever, at the same time that it is conceived to be, in certain circumstances, a mere aggravation of other forms of disease. "Typhus fever," says a recent writer, "is not only an endemic disease *sui generis*, but so strong is the predisposition to that form of *pyrexia* that it is prone to become an aggravation and superaddition to other forms of fever; and all the remittent types and degrees, as well as the catarrhal and peripneumonic fevers, are apt, either when long continued or improperly treated under a heating re-

gimen, to glide into it." We apprehend that a good deal of the confusion which exists among us on the subject of fever depends on the importance which has been attached to the *typhoid state.* Nor will this confusion subside until medical men shall learn to consider the typhoid state as merely a group of symptoms which, while it is often ingrafted on common contagious fever as it sometimes is on smallpox, remittent fever, &c., yet is no more an essential part of such contagious fever than of those other diseases. It is surely unnecessary for us to prove that the fever with which the population of this country is so often and so extensively affected, has peculiarities which do not admit of its being confounded with every disorder which may assume a typhoid appearance. Viewed in the mass, and not in single cases and exceptions, it has a definite progress and succession of symptoms, has particular periods of increase and decline, and possesses unequivocally the property of diffusing itself by contagion. When the fevers which have been ascribed to divers malarial substances, and which may have exhibited the typhoid state, shall be found to possess all these peculiarities of the common contagious fever, we shall admit their identity with it; but another question will then remain to be discussed—Have these fevers originated from the causes to which they have been referred?

It is generally admitted that the contagious fever of this country may be and often is generated anew. At the same time there are some who hold with Dr. Bancroft, that this fever is never produced but by the specific contagion, which they presume never to be extinguished; but which having been "the original work of our common Creator, must have been continued in existence by the energies of a living principle, exerted successively in the different bodies through which it has been transmitted from one generation to another." (Bancroft, p. 109.) To reconcile this doctrine with experience the contagion of malignant fevers has been shut up for years, according to some "in holes, and chests, and caves," and has even made its hiding-place in spiders' webs.

As much of the matter in the works before us has been accumulated on the supposition that the continued fever of this country is capable of being generated *de novo*, we shall assume, for the present, the common opinion to be true. Nor does it appear that there need be much hesitation in making the assumption, since it is so much in accordance with the convictions of many practical men of acute discernment. Dr. Bateman says on this subject: "We cannot doubt that a great number of the cases of fever which occur during an epidemic season are entirely independent of contagion for their origin." (p. 12.) Dr. Pritchard, too, observes that "the instances are very numerous in which fever has arisen under circumstances almost precluding the possibility of an origin in contagion; and so many examples of this description have fallen under my own observation, as fully to persuade me that this disease does originate spontaneously or independently of communication with any infected body." (Fever in Bristol, p. 94.) Dr. Percival, and a host of other practical observers, hold the same opinions.

Of the many notions which have been promulgated in respect to the origin of fever, a sufficiently distinctive arrangement may be made under two heads: 1st. That there is a "fever-poison," generated from sources

exterior to the living body. 2d. That certain physical and moral conditions may so act on the operations of the body as to cause it to generate within itself that which produces the phenomena of fever, independently of any exterior poison.

We cannot afford space to recount the many sources to which the fever-poison has been referred, nor the many authors who have yielded their testimony in favour of the generation of this substance exterior to the living body. All that is of real importance in these long-cherished opinions will fall to be considered as we proceed in our examination of the works before us. We turn in the first instance to the "Reports of the Sanatory State of the Labouring Classes in and about the metropolis." In the Report by Drs. Arnott and Kay we have the following passage:

"Besides the malaria arising where nature is uncultivated, we find that, wherever men congregate and bring together the quantities of vegetable and animal substances which constitute their food, in the preparation of which there is much refuse, or where the excrementitious matters from their own bodies (being the matter of their food again rejected, and in another form,) are allowed to accumulate, there is produced another malaria, often as destructive to life as the most active which dwells in an Indian jungle. The fevers called typhus, putrid, malignant, jail, hospital, ship-fever, &c., are the produce of this malaria, and, when once induced, the bodies of persons affected give out a contagious malaria, often more quickly operative on other persons than the original cause." (p. 12.)

In confirmation of this doctrine there is adduced a series of communications from district surgeons tending to connect the prevalence of fever with the existence of various sources of the malaria in question. First, there are references made to the "imperfection, or want of sewers and drains." The following is a specimen of the evidence on this point, from the pen of Mr. Wagstaffe, a parochial surgeon in Lambeth:

"There are, at the present time, many cases of severe fever in and about the parish above alluded to, which have continued for some time.......The primary cause of this infection I believe to be the malaria or effluvia arising from the state of the drains or stagnant filth: the heat of the sun acting upon the mud sends forth this kind of malaria, which, impregnating the air, is the first cause of fever." (p. 17.)

Next, "the existence of uncovered and stagnant drains or ditches, containing vegetable matters in a state of decomposition," is referred to, and its connexion with fever maintained in a communication from the district surgeon, Mr. James Appleton. Alluding to Saffron Hill, which is selected as a case in point, the communication proceeds,

"First, there is an open sewer running the whole length of the district, not a drain but almost a river of filth, which passes under Farringdon street and Bridge street (where it was formerly known by the name of the Fleet ditch), and empties itself at the foot of Blackfriars Bridge: upon the very edge of this ditch many of the poor have their dwellings, so that they may be said to live continually in an atmosphere tainted by it. Next, I may mention that some of the privies in the neighbourhood of this sewer are in a very sad condition; and lastly, the great mass of the houses in this neighbourhood are exceedingly dirty, and contain as many inhabitants as they well can. The great majority of the cases of sickness occurring in this district are in the locality above alluded to; and the diseases most prevalent since I have had the charge (six years and a half) are typhus and continued fevers. Six years ago fever prevailed very much in this particular neighbourhood; and again, for nearly the last two years, we

have never been quite free from it. Many of these fever cases become chargeable to the parish, in some shape or other, principally by being admitted into the workhouse, in which we have been obliged to appropriate one ward to fever cases, and which has seldom been empty for the last eighteen months. The number of fever cases occurring in the year 1837 was about 200; of these about sixty cases were treated in the workhouse. No doubt some of these cases came from various parts of the district. Considering the filthy habits of the people dwelling in this particular locality; considering the privations many of them undergo with respect to food, and their intemperate use of ardent spirits; that they are huddled together in ill-ventilated rooms, and that this place is the resort of Irish lodgers who are travelling the country, *it is exceedingly difficult to give an opinion how far these cases of fever have had their origin in such causes; how far they have had their origin in states of the atmosphere equally affecting the crowded parts of this metropolis; and how far they have originated in the local causes above named;* especially as I must not omit to mention that during three years we had very few cases of fever, and also that this part was very lightly visited by spasmodic cholera, not more than a hundred cases occurring, so far as I know, from its first appearance in this town." (p. 18.)

Then, thirdly, we are introduced to "open stagnant pools of water, rendered putrid by the admixture of animal and vegetable substances," on the evil influences of which the report of the district surgeon is as follows:

"I may state that in my district, comprising Homerton and Mare street, of the Hackney union, I am seldom without cases of a typhoid character, and have carefully searched through my register of sickness from Lady-day 1837 to Lady-day 1838, and find there have been twenty-four cases of severe typhus, of which four were fatal; fifteen of the number were in one locality, named Silk-Mill-row, and Wick street, attributable, I think, to an obstruction by a dam to a mill, which allows a large accumulation of decaying and other matter of a deleterious nature, likely to cause an atmosphere not at all congenial to health, which, aided by, I am sorry to say, the innate want of cleanliness and care on the part of the poor, frequently gives rise to fevers of this description, notwithstanding my very urgent and strenuous endeavours to inculcate the importance of it to their own welfare and comfort." (p. 19.)

Fourthly, we are told,

"Undrained marsh-land is mentioned as a cause of fever in Great Stanmore parish, Edgeware, and the medical officer, Mr. Foote, urges the draining of the marsh at the public expense. Two years past a fever raged at Red Hill, which I attributed to the lodgment of the filth from privies, which I had removed at the time; and the same thing occurred at the Hyde, the fever prevailing there being of the typhoid type; and I consider that unless the ditch is cleaned the same kind of fever will prevail again; and also at the Marsh, in the parish of Great Stanmore, typhus fever lately prevailed amongst the poor." (p. 19.)

And this suffices to *prove* the influence of marsh-land!

Fifthly, "accumulations of refuse, either thrown from the houses or otherwise collected in the streets, courts, and lanes," are specified as causes of fever, and the principal communication on these is Mr. Bowling's letter of May, 1838.

"In reply to your letter of the 27th ultimo, enquiring if any and what cases of fever have come under my care, which have been occasioned by the want of drainage or other causes capable of producing fever, I beg to state, from an experience of thirty years, during which time I have been the medical attendant of the poor of Hammersmith, that we have always had, at certain seasons of

the year, fever prevailing to a great extent among the poor, attributable in a great measure to miasma, produced by a quantity of water which had been left stagnant on the surface of the earth, after brickmaking, and which, in process of time, had become full of vegetable matter. Some years ago this evil had become so alarming that the inhabitants, influenced by the respectable medical men in the neighbourhood, agreed to adopt measures for improving the drainage, and the parish expended considerable sums in so doing, but we have still several places inhabited by paupers without any drainage at all, or what there is so very insufficient that a great quantity of filth of all descriptions is constantly lying on the surface......It appears by the register of sickness and mortality, that we had 104 cases of fever from the 29th of September to the 23d of March, and the greater part of these are certainly to be attributed to causes that might be removed by improved drainage or greater cleanliness." (p. 20.)

Another medical officer delivers himself thus:

"In many parts of this parish a total absence of fever is but of rare occurrence, and it is generally more prevalent in spring and autumn. Although undoubtedly much may be attributed to insufficient drainage, a great deal of disease is produced by the careless and dirty habits of the lower order of people dwelling in many parts of this neighbourhood, who, regardless of all consequences, persist in throwing rubbish and other offensive matters in the streets in front of their houses, which naturally engender much disease." (p. 21.)

Sixthly, the disease is traced to the "lodgment of filth in large cesspools and privies, in situations where the exhalations are destructive of health." The following is the evidence of this:

"You request me," says the medical officer, "in your letter now before me, to describe the nature of such places where fever has most prevailed; to which I reply that fever has been most severe in those courts and alleys where there is no free circulation of air; such as, for instance, Johnson's Change, in Rosemary lane, in which there are about twenty houses, in almost every one of which fever prevailed. The disease first made its appearance there in the month of August last, and on my first visit I found the intolerable nuisance of the overflowing of a cesspool or privy, which continued for some time, there being no sewer to carry off the soil." (p. 21.)

Seventhly. "The situation of slaughter-houses in densely-peopled districts, among narrow streets, and the bad ventilation of these establishments," is commented on in the following letter.

"In answer to your communication of the 27th ultimo, I beg to state that the parish with which I am officially connected comprehends the poorest and most dirty, lowest and worst ventilated, parts of the city of London, chiefly inhabited by the humblest classes of the Irish, and the most abandoned of both sexes; West street, John's court, and Field lane, with the numberless intricate labyrinths and courts, the haunts of prostitutes, pickpockets, and thieves of every description, in which fever seems to have taken up a permanent abode. I have known it to exist there through heat and cold, through wet and through drought, through every variety of weather; and that the district has never been wholly free from it. Owing to the absence of cleanliness, the crowded state of the rooms, six or seven inmates sleeping in one small room, intemperance, the accumulations of dirt and filth that are allowed to take place, all contribute to feed disease and to futilize the efforts of the medical attendant to eradicate it. In addition to this, the number of slaughter-houses that are in the neighbourhood, or on its immediate confines, and the Fleet ditch, the reservoir of all the contiguous sewers, runs underneath these places, above the bed of which many of the houses in the back alleys of Field lane are only a few feet elevated; all

these circumstances constitute the constant source of the generation of contagion. The last six or seven weeks we have been called on to attend many cases of typhus fever, of a very malignant character, chiefly attacking the labouring classes, residing in the dirty and most unhealthy portions of our locality. Upwards of twenty cases have occurred within the last three weeks, three of which terminated fatally, two taken into the workhouse, &c. The majority attacked were those who were unable to procure adequate nutriment, from want of employment during the last inclement winter." (p. 22.)

In the eighth place there is an account of the bad effects of certain burial-grounds in crowded districts evinced by the fact that "a court filled with poor people (not forty yards from one of these burial-grounds) was attacked by fever." (p. 23.)

And finally, reference is made to "the want of ventilation in narrow alleys and close courts inhabited by the working classes," in which there is little else than a repetition of what has been stated under the previous heads.

At page 13 of the Report there are some curious particulars of the effects of malaria from some of the causes noticed above—for it appears that the production of contagious fever is only one of the many ailments which may flow from the state of matters to which we have been referring. The following are some "illustrative facts" by Dr. Arnott.

"1. In the field near Euston square, towards Somers-town, now occupied by the commencement of the Birmingham railway, there was until lately, near some very extensive cow-sheds, the meeting of several public drains or sewers in an open ditch, which often overflowed and covered a considerable space with a lake of the most odious filth. In the neighbourhood of this field typhoid fevers were frequent, and in a school of 150 female children in Clarendon square, Somers-town, every year while the nuisance was at its height the malaria caused some remarkable form of disease. In one year it was an extraordinary nervous affection, exhibiting rigid spasms, and then convulsions of the limbs such as occur on taking various poisons into the stomach: more than thirty of the girls were thus affected. In another year it was typhoid fever, affecting an equal number of children; in another, ophthalmia; in another, extraordinary constipation of the bowels, and so forth. Since the covering of the drains all these diseases have disappeared."

Before passing on to the next part of the report, which is from the pen of Dr. Southwood Smith, we would beg to offer a few remarks on the views illustrated by the extracts we have given. We must candidly state our extreme surprise that in these days such meager details as the above should have appeared to the gentlemen who drew up the Report to justify the strong opinion which they express on the origin of contagious fever. If they will but leisurely consider the documents they have selected for publication, it is hardly possible that they can fail to observe—what everybody else must see at once—that the utmost they have succeeded in proving is simply the *coincidence* of fever and the offensive effluvia of filth, while there is not the vestige of anything bearing the semblance of *proof* that the former stands to the latter in the relation of an effect to its cause. Having allowed themselves to prejudge the question, the authors of this Report appear to have entirely forgotten that fever could possibly originate from anything but malaria; and therefore hesitate not to maintain "that by proper sanatory *police regulations*. . . .

. . . the typhoid fevers of London and other places might be made to disappear." (p. 14.) No doubt the subject is an involved one: there are many circumstances affecting the class in which fever chiefly prevails, claiming attention in the investigation, among which the offensive state of their dwellings and neighbourhood occupies a very prominent place: yet we think that the philosophical enquirer, with only such materials before him as those on which we are commenting, has no ground for any other conclusion than this, in respect to bad air (malaria) and fever,—that both are common attendants on poverty. How far other, both closer and more constant companions of destitution, may operate in the production of fever we shall have occasion to notice as we proceed; meanwhile we would direct the attention of our readers to those passages in the extracts which prove the existence of extreme privation among the unhappy persons who are described as most liable to fever. Without stopping at present to comment on the many improbabilities which readily present themselves in the conclusions of Drs. Arnott and Kay, we would specially notice, in passing, the difficulty expressed by one of their correspondents, at p. 18, in determining on which of the many peculiar circumstances which affect the poor fever more particularly depends, and to the important testimony of Mr. Evans, p. 25, whose letter is given for another purpose. "In looking over my books," says Mr. Evans, "I find that, in the space of nine months I have attended upwards of 500 pauper cases (of fever), but I cannot trace the disease to any local cause; for we have in the parish of St. George very good drainage through the parish, and very little accumulated filth, with the exception of Falcon court, White street, Noel's court, Hunter street, and Peter street (Mint), but here the disease does not exist more severe than over the parish in general." Of the curious "illustrative facts" of Dr. Arnott, we surely require to say very little: no one knows better than that most ingenious physician and philosopher that an apparent cause may be very far from being a real one, and that the recent removal of a nuisance and the simultaneous cessation of a disease may have no mutual dependence.

The Supplement No. 2 of the Reports is the production of Dr. Southwood Smith. From this we make the following extract:

"The exhalations which accumulate in close, ill-ventilated, and crowded apartments in the confined situations of densely-populated cities, where no attention is paid to the removal of putrefying and excrementitious substances, consist chiefly of animal matter; such exhalations contain a poison which produces continued fever of the typhoid character. There are situations, as has been stated, in which the poison generated is so intense and deadly that a single inspiration of it is capable of producing instantaneous death; there are others in which a few inspirations of it are capable of destroying life in from two to twelve hours; and there are others, again, as in dirty and neglected ships, in damp, crowded and filthy gaols, in the crowded wards of ill-ventilated hospitals filled with persons labouring under malignant surgical diseases and some forms of typhus fever, in the crowded, filthy, close, unventilated, damp, undrained habitations of the poor—in which the poison generated, although not so immediately fatal, is still too potent to be breathed long, even by the most healthy and robust, without producing fever of a highly-dangerous and mortal character." (p. 32.)

One object of Dr. Smith in this and a previous passage is to show

that while exhalations from sources in which vegetable matter predominates produce what are known as the marsh fevers, those which proceed from putrefying and excrementitious substances in populous towns produce the continued typhus fevers, because they are the products of animal matter chiefly. A shade of probability or congruity may possibly be supposed to arise from this ingenious account of a presumed adequate difference in the poisons. But even this plausible appearance of completeness in the theory is destitute of any real value. We apprehend that animal matters do not enter so largely into the commissariat of the poor as to leave so formidable an accumulation of relics as shall materially taint the atmosphere in which they live; and we know no process by which the feculent remains of their vegetable diet can be so transformed that the impure heaps which surround their dwellings can be justly esteemed impregnated with a preponderance of animal substances. The passage we have quoted and its context are objectionable also on this ground, that there is a manifest confusion of the known asphyxiating effects of mephitic gases with their presumed capacity to produce typhus fever. If the several statements of Dr. Smith were put into the simple form of the only proposition which they really contain, they would amount merely to this, that exhalations from certain putrescent matters have the power of producing both asphyxia and continued or typhus fever; the former of which is a result familiar to all, and the latter a mere assertion deriving a little hue of probability from its juxtaposition to a known truth. There is a wide difference between the asphyxia which is caused by mephitic gases and typhus fever—a difference which can never be explained, as Dr. Smith attempts to do, by a reference to the diversity in the doses of the poison. We presume that if a few doses of the poison in its less potent shape were sufficient to create typhus fever, *a fortiori* such a quantity of it in a more concentrated form as would be capable of producing a state of asphyxia not ultimately fatal, would, commonly at least, leave the sufferer for days or weeks in the toils of a "highly-dangerous" fever; yet the reverse is the case, as the histories of mephitism amply demonstrate.

A large portion of Dr. Smith's Report is occupied by details of the disgraceful condition of many parts of the Bethnal Green and Whitechapel districts, in respect to filth and defective sewerage; and it would appear that they richly merit his assertion that "it is not possible for any language to convey an adequate conception of the poisonous condition in which large portions of both these districts always remain, winter and summer, in dry and in rainy seasons, from the masses of putrefying matter which are allowed to accumulate." (p. 34.) That this "poisonous condition" is the source of the fever which frequently prevails to a considerable extent in these unions is the doctrine which is maintained throughout, because "from the constant prevalence of fever in these and other districts, it could not be doubted that the poison of fever is constantly generated in these places;" and from the local causes —the depositories of filth, which are described in ample detail. We need not adduce his evidences of the neglected and filthy state of the [illegible] which he has selected for description; they form but a more [illegible] picture of the same kind as those which we have already

noticed. In the following quotation, however, there are some things worthy of a moment's consideration.

"There is evidence, derived from the history of these very localities, that the formation of a common sewer, the filling up of a ditch, the removal of stagnant water, and the drainage of houses, have rendered a district healthy, from which, before such measures were adopted, fever was never absent. This is strikingly exemplified in the present healthfulness of the upper part of the Hackney road, in which an excellent common sewer has been *recently* made, the neighbourhood of which is now well drained. In this part of the district no case of fever is known to have occurred during the present epidemic (1838), although formerly the houses, even in the principal thoroughfare, and more especially the streets, lanes, courts, and alleys adjacent, were the constant seats of fever. A still more striking illustration of this fact is afforded by the altered condition as to the health of the lower part of High street, Aldgate, in the jurisdiction of the city of London. The south side of this street is occupied by butchers, and the slaughter-houses are behind the street. Formerly this place was in an exceedingly filthy condition; at that time fever of a typhoid character was *occasionally* prevalent in all this neighbourhood. About three years ago a common sewer was made by the corporation of London in this street, into which, after incredible trouble, the commissioners succeeded in inducing the butchers to open drains from the slaughter-houses and the dwellings around. Even now the blood and filth from the slaughter-houses lie sufficiently long on the surface to produce an offensive odour; but on account of the excellence of the drainage, the same particles of matter do not lie sufficiently long to putrefy. Fever has been comparatively absent from this neighbourhood ever since the opening of these drains. Dwellings thickly crowded with inhabitants, stand all around the slaughter-houses, yet here, where the materials for the production of the worst forms of fever are most abundant, scarcely a case has occurred even during the present epidemic. On the other hand, in the passages, courts, and alleys, on the very opposite side of the street, from the houses of which there are no drains into the common sewer, fever of a fatal character has been exceedingly prevalent. I have myself *very recently* attended several families in these courts labouring under the worst forms of spotted fever; but I have neither seen nor heard of a case on the opposite (the south) side of the street; whereas there is hardly any part of Bethnal-green or Whitechapel in which fever has been more prevalent or fatal than in the streets, courts, and alleys which go off from High street, Whitechapel, continuous with High street, Aldgate, to which the beforementioned sewer does not extend. In the streets, courts, and alleys just adverted to, which branch off from the main street of Whitechapel, there is either no drainage at all, or what there is is superficial and exceedingly imperfect." (p. 35.)

Without adverting to the proofs which exist of the incapacity of such effluvia, as are here specially referred to, to originate contagious fever, we take the passage simply on its own terms as professing to detail the *experimentum crucis* in respect to the agency of malaria. We object, first, to the insufficiency of the facts. Every one who has attended much to the local histories of fever is familiar with the circumstance that in the same town the disease is very liable to shift its seat, not only during the persistence of one epidemic but in successive epidemics, so that the region specially affected by one of them may enjoy even a total exemption from its successor, though no changes have occurred in the locality during the interval. Dr. Smith gives us no precise statement of the history of the south side of High street, Aldgate, in respect to such exemptions, but he admits that while in its filthy state fever was only "occasionally prevalent" there. Why not *always*, since "formerly this place

was in an extremely filthy condition?" There must surely be a something else than malaria necessary to generate fever, if, the local impurities remaining the same, fever was only occasionally present. The recent nature of the experiment in both the Hackney road and the High street in question deprives the statements of any degree of weight in respect to the point at issue.

In the second place, the accounts given by Dr. Smith, if they prove anything in regard to the effects of the improved sewerage, prove rather too much. They prove that a common sewer may deprive fever of its contagious power. We have the remarkable phenomenon of "dwellings thickly crowded with inhabitants," and these inhabitants the poorer classes, rendered incapable of harbouring the contagion of a highly contagious disease in consequence of the "excellence of the drainage!" The new sewer, if the recent exemption is to be traced to it, proves just as much in respect to this as to the former conclusion. How much more just it would have been to have ascribed the late freedom of the locality from fever, as well as the former occasional exemptions, to the non-introduction of contagion, we leave our readers to judge; for even Dr. Smith does not exactly hold that good drainage will prevent the operation of contagion, as appears from his expressions at p. 36, "fever will sometimes spread among the inmates even in the best drained, the best ventilated, and in all respects the best regulated houses," (workhouses.)

After concluding his account of the filthy state of Bethnal-green and Whitechapel unions, and of the prevalence of fever in those districts, Dr. Smith observes, "The previous statements connect in the clearest manner the prevalence of fever with poisonous exhalations arising from putrid vegetable and animal matter" (p. 43); and in further illustration of the same opinion he quotes at length from Pringle's Observations on the Diseases of the Army, an account of the malaria and its effects which were witnessed in Flanders. The author appears, strangely enough, to confound the origin of the remittent fevers of the Low Countries, which are incommunicable diseases, with that of typhus, and to draw an argument in favour of the malarial origin of the latter from what has been amply acknowledged in respect to the former; a course about as justifiable in medical logic as it would be to trace measles to almonds or oysters because these can produce the nettle-rash, which ranks also among the exanthemata. It is astonishing to us who know Dr. Southwood Smith's great powers of mind and his acute and logical spirit, that he could have allowed such a passage as the following to drop from his pen. The only way we can account for this and several other things in the work before us is by supposing that the enthusiasm of philanthropy and a vivid imagination have secretly jumped to desired conclusions, while the cool and calculating eye of philosophy was closed in slumber.

"The room of a fever patient, in a small and heated apartment in London, with no perflation of fresh air, is perfectly analogous to a standing pool in Ethiopia, full of the bodies of dead locusts. The poison generated in both cases is the same; the difference is merely in the degree of its potency. Nature, with her burning sun, her still and pent-up wind, her stagnant and teeming marsh, manufactures plague on a large and fearful scale. Poverty in her hut, covered with her rags, surrounded with her filth, striving with all her might to

keep out the pure air and to increase the heat, imitates nature but too successfully; the process and the product are the same; the only difference is in the magnitude of the result." (p. 33.)

How all this has been determined we need not ask, because we are sure that Dr. Smith would be puzzled to reply; but we must record our regret that in these days of laborious research, loose notions, like the above, should emanate from men of high reputation and deserved influence, as they cannot fail to have the effect of hindering the progress of truth. Dr. Smith illustrates and supports his doctrine of the malarial origin of continued fever, by referring to facts which relate merely to periodic fevers; and he maintains the identity of the "fever-poison" of this country with the poison of the plague; wherefore, on the principle that things that are equal to the same thing are equal to one another, plague and ague are generated by the same poison!

Quitting these speculations, which carry us a century back in etiology, we would observe, that the direct evidences which may be adduced to prove the insufficiency of the exhalations from putrefying organic matter to generate contagious fever are neither few nor inconclusive; and that even if such evidences were defective there would, notwithstanding, remain the necessity of proving that such malaria is the source of fever, in preference to other circumstances existing in the same localities, and its poisonous effects in respect to fever not a mere fancy which the long habit of repetition has dignified with the importance of a dogma. The many facts which have been accumulated in evidence of the innocency of putrescent exhalations must, partly at least, be known to those who contend for the malarial origin of fever; we should therefore have expected that instead of contenting themselves with adducing instances in which malaria and fever coexisted, they would have taken some pains to expose what inaccuracies they perceive in the opposite details. But this they have omitted to do.

There are some old and some new histories at utter variance with the conclusions and hypothesis of the Reports on which we have been commenting. Although it has been justly remarked, that a fact never becomes old, that is to say, superannuated, we shall pass by the testimony in exculpation of malaria as the source of fever which is yielded by the exhumations at Dunkirk in 1783, and at Paris in 1786, as well as the many details collected by Chisholm and Bancroft, all of which are well worthy of being considered; and shall introduce to our readers, what may be new to many of them, the researches of Parent-Duchâtelet, the notice of whose admirable work, in our Fifth Vol. (p. 447), was, we regret to say, much too imperfect. Fifteen years of a life which terminated prematurely for the science to which he had devoted his uncommon energies, witnessed his unwearied application to his favorite study, while they furnished him with ample materials for the training of a mind deeply imbued with the true spirit of induction. We find in the essays of Duchâtelet, almost everything we miss in the pages of the "Report." We find enquiries conducted on a comprehensive scale, circumstances noted, weighed, and compared, with a care and precision commonly confined to the experimental sciences; and, therefore, conclusions that claim our confidence and assent.

The Essays of Duchâtelet embrace many subjects connected with the public health, and among them we find some to our present purpose. The immense reservoir of impurities at Montfaucon can scarcely be surpassed in fitness for an enquiry respecting the effects of putrid effluvia.

"The influences of this *voirie*," says Duchâtelet, "ought to increase equally with the quantity of matter which is deposited there; a multitude of facts prove, and reasoning alone may suffice to demonstrate, that the intensity of offensive emanations which proceed from any place is always in proportion to the quantity of the matters which furnish these emanations: at present, those which proceed from the '*voirie*' of Montfaucon, are constantly insupportable in a circumference of two thousand yards; the winds carry them sometimes, in all their intensity, to more than four thousand yards; and it appears from accounts collected by the commission charged with ascertaining the ravages of cholera in the rural districts, that certain atmospherical conditions, rare it is true, conveyed them to the extent of eight miles. Could it be otherwise, since the basins alone of this reservoir contain 32,800 yards of surface, without reckoning twelve acres occupied by dry matters and the curriers' yards; since from 230 to 244 cubic yards of the products of the 'fosses d'aisances' are carried thither daily; and since the greater part of the bodies of twelve thousand horses, and from twenty-five to thirty thousand small animals are left to rot on its surface!" (tom. ii. p. 287.)

More in detail we have an account of the curriers' yards situated on the borders of the general receptacle.

"The nature of the operations which are practised in those yards, the fetid emanations which proceed from them, and the disgusting and destructive animals of which they favour the increase, have always made them to be considered as dangerous places which ought to be far removed from habitations. .... In reality nothing can be compared to the fetid effluvia (l'infection) produced by these establishments; everything there announces negligence and barbarism; their aspect alone induces one to draw back with horror; and one asks himself, on beholding them, if he be actually in the nineteenth century, and at the entrance of a city which makes pretensions to be the capital of the civilized world." (tom. ii. p. 123-4.)

Into this central establishment for the slaughter of horses, it is calculated that 12,775 of these animals are annually brought. In the "chantiers d'équarrissage," the bodies of these animals undergo a multiplicity of operations; the hair, the hides, the flesh, the bones, the entrails, all are more or less the subjects of labour and the sources of effluvia.

"Let one figure to himself what may be produced by the decomposition of heaps of flesh and entrails abandoned, for weeks and months, in the open air and to the heat of the sun, to spontaneous putrefaction; let him add, in thought, the nature of the gases which can arise from the heaps of carcasses which remain covered with much of the soft parts; let him further add the emanations furnished by a soil, which for years has been drenched with the blood and fluids of animals; those which emanate from this blood itself which, in both of the yards, lies on the pavement without being able to escape; those, in fine, of the kennels of the gutspinners and dryers in the neighbourhood; let one multiply as much as he pleases the degrees of stench, by comparing it to that which every one of us has been enabled to perceive on passing near the bodies of animals in decomposition, which it was necessary to encounter, and but a feeble idea shall be formed of the truly repulsive odour which emanates from this sink, the most offensive that it is possible to imagine!" (tom. ii. p. 222.)

Surely these descriptions prove the existence of a malaria, compared to which the effluvia of a slaughter-house or a common sewer are as nothing; surely in such a place, if anywhere, fever must haunt the habitations of men, and fetter its victims in the helplessness of the most malignant forms of typhus. Let us see what Duchâtelet has ascertained on this point.

"If we ask the master *équarrisseurs* and the workmen (knackers), one after the other, they reply to us, that they are never ill, and that the emanations which they continually breathe, far from being hurtful, contribute to their good health: this evidence is assuredly important, but it does not suffice. Let us seek for proofs more convincing. If we examine them, we shall see that they bear all the characteristics of a health the most flourishing, and that in this respect they resemble much our butchers. We are not the only persons who have made this observation, as proved by the passage we have quoted from a Report made in 1810 by MM. Deyeux, Parmentier, and Pariset, on the yard which existed then at the Gare. These gentlemen speak of the surprise they felt at the brilliant health of the woman named Fiard, and her five children, who laboured the whole year in the yard, and slept in the same place, where it was impossible for the members of the commission to penetrate, on account of the extreme fetor which it exhaled. All, however, do not attain the robustness which is common to the majority; some remain thin, though preserving good health. We have made this remark both on males and females. . . . . Are the chances of longevity less favorable among these than among other artisans? Everything appears to prove the contrary. We see many *équarrisseurs* who are sixty and seventy years old, and who are perhaps the strongest and the most active of those who work in the yards of Montfaucon. We have taken precise notes respecting their fathers and mothers, and have learnt that they all died at a very advanced age, and almost always exempt from the infirmities of old age. . . . . These singular facts, so much in opposition to what has hitherto been published respecting the influence of putrid exhalations, are confirmed by the long experience of MM. Damoiseau and Huzard, and especially by the latter, who for sixty years has not ceased to have almost daily intercourse with the *équarrisseurs*. It may be said that these workmen, born so to speak in the trade, and all the issue of parents who have exercised it, have lost the capacity of being influenced by putrid emanations, which yet exert all their activity on others: we reply to this objection by the following facts: The strangers who come often, or even every day to the yards, and who remain there a longer or shorter time, are not incommoded." (tom. ii. pp. 228-9.)

This statement is illustrated by the fact that new workmen occasionally employed are not remarked to be more subject to disease than the others. Duchâtelet made enquiries also among the quarriers and plaster-makers of the vicinity, who are exposed to all the influences of the yards; and though all agreed respecting the discomfort produced by the effluvia, yet none of them accused these as affecting the health of their numerous workmen, although they labour while the putrescency is at its greatest degree of intensity, during the heats of summer. *Apropos* to these details M. Duchâtelet introduces a notice of certain other sources of putrid animal exhalations which exist in Paris. Every year nearly two hundred exhumations are performed at one of the cemeteries, in order to transport into new grounds, or tombs, bodies temporarily deposited in particular vaults; and although these exhumations are carried on at all seasons of the year when the bodies have been several months dead, and putrefaction going on with activity, yet it has "never hitherto been remarked that the least accident has happened to the workmen en-

gaged in these labours, which are the more toilsome, and ought to be the more dangerous that they are obliged to breathe, in the vault itself, the emanations which have been shut up for a long time in a narrow space." (tom. ii. p. 230.) He brings also to the support of these facts the observations of MM. Guersent and Labarraque, who have proved that the manufacturers of gut-strings enjoy the best of health, though living in an atmosphere of effluvia, and continually in contact with intestines undergoing a lengthened maceration.

It would appear that there have been in Paris, as well as in London, functionaries abundantly disposed to confide more in their organ of smell, than in their reasoning faculties, while investigating the condition of the public health, and the sources of disease, of which the following deliberate determination, expressed in a Report for 1832, and quoted by Duchâtelet, affords an instructive example: "As for us, in spite of all the reasonings of tradesmen, and all the logic of science, our spirit refuses to believe that establishments so loathsome as those of Montfaucon, do not present any cause of insalubrity." (p. 237.) So much for the indomitable spirit of prejudice.

In the same volume there is an account of the effects of the atmosphere of the Parisian dissecting-rooms, on the health of those exposed to it, of which we gave a pretty detailed notice in the article already referred to. (Br. and For. M. Rev. V. p. 452.) We shall here merely repeat the result of M. Duchâtelet's own experience of five years' connexion with the dissecting-rooms: he says, "We can affirm that the number of those who have become ill in a serious manner, during the course of their anatomical studies, has not been one in a hundred; now, when of 100 individuals, subjected for six months to the action of any influence, ninety-nine experience nothing; it appears to us, that in good logic, the exception furnished by the hundredth ought to be considered as null." (tom. ii. p. 36.) These will be esteemed no despicable facts by those who have spent some hours in a Parisian dissecting-room; and by those who have not, some faint conception of their atmosphere may be formed, when it is stated that, in the rooms of Clamart, from 1200 to 1400 bodies are annually dissected, and that no particular attention is bestowed on cleanliness. There are many other accounts of the exposure of persons to the effluvia of decomposing animal matter, contained in Duchâtelet's work, of which we can make no special mention. Before quitting the consideration of these animal effluvia, however, we would briefly notice a passage illustrative and explanatory of the opinions entertained on the cause of fevers connected with military operations. While commenting on certain objections urged against the salubrity of a particular manufactory, Duchâtelet observes, "You speak of the typhus which the projected manufacture might produce; you say that in 1814 fevers and typhus were seen to follow the disastrous battle of Paris. We have seen the typhus, in 1814, accompany the French army in its retreat, and make dreadful ravages in it; we have seen it arrested spontaneously in April of the same year, and since that time, it has not come to our knowledge that any one has observed it in an epidemic manner near Montfaucon, or anywhere else; as to the battle of Paris, if the emanations from the bodies could give origin to typhus, it is in the villages of Belleville, Pantin, and Prés-Saint-Gervais, that it should have been noticed, be-

cause the most deadly actions took place within their walls, or in their vicinity." (tom. i. p. 47.) The action occurred on the 30th of March, 1814, and of the results which might be presumed to affect the health of the neighbourhood, it is stated that "4000 horses were collected in the neighbouring plains; they were all accumulated at the same place;.... for ten days they exhaled a noisome stench; however, they were not injurious either to those who directed the operation (of burning them), or to the numerous workmen who seconded them." (tom. i. p. 48.) At the villages specified above, it has been ascertained from the registers, that fewer deaths occurred in April, 1814, than in the same month of the year before and the year after. These facts afford good ground for suspecting that other causes should be blamed for the prevalence of fever in bodies of troops than the infected air of battle-fields, to which it has, from time to time, been referred; and, indeed, the other accounts which we have quoted from Duchâtelet strongly attest the insufficiency of such a state of the atmosphere to produce the effect ascribed to it.

In the manufacture of a particular manure from the contents of the "cabinets d'aisance" and sewers, deposited in the general reservoir at Montfaucon, exposure to fetid exhalations is so great, that those occupied in this employment should of necessity be the subjects of any evil effects which such exhalations are capable of producing. This manure is termed Poudrette, and is neither more nor less than dried ordure. Circumstances led Duchâtelet to make enquiries respecting the health of the persons engaged in this disgusting business; and the following is a summary of his observations.

"Before engaging in the researches, of which I am about to state the result, I was so persuaded that the 'voirie' of Montfaucon, would furnish me with the *data* which were necessary, that I regarded the question as already settled; I confess that my surprise was great, when instead of encountering a population frail and languid, as I expected, I did not see a single workman who did not present all the exterior signs of the best health. I was at first tempted to attribute to habit this faculty, which they had to remain unscathed in the midst of fetid odours and emanations, which I regarded as very dangerous; but it was easy for me to see that habit went for nothing, since the new workmen, whom they received every day, are not more affected after three or four weeks than those who have laboured there ten and twenty years; moreover, there exists, among the workmen of the 'voirie' an opinion that the emanations which proceed from it, far from being noxious, have, on the contrary, a salutary influence on the health, that they protect from epidemics and cure many diseases." (tom. ii. p. 273.)

Though inclined to question these latter conclusions, while resting on the authority of these workmen alone, because they might possibly be interested in concealing the truth; yet, being open to conviction from adequate testimony, he could not but yield credit to the facts when attested also by the agricultural population, the carriers, and manufacturers of plaster, resident in the vicinity, although directly opposed to his previous opinions. It is necessary to add to the foregoing statements an account of the manufacture in question, and of the locality in which it is conducted; the details of which are of a nature which throw far into the shade the descriptions of impurity and loathsomeness which we had previously perused. The "voirie" of Montfaucon, is composed of six basins on successive elevations. In the two highest are deposited the

contents of the privies of the city on their arrival at the "voirie;" th four others are but receptacles of the liquid matters which flow fron the upper basins. The average quantity of matter transported annuall to these basins, amounts to above 16,000 loads. The matter in the highes basin, which, from the partial separation of the more liquid substance acquires a pasty consistency, is that which is dried for the purpose o making one sort of "poudrette;" for this purpose it is withdrawn fron the basin, is spread on certain neighbouring pieces of ground appro priated to the purpose of drying it. When sufficiently dry it is raisec in heaps eight or ten yards high, and from sixty to eighty long. In thi form it remains for a long time, sometimes for several years; and, finally it is reduced to powder. In this last state it presents the appearance o a dark gray earth, of little weight, unctuous to the touch, friable, an exhaling a nauseous odour. Another kind of "poudrette" is made fron the liquid matter of the basins, which consists simply of urinary sub stances, with more or less of the matter already described. That thes several processes are eminently calculated to afford the amplest scop for the exhalation of every noxious principle which filth can contain will scarcely be questioned; yet it is in an atmosphere offensive beyon comparison, in consequence of these exhalations, that health and re markable exemption from epidemic diseases have been ascertained t exist.

Our readers need not apprehend that we are about to advocate th salubrious qualities of fetid effluvia, or to defend the neglected and filth condition of many parts of our large towns. We shall by and by show that the freedom from epidemic diseases which the workmen a Montfaucon enjoy, can be traced to another peculiarity of their situation and that the details which we have quoted, afford in reality no plea fo nastiness; but *il ne faut pas faire le diable plus noir qu'il n'est.*

We cannot quit the work of Duchâtelet without noticing his accoun of the stream of Bièvre, which traverses a part of Paris, and its effect upon the public health. He informs us that it is difficult to form an conception of the state of this stream in summer, when its bed is nearl dry; its breadth is from eight to ten feet, and it varies in depth fron seven feet to one or two; and scarcely has its mud been rendered some what dry than it cracks and sends forth exhalations "insupportable t those who pass in the vicinity, and much more so to those who dwell o its margin." This state of matters begins to be noticed above Gentilly but is much more remarkable in Paris, "where the mud, entirely com posed of the *debris* of animal substances, swells when it is dry, pufl up, and bursts, precisely like dough subjected to fermentation." (tom. i p. 128.) In consequence of the great exhalation of gases from the pu trefying mass, meat cannot be preserved during summer more than eigh or ten hours; plate and the "batterie de cuisine" are altered and tar nished, for it is chiefly where the stream flows among the houses, an ventilation is prevented, that the evil is the most distressing. "One woul conceive, at the first sight, that such a focus of putrescency, traversin a quarter inhabited by 30,000 individuals, for the most part in povert and crowded together, should produce serious diseases, or at least giv to this population an appearance of languor and debility as the predomi nant character of their constitution; such is at least the idea generall

entertained in Paris respecting this part of the suburb of Saint Marceau. . . . . . . It is not without lively satisfaction that we are enabled to reassure the inhabitants as to the results of the influence which the exhalations of Bièvre have on the health of those who are exposed to them, whether temporarily or continually." (pp. 129-30.) Although prejudiced against the stream and its influences when he began the enquiry, he found it impossible to establish any difference in the physical constitution of those who resided on its borders and of those who lived in other quarters. The register of a dispensary existing in this district for twenty years exhibits no peculiarity in the maladies prevailing among the inhabitants, nor anything in the diseases in general different from those of other quarters. The truth of these conclusions is attested in the strongest manner, and established by enquiries and observations made in every season of the year. To the same purpose is the account given of the health of those engaged in cleaning the common sewers of Paris, often a fearful business it would appear; but we have not space for any details on this head.

The publications which relate to the "fetid irrigations" practised in the vicinity of Edinburgh, do not for the most part contain anything very clear in respect to their properties, with the exception of evidence that they are abundantly offensive. It seems that on two sides of the city a considerable extent of ground is rendered highly productive of a luxuriant grass by means of certain common sewers.

"Without affecting precision, we may say that the meadows to the west extend two miles in length and one in breadth, and contain about two square miles, while those to the east of the city may be about two miles long by half a mile broad, and contain perhaps one square mile. The sum total is three square miles, that is, 1920, or about 2000 acres, equivalent to 87,120,000 square feet of poisonous swamp. This, then, is the surface covered with the drainage of the city, where it is spread out in small currents, which sink into the ground till it be completely saturated like a sponge. The sun, acting on the ground so prepared, decomposes the animal and vegetable matter there deposited, and gives rise to all those effects formerly enumerated; while, whatever wind may blow, the exhalations are wafted from one or the other system of drainage to every part of the town." (Papers relating, &c., p. 22.)

This very reprehensible system of irrigation has been gradually increasing in extent for the last thirty years, and now amounts to so great a degree as to become extremely offensive in its effects on a considerable portion of the town, especially on its eastern skirts. Contagious fever has been gradually on the increase also in Edinburgh for above twenty years, and nothing is more natural than that the two evils should be regarded somewhat in the light of cause and effect. We need not enter into the evidence which is adduced to prove the insalubrity of the irrigations, the more especially as nothing appears to be proved but that disease exists within the range of their exhalations, and that if offensive effluvia from decaying animal and vegetable matter be capable of producing putrid diseases, the vicinity of those fetid marshes ought to present ample testimony to the fact. On this subject little is adduced beyond mere opinion, and where facts are stated they are much of the inconclusive character of those contained in the reports already referred to.

Very different are the facts adduced on the opposite side (for the sub-

ject has been stoutly contested), particularly in respect to the connexion of the irrigations with fever. The principal facts are the following: Fever attains its greatest amount in winter when the irrigations are discontinued, and in summer, when the irrigated grounds are sending forth their effluvia in such abundance and intensity as to produce sickness and other disagreeable effects on those passing in their vicinity, fever is very generally at an extremely low ebb. The greatest number of cases of fever does not occur in these districts more immediately in contact with the marshes, a circumstance which is attested by the records of the Fever Board for four years prior to 1838, the particulars of which are contained in page 38 of "An Examination of the Statements, &c.," and has been confirmed to ourselves by information of a corresponding description derived from the Records of the Royal Infirmary of Edinburgh. "Fever is not confined to any particular locality in Edinburgh, with this exception, that it is generally most prevalent where the inhabitants are poorest." (An Examination, &c., p. 25.)

It may be said that these statements in reality prove nothing in respect to the question at issue, because typhus fever is not maintained to be the offspring of marshes. These irrigated lands are, however, not mere marshes; they are rendered fetid, not by the decay of the vegetable products of the soil, but by the contents of the city drains and sewers—the very matters to which so much is ascribed in the generation of fever.

We sometimes hear and read of contagious fever being referred to the exhalations from putrefying mud on the margins of rivers, ditches, and ponds, and the fever of the metropolis we observe to be referred to this source from time to time. We need scarcely remark that this is the very source to which the worst forms of the periodic fevers are commonly referrible, as is familiar to those who are acquainted with the history of those diseases; and if the same substance can also produce continued fever we should expect that wheresoever the latter prevailed in connexion with an apparent cause of this kind, we should have a certain proportion of the periodic fevers likewise, which are unquestionably of malarial origin, and it is probable that the continued and periodic fevers, if traceable to the same source, would correspond in some degree in their frequency. In the Reports on the sanatory state of the poor in the metropolis, there is notice made of the intermittent as well as of the continued fever to which certain districts are liable; and it appears that there is no relation between the two of that kind which we should expect to exist if they owned a common origin. Thus, intermittent fever is most abundant in Poplar, where the Report shows it to have existed in the proportion of one in every seventy-five of the paupers, while continued fever bore the proportion of one in fifteen; and in Kensington, the intermittents amounted to one in seventy-one of the paupers, continued fever to one in twelve. In St. George in the East continued fever was as high as one in eleven of the paupers while there were no intermittents; in St. George the Martyr's there were only three cases of intermittent fever to 1247 of continued; and in Lambeth, the proportion of intermittent fevers was one in 526, that of continued fever having been one in five of the pauper population. The conclusion to which these facts lead is obvious.

The same conclusion may be attained by referring to histories of the

epidemic prevalence of periodic fevers. Dr. Hamilton, in his account of the fevers epidemic at Lynn Regis towards the close of the last century, alludes to several cases of continued fever as having occurred and put on the form of "putrid, malignant, fever," but, he observes, "these were all attacked in the beginning with the reigning epidemic, the remitting fever." In the account which Caillard has given of the epidemic which occurred in the vicinity of the Canal de l'Ourcq, near Paris, in 1810, and subsequent years, there are some circumstances narrated which possess no inconsiderable interest in reference to enquiries respecting the operation of malaria variously compounded. "In the route from Paris to Pantin," says he, "exposed on the one side to the miasmatic emanations of the canal, and on the other to the putrid effluvia of the '*voiries*,' the diseases were numerous, almost all serious and obstinate. .... This disastrous effect of the union of putrid effluvia with marsh miasmata was especially evident in one part of this route termed the Petit-Pont hamlet, inhabited by a currier and a gut-spinner, the putrid waters from whose operations are prevented from escaping by the banks of the canal, and exposed before the draining to the emanations of a large marsh. This hamlet was so unhealthy, that of twenty-five or thirty inhabitants, I visited above twenty seriously affected, of whom five died." Other similar examples are described of localities exposed to the combined agency of animal and vegetable miasma, producing the more serious class of the prevalent disorders, as at Bobigny, Noisy, and Pantin, the two former containing ponds in the midst of the houses, which were "the receptacles of the impurities of these houses, and of the cattle connected with them," and the latter both in the neighbourhood of marshes, and exposed to the effluvia of Montfaucon, and of the putrid manure used on the grounds in the vicinity. Yet the fevers which prevailed in those places were always of the intermittent and remittent types. "When the disease was treated by reiterated purgatives and emetics, it became very easily continued, and had ordinarily a fatal termination on the fifteenth or twenty-first day." Before it can be supposed that these last were cases of the continued fever, to which we are so much exposed in this country, it will be necessary to show that the contagious continued fever assumes at any period the character of a remittent, a disease, by the bye, which has entirely disappeared from London, where the continued form has lately been so prevalent. To the mere circumstance of continuance in fever too much importance is ascribed; a single bloodletting in the cold stage not unfrequently converts a tertian into the continued type; yet the fever does not on that account cease to be a marsh fever, nor does it become a contagious typhus fever.

Among those who contend for the malarial origin of fever two opinions may exist in respect to the nature of the malarial "fever-poison:" to wit, that it is different from the matter of contagion, or, that it is identical with it. There are very serious difficulties in the way of both opinions. If the former be adopted, it is necessarily maintained that the malaria can produce a disease, in the course of which a substance is elaborated quite unlike the original, yet capable of producing the same complicated series of effects; a doctrine to which it would be difficult to produce a parallel in the science of medicine. On the other hand, if it be held that the malaria and the principle of contagion are one and the

same in nature and properties, how happens it that in the several instances we have adduced, communities of men have lived, and continue to live, in an atmosphere loaded to the utmost pitch of endurance with the exhalation of putrescent organic substances, without any peculiar liability to fever, while, as is well known, no class of men can for any length of time be exposed to a similarly concentrated contagion, without exhibiting ample evidence of the evil that is among them, in the abundance of its victims? Either there is no "fever-poison" in the malaria of putrefying organic remains, or it is unlike the substance of contagion. The one will produce disease in twenty-two out of every twenty-three exposed to it in no uncommon way, according to the observations of Haygarth, while the other, existing in unusual abundance and intensity, displays no trophies of its asserted power.

We would call particular notice to the instances adduced by Bancroft, Chisholm, and Duchâtelet, of the absence of contagious fever, in circumstances eminently calculated to produce it if putrid effluvia possess such a power, because we perceive, not only from the Report on the sanatory state of the poor in London, but from the recent Parliamentary Report on the Health of Towns, which we have had an opportunity of but hastily glancing at, that there is a tendency among those who have given evidence on the state of the public health, especially in respect to fever, to regard too exclusively the local nuisances in the neighbourhood of the poor, to the neglect of their condition in respect to food, clothing, and employment. Doubtless, allusions are occasionally made to these circumstances, but in a way, generally, which conveys the impression that they have attracted but little notice, and that a merely secondary importance is, at the most, to be ascribed to them in contributing to the prevalence of fever. The proofs which exist of the incapacity of malaria under circumstances of peculiar aggravation to generate fever, as detailed by the authors to whom we have referred, are to be refuted neither by a heedless neglect of them nor by volumes of mere reiteration of a contrary opinion.

We are far from being certain that the best plan has been adopted for gathering comprehensive and accurate statements in regard to the origin and prevalence of fever, by applying to a multitude of individuals for the results of their observations, and the opinions they have founded on them. Few are well fitted for enquiries of this nature, and fewer still, among practitioners of medicine at least, have that undisturbed leisure from professional occupations, so essential for following out a system of deliberate and persevering research, which alone is adapted to yield the materials proper for the elucidating subjects so complex in their relations, and demanding so much careful analysis. The particular merit of Dr. Alison's pamphlet, on the Management of the Poor in Scotland, where reference is made to the causes of fever, consists in the just and philosophic consideration which is extended to the circumstances in which the question is involved, not merely as they have presented themselves in the course of his individual experience, but as they are known to exist in the history of the country and of science. An experiment, so to speak, thus conducted on a grand scale, is of immeasurably greater value, and more worthy of confidence than a thousand little operations, which, owing to their very smallness, are liable to uncertainty and error from many in-

cidental causes. The particular nature and object of Dr. Alison's work do not admit of his entering into lengthened details of the origin and propagation of fever; but we feel assured that he has taken the right method of determining those momentous questions, by instituting a comparison of the different circumstances of the poor in those parts of the country which are the least affected with fever, and in those where fever is the greatest scourge.

Destitution, the want of proper nourishment and clothing more especially, has been often asserted to be the grand basis of epidemic typhus fever; and, therefore, this fundamental doctrine of Dr. Alison's views, in respect to the prevalence of that malady, is neither new nor uncommon. But there is much of the merit of originality, and of the raciness of novelty, in the manner with which the doctrine is handled by Dr. Alison, and in the range of his illustrative argument. In so far as fever is the subject of the work, the proposition which Dr. Alison endeavours to demonstrate is the following: "The existence of epidemic fever in any great community, particularly if there be neither war nor famine to explain it, becomes a most important test to the legislator of the destitute condition of the poor, and, as I shall endeavour to show, of the deficiency of the funds which, in a better regulated state, are applied to their support." (p. 18.) From this passage it may be gathered that Dr. Alison is no advocate of the influence of malaria in generating fever. His views on this subject are more fully declared in the following passage:

"In the appendix to the Fourth Report of the Poor-law Commissioners, it is stated by Drs. Arnot, Kay, and Southwood Smith, that the malaria arising from putrefying animal and vegetable matters produces typhoid fevers. Although I highly respect all these gentlemen, and approve of the practical inference which they draw from that opinion, so far as it goes, because I have no doubt that vitiated air, like all other causes which weaken the constitution, favours the diffusion of fever, yet I cannot subscribe to their opinion, that this cause is of itself adequate to the production of contagious fever. And if, trusting to that opinion, the public authorities should think it sufficient, in any situation where contagious fever is prevalent, to remove all *dead* animal and vegetable matter, without attempting to improve the condition of the *living* inhabitants, I am confident that their labour will be in vain. The true specific cause of the contagious fever, at least in Edinburgh, certainly does not spring from anything external to the living human body." (p. 11.)

Dr. Alison refers to a paper in the Edin. Med. and Surg. Journal for 1828, for the more extended statement of his opinions, and the evidences in support of them. In that article, and in his present work, the author confines himself to the denial of the malarial origin of fever and to the illustration of its propagation by contagion. He recognizes elsewhere in the facts and observations collected by Chisholm and Bancroft, evidences that the effluvia of putrefying organic substances are not the sources of contagious fever; and in the mode in which the disease spreads through a community he perceives chiefly the operation of contagion. Prominently as destitution stands in the work of Dr. Alison among the causes of fever, he gives it, almost exclusively, the secondary rank of a merely predisposing cause, as in the following passage:

"It is not asserted that destitution is a cause adequate to the *production* of fever (although in some circumstances I believe it may become such); nor that it is the sole cause of its extension. What we are sure of is, that it is a cause

of the rapid diffusion of contagious fever, and one of such peculiar power and efficacy, that its existence may always be presumed, when we see fever prevailing in a large community to an unusual extent." (p. 10.)

The following extracts from Dr. Grattan's account of fever in Ireland, in 1818, are adduced in illustration of the circumstances in which destitution may generate fever:

"'Next to contagion, I consider a *distressed state* of the general population of any particular district the most common and most extensive source of typhoid fever; whether this has been the result of war, or been produced by the more gradual progress of domestic misfortune.........The present epidemic is principally to be referred to the miserable condition of the poorer classes in this kingdom; and so long as their state shall continue unimproved, so long will fever prevail, probably not to its present extent, but certainly to an extent sufficient to render it at all times a national affliction.........In crowded cities, especially when much poverty prevails, the inhabitants, listless and desponding, become inattentive to cleanliness in their persons and habitations, their contracted means compel numbers to reside in the same dwelling, and their apartments are in general filthy and ill ventilated. In winter, in consequence of the want of fuel and of sufficient clothing, any aperture is closed through which the air might procure admission. In these circumstances the contagion of fever is often developed, and then almost every individual within the sphere of its operation, and predisposed by the debilitating effects of mental anxiety, is attacked by the disease.'" (p. 11.)

This is very nearly the same opinion of the origin of fever as that which has been expressed by Dr. Ferriar, in his Medical Histories, who specifies the following as the principal circumstances which produce the peculiar poison of fever: "1st. Want of fresh air. 2d. Deficient or improper diet. 3d. Want of cleanliness, and chiefly want of proper removal and change of clothes. 4th. Anxiety and depression of spirits." (vol. i. p. 242.) Under these circumstances it is presumed that the poison of fever is generated within the living body.

Doubtless there must be great difficulty in affording anything like demonstrative proof of the actual origin of fever from these causes alone. Contagion is a principle which is naturally suspected to lurk in the localities which have been once affected with fever; and as yet so little is known of its qualities and of its power of maintaining its existence, that every difficulty in the origin of fever is by some as readily resolved by a reference to contagion as by others to malaria. That this opinion is correct in the vast majority of instances it were folly to deny; but that it is correct in every instance it were, we apprehend, nearly as unwise to maintain. Smallpox is commonly made the subject of comparison in questions of contagion. It is concluded to be incapable of being generated anew; and contagious diseases which correspond in the essential features of their history and prevalence with smallpox, may, perhaps justly, be considered as alike in that respect. There is no such correspondence, however, between fever and smallpox. Wherever disaster and privation exist for any considerable time, affecting large communities or bodies of men, fever is a too certain attendant; and hence, times of scarcity and of commercial distress, and reverses of fortune in military affairs, have been esteemed, from ample experience, the periods peculiarly favorable to epidemics of this kind.

We think that there is much evidence in Dr. Alison's work in support

of the conclusion to which he has arrived, that the prevalence of contagious fever is "an indication and test of much previous privation and suffering" among the poor.

"Having been," says Dr. Alison, "for many years past a witness of great and, I am sorry to add, of increasing sufferings among the poor of this city (Edinburgh), having satisfied myself that those sufferings here and in Glasgow and other large towns in Scotland are much greater and more general than in towns of equal size in the best regulated parts of Europe, and being thoroughly convinced that the opinions generally entertained in Scotland as to the best means of relieving them are very erroneous, I feel it to be a duty to lay the result of my observations and enquiries before the public. That I do not speak unadvisedly when I use the word *increasing* will appear from the following facts, which, being taken from the records of public institutions, are not liable to the fallacies which might be suspected in the statements of any individual:

"1. The expenditure of the Society for the Relief of the Destitute Sick, the members of which are uniformly men of experience in regard to the habits of the poor, and never grant relief without personal inspection in their own houses of families suffering at once under disease and destitution, has increased from £736, the average of the years 1814-15-16, to £1816, the average of the years 1836-7-8; and the number of individuals receiving relief from them has increased from 3223, the average number in the three former years, to 10,570, the average of the three latter years." (p. 1.)

The increasing number of applicants for accommodation in the Royal Infirmary, and the necessity which was felt a few years ago to institute a house of refuge, by which temporary relief is annually afforded to above 1600 persons, are additional testimonies of the increasing evils referred to.

The causes of the deteriorated condition of the lower classes in the Scottish metropolis are stated to be the diminished proportional expenditure of the higher ranks; the smaller number of wealthy persons who reside permanently in Edinburgh; the reduction in the establishments of the courts of law, and of the different public boards; the failure of building speculations, and the depreciation of property; all of which have necessarily deprived a large number of persons of employment, or rendered their employment irregular and precarious, while

"It is also to be remembered, that this and all other assessed districts in Scotland present a point of attraction to the poor of the numerous districts in Scotland (517 parishes) where there is no assessment for the poor, and often no hospital or other medical charity. The medical charities here are much burdened with cases, often incurable, from those districts; and as long as the relief given in those parts of the country is so small, while the law apportions some allowance (scanty although it be) to all infirm and destitute persons who have lived three years in Edinburgh, I apprehend it will act as a continued bounty on the importation of distressed and half employed families from those districts. Above all, this law has long acted as a bounty on the importation of such families from Ireland; which has gone to such an extremity as fully to justify the observation, 'that if we are to cut off the sources of mendicity we must cut off Ireland.' It need hardly be added, after what has now been stated, that the condition of great numbers of the poor in Edinburgh, particularly during the winter, is one of extreme destitution; approaching in many respects very closely to that which has long been the subject of astonishment and compassion to those who have visited the worst parts of Dublin and other Irish towns." (p. 4.)

To this are annexed extracts from several works descriptive of the unparalleled wretchedness of the lower Irish in their native country, where fever is notoriously in wide-spread and ceaseless operation.

"That there has been a still more rapid increase of destitution, and that similar 'scenes of wholesale human degradation and misery' exist to a still greater extent in Glasgow, is sufficiently shown, *First*. By a few of the facts recorded by the committee appointed there last year to enquire into the cause of the increased assessment: 'The aid to casual poor exhibits more than a threefold increase between 1829 and 1837,' and 'the expenditure for coffins for paupers and their children is nearly *four* times as great now as 1829-30.' *Secondly*. The same is shown by some sentences recording observations made by one of the assistant-commissioners on the handloom enquiry: 'The wynds in Glasgow comprise a fluctuating population of from 15,000 to 30,000 persons. This quarter consists of a labyrinth of lanes, out of which numberless entrances lead into small square courts, each with a dunghill reeking in the centre. Revolting as was the outward appearance of these places, I was little prepared for the filth and destitution within. In some of these lodging-rooms (visited at night) we found a whole lair of human beings littered along the floor, sometimes fifteen and twenty, some clothed and some naked; men, women, and children huddled promiscuously together. Their bed consisted of a layer of musty straw intermixed with rags." (p. 7.)

In respect to Scotland generally, the following account, quoted by Dr. Alison, is stated to be a true picture:

"The female field-labourers, (very numerous here as in every town in Scotland,) when employed, earn only eightpence a day, and are unable to provide anything for the future. Accordingly ceasing to be fit for work about the age of fifty, they inevitably become destitute, and depend for the remainder of their lives on the charity of their neighbours or parochial allowance. The number of such poor women in almost every town in Scotland is distressing to think upon. Though unfit for active exertions, they have a tenacity of life which usually carries them through many years of extreme penury. Habitual piety gives them resignation, sometimes even cheerfulness; but this ought not to blind any enlightened or humane enquirer to the real nature of their situation. The fact is, they live in a condition to which that of most domestic animals is a luxury. The parish rarely offers to such persons more than a shilling a week. Individuals occasionally give them some scraps, but this succour is very trifling. Their mode of life is often altogether a mystery, nothing like the usually understood means of maintaining life being found within their reach." (p. 19.)

Against the lowest depths of destitution there is no security afforded to the wretched poor of Scotland, as there is in the legal provision guaranteed to those of England. It might amuse our English readers (if such a theme were fit for amusement,) to learn the reasons which induce our northern neighbours to keep the parochial relief for the destitute at the lowest rate consistent with the appearance of relief. We have somewhere read an eulogy on the system which gave ample scope for suffering on the part of the poor, on the ground of its conducing to cultivate the feelings and perfect the characters of the rich, by exciting and sustaining their sympathies; and, monstrous as this may appear, it is very nearly what is commonly urged by those who deprecate the introduction of a more effectual system of assessment for the benefit of the destitute. What voluntary contributions have done for the Scottish poor seems to be miserably little, at least in *protecting* them from suffering; and it appears to us a strong argument, independently of its invariable insufficiency, against leaving those who are ever on the verge of destitution, to the tender mercies of a voluntary system, that before the latter can be roused into active operation in the event of sudden reverses

in trade or in agriculture, a fearful extent of misery and want has to be endured by the thousands whose heritage is poverty.

"The proportion," says Dr. Alison, "in which the legal relief given in England exceeds that given in Scotland varies considerably. The whole population of 662 unions and parishes in England, to which the new law has been applied, is, in round numbers, 11,166,000, and the sums applied in them to relief of the poor under the new law in 1838 were £4,254,000. The population of Scotland is stated by the committee of the General Assembly at 2,315,000, and the whole sums applied to the relief of the poor, about one half of which are raised by assessment, are £140,496. The whole funds thus applied are nearly six times as great, in proportion to the population, in England as in Scotland. The average expenditure per head on the population in England is stated in the last report of the commissioners to be now reduced to 5s. 10d., and in Wales to be 6s. In Scotland it is less than 1s. 4d." (p. 31.)

In Edinburgh the amount of the legal provision for the poor is 2s. 6d. a head on the population; in Glasgow, Dundee, Paisley, and most of the other Scottish towns it is less than 2s. a head on the population,—less than a third of the expenditure in England. The highest provision granted to a pauper in Edinburgh or in Glasgow (even to a widow with a family) is less than 2s. a week, and the pension to a single male or female pauper is about 1s. a week; whereas in England a widow with four children has from 4s. to 7s. a week, and an aged or disabled man or woman has from 2s. to 4s. Besides, in all Scotland there are but four workhouses, while in England, in May, 1839, there were 587. Scotland appears to equal disadvantage when compared with Hamburg, Holland, many parts of France, and other continental states, in respect to the legal provision for the poor. That this great defect is supplied by the abundance of voluntary contributions is utterly disproved by the amount of misery and want existing in the larger Scottish towns more especially. Dr. Alison also attests the extreme insufficiency of the support given by the public of Edinburgh to those charitable institutions which aim at relieving the prevalent wretchedness. There appears to be no want of support to those institutions in which sickness is to be relieved, but "the funds of several of these institutions, viz. the Infirmary and Dispensaries, are in a great measure supplied by the students of medicine who come from a distance." The funds of other charities, intended for the relief of indigence alone, appear to be miserably defective, and on the decline.

In consequence, as Dr. Alison maintains, of the absence of adequate relief for the poor, disease, and more especially fever, has been on the increase.

"For many years past contagious fever has never been absent from Edinburgh, and there have been three great epidemics of that disease in the last twenty-two years, beginning in 1817, 1826, and 1836, (the last of which has now nearly subsided,) each lasting nearly three years, and each of the last two affecting, I believe, nearly ten thousand persons. The number of fever patients admitted into the Infirmary and Auxiliary Fever Hospital from November 1817 to November 1820, was 3090; from November 1826 to November 1829, it was 4318; and from October 1836 to October 1839, it was 4850." (p. 8.)

It is conceived that not more than half the cases which occurred during these epidemics were removed to the hospitals, and therefore that the whole number affected between 1836 and the close of 1839 must

have been at least 10,000. This calculation we believe to be considerably below the actual amount. In Glasgow, it appears from the table contained at p. 10 of Cowan's Vital Statistics of Glasgow, that between 1827 and 1836 not more than one third of the whole cases of fever were received into hospitals; and though the difference of accommodation is in favour of Edinburgh, we doubt if it be so great as the calculation of Dr. Alison, compared with the latter statement, would lead us to infer. That the prodigious amount of fever in the several periods noticed by Dr. Alison was new to Edinburgh is very certain, as appears from a letter from Dr. Gregory to Dr. Clark of Newcastle, dated May 2, 1802; where it is stated that accommodation for fever patients had been opened in the Edinburgh Infirmary twenty-five years previously, and that "the ward for men contains eighteen beds, and that for the women contains fourteen beds, without any crowding; *and have very seldom been full.*"*

"That it is always in persons suffering," says Dr. Alison, after referring to the sufferings of the Irish in 1818, "or who have lately suffered, similar privations and sufferings, and the mental depression and despondency which naturally attend them, that continued fever becomes extensively prevalent, is fully established by the history of all considerable epidemics. The elaborate work of Drs. Cheyne and Barker, shows that this has been strictly true of all the great epidemics which have appeared in Ireland since 1700, each of them lasting fully two years, viz. in 1708, 1720, and 1731, in 1740-41 (after the great frost of 1740), in 1800-1, after the rebellion, the transference of the seat of government to London, and the scarcity of 1799 and 1800; and again, in 1817, after the 'transition from the state of war to that of peace,' and the scarcity of 1816 and 1817. That work contains reports from the most eminent physicians in all parts of Ireland on that great epidemic, all agreeing in the statement, that 'the poor were the greatest sufferers, and the fever seemed to rage among them in a degree proportionate to the privations they had endured.' In Ireland, accordingly, at least during the present century, as the general condition of the poor has been decidedly worse than either in England or Scotland, so contagious fever has never ceased to be more generally prevalent. The same observation applies to the epidemic fever in London after the scarcity of 1800 (the last great epidemic which has occurred there)—to the great continental fever of 1813-14, which followed the track of the French army retreating from Russia, but never made much progress in the victorious allied army—to the epidemic fever of 1817, in Italy, consequent on the scarce year 1816—to the epidemic which affected the British army in Holland after the disastrous retreat from Flanders in 1794—in Portugal after that from Burgos in 1812—and to that which nearly decimated the British legion at Vittoria in 1836." (p. 12.)

If the history of the "petechial typhus," which is a common continental name for the contagious fever in question, were fully detailed it would be found that few wars have ever occurred in Europe without this calamitous attendant. The wars of Maximilian II. in Hungary,—of Francis I. in Italy—of Charles V. and of Charles XII.—of Louis XIV. down to those of our own times, amply justify the title of *war-pest* (kriegspest), as applied to typhus by the Germans. It has not always, indeed, afflicted exclusively armies dejected by defeat, and worn out by the sufferings of a retreat or of a siege; but it must be remembered that these disasters were not necessary in former times to create the most aggravated miseries among the troops. Let any one consult Sir John

* A Collection of Papers, &c., p. 58.—*Newcastle*, 1802.

Smythe's Discourses, and he will find that the very miseries and privations which reign among the modern victims of fever were the common lot of the soldiery in the fifteenth and sixteenth centuries. With a correction of the orthography, the following is an extract to the purpose. "But such covetous men of war, under the pretence (as though their soldiers had been either natural fools or children), did, contrary to all military order, put the greatest part of their soldiers' pay into their own purses, allowing them great scarcity of provisions. By which means it came to pass that divers thousands of their soldiers, partly by hunger and partly by evil lodging, and altogether by the small care and misuse of our such men of war, did perish." (See Chisholm on Contagion.) The French revolution while it drew the horrors of war on Italy and Germany, inundated these countries with the war-pest; the former country being the seat of its ravages in the earlier years of the Republic; and the latter subsequently, more especially in the great epidemics of 1803, in the Austro-Russian campaign of 1806-7, in the Prussian war of 1813, during the deplorable retreat of the French from Russia. It is not only from their history in times of war that other countries afford testimony to the truth of Dr. Alison's views. We have already referred to the fevers of Italy in 1764 and 1817, which occurred in circumstances of great and general privation. Many other epidemics might be mentioned in connexion with the like cause, but we restrict ourselves to the following notice of the events which led to an epidemic at Louviers, in Normandy, in 1770, as detailed by Lepecq de la Cloture. "Let us remember," he says, "that for a number of years the price of provisions had become extremely high; and that suffering having sensibly increased, the commune of Louviers was considerably enfeebled, which obliged the manufacturers to lessen a part of their work; consequently a number of operatives remained without employment, and the wages of others were diminished. What happened then? One did not see, as formerly, the workmen enjoying themselves on fête-days, and drowning a part of their care in wine. No, there was for them no more movements of joy. Sadness was painted in pallid colours on every face. Misery had prostrated their spirit. In short they were reduced for the most part to deprive themselves of some of their wretched furniture, to sell them that they might appease their hunger, and to furnish some morsels of bread to the urgent demands of their children." Four thousand of these workmen were employed in woollen manufactures. Their reverses and sufferings appear to have been identical with those of a similar class among ourselves.

"That the same cause," continues Dr. Alison, "has acted very powerfully in producing the recent epidemics in Scotland appears distinctly from the following considerations. First, it appears from observing the times of these epidemics, the first in Edinburgh beginning in 1817, after two bad harvests, and at the same time as the Irish one; the next in 1826, after the great failures in 1825, and the sudden cessation, particularly of building speculations, in Edinburgh; and the last in 1836, after the great depression of trade both in Glasgow and Dundee, with which towns the lower orders here are much connected, and under the combination of other circumstances already mentioned, which have depressed the condition of the poor in Edinburgh of late years." (p. 13.)

In Glasgow the increase of fever within the same period has been still more remarkable. This is fully illustrated by the first Table in Dr.

Cowan's valuable tract on the Vital Statistics of Glasgow, in which we have the "total number of patients treated in the Glasgow Royal Infirmary from 1795 to 1836, distinguishing the number of fever patients each year." During the first seven years, 484 cases of fever occurred; in the second period of seven years, 512 cases; in the third, 574; in the next, from 1816 inclusive, 3866; from 1823 to 1829 inclusive, 6075; in the last seven, from 1830 to 1836, 11,750. These numbers are not to be considered as representing nearly all the cases of fever which occurred in Glasgow, especially during the latter periods; for between two other hospitals, appropriated to fever alone, 3074 cases were treated during two of the epidemics; and besides there were treated at their own houses between the years 1827 and 1836 inclusive, not less than 6202 cases, by the district surgeons. In the mortality bill for 1839, we find that the total deaths from fever in Glasgow for the five years preceding 31st December, 1839, amounted to 4788, which, at the average mortality of one in every ten cases (which is rather above the actual rate), gives for that period 47,880 cases of fever. In 1837 alone, above 20,000 of these cases are supposed to have occurred: not far short of one in twelve of the whole population. That the same public calamities affected the people of Glasgow which have been noticed as conducing to the prevalence of fever in Edinburgh, cannot be doubted. Yet we observe a passage in Dr. Cowan's work which appears utterly at variance with the proposition of Dr. Alison. Alluding to the increase of fever in Glasgow, he says "this increase, especially during the last seven years, has taken place, not in years of famine and distress, but during a period of unexampled prosperity—a period when the wages of labour have been ample, the prices of provisions comparatively low, and every individual, able and willing to work, secure of steady and remunerating employment." (p. 13.) Without some attention and enquiry this account might be apt to mislead. The first great epidemic in Glasgow accompanied a period, as attested by Dr. Cleland, of great stagnation of trade. He informs us that in 1816-7 so great was the distress of the poor that above 23,000 persons sought aid from the charitable funds; and that this condition did not soon subside appears from another statement of his, that, in 1819, 5256 handlooms were unemployed. Prior to 1816 there is no record of vast reverses in the trade of Glasgow, nor of the extensive prevalence of fever: since then great fluctuations in the prosperity of that city have been certainly not uncommon, as is signally exemplified by the failures of 1825 and 1835, after each of which years fever underwent a great increase, which continued more or less for several successive years. Dr. Cowan's observation, which we have quoted, referred chiefly to the seven years which preceded 1837; and if the comforts of the people were greater on the whole than they have been since, in like manner the amount of fever among them was much less.

"From the close of 1836," says Dr. Cowan, "one of those periodical depressions in trade, arising from our monetary system, has visited this city, and deprived a large proportion of the population of the means of subsistence. From the existence of secret combinations among the working classes in various departments of trade, but especially among the cotton-spinners, and the 'strikes' which resulted from these combinations, a very large proportion of the inhabitants, in addition to those already suffering from the state of the money market,

were suddenly deprived of employment and consequently of the means of procuring food. The high price of coal was the means of diminishing the hours of labour, and consequently the amount of wages in numerous factories, and placed fuel beyond the reach of the lower classes for domestic purposes. And in addition to these sources of misery, the average prices of grain were much higher during 1837 than they had been for some years previously." (p. 34.)

Accordingly the amount of fever, which had been still considerable since the previous epidemics, and had never since its sudden and great increase in 1815-6 fallen nearly so low as it had been before that period of depression, greatly increased, so that according to the calculations of Dr. Cowan it would appear to have risen from 6180 cases in 1835 to 10,092 in 1836, and 21,800 cases in 1837. In the absence of a legal method of relief for the multitudes of able-bodied persons thrown out of employment by the several adverse circumstances which have been detailed, soup-kitchens were opened which daily supplied about 18,500 with food for three months, "but notwithstanding all these exertions, famine and pestilence prevailed to a fearful extent, and the rate of mortality (exclusive of the still-born) rose to 1 in 24·63 of the population." (Cowan, p. 34.)

"Again," observes Dr. Alison, "if these principles as to the connexion of epidemic fever with misery and destitution are correct we may expect to find another illustration of them in Dundee, where there has been, first, a great increase of the population, attracted by the rapid extension of manufactures, then a complete stagnation of trade and suspension of manufactures, throwing great numbers out of employment; and lastly, a prevalent belief among the higher ranks, that 'the older and better system of supporting the poor by voluntary contributions,' ought to be preferred to assessments, and, in consequence, a legal provision, even in the worst times, hardly exceeding, as will afterwards appear, the average of the Scottish country parishes. Accordingly we find that prior to the year 1818, 'little demand was made, comparatively speaking, on the Dundee Infirmary for the reception of fever cases, but during that and the following year the disease raged to such an extent in the town and suburbs, that the house became inadequate to the wants of the community. Since that period the progress of the disease has been various, but on the whole *vastly on the increase.*' The last epidemic appears to have attained its maximum there in 1836, when the fever patients in the infirmary were 773, and the deaths from fever, stated in the mortality bills, (which I understand to be kept very accurately,) are 297. But in the years 1836-7-8-9, fever continued very prevalent, and the deaths from it were 880, the whole number of deaths in these years having been 7160, or 1790 in the year, in a population which, in 1831, was 45,000, but is since supposed to have extended, chiefly, however, by accessions from the country, to above 60,000. From these facts we may infer that nearly 10,000 inhabitants of Dundee, or nearly one in six of the population, must have had fever in these four years. The deaths by it were almost exactly one eighth or 12·5 per cent. of the whole (297 in 1962), and exceeded those by consumption by nearly one third, viz. 297 to 200." (p. 14.)

If the connexion just referred to be really of the nature maintained, we should expect to find that large communities shielded from the excess of wretchedness to which the Scottish and Irish cities are subject, should differ from them in their liability to fever. And the connexion in question will be all the more obvious if the communities which are thus less subject to fever and to destitution, are in other respects in much the same condition as those are where fever and destitution are co-extensive. We find this difference in a very marked degree in our English cities;

we find their poor much less exposed to suffering and want; we find them much less subject to fever; but we find them also, as is amply attested by the reports on which we have already commented, and by the recent parliamentary enquiry into the health of the large towns in England, exposed to local nuisances, filth, and effluvia, differing, as far as we can judge from the terms in which these are described, neither in degree nor kind from those to which the poor of the Scottish cities are exposed. In Manchester, with a population of nearly 228,000, it appears that the average annual number of cases of fever treated in the hospital for the seven years ending in 1836 amounted to 497, while in Glasgow, with a population of 244,000, the number treated annually in hospital during the same period amounted to 1842; and that in 1836 the number of cases admitted into the Manchester hospital was 780, and into the Glasgow hospitals 3125. "The prevalence of fever in Glasgow," says Dr. Cowan, "when compared with Manchester, is still more strikingly contrasted by the great change which has taken place in this respect. From 1797 to 1806, both inclusive, the number of the fever patients treated in the Glasgow Infirmary was only 883, while those treated in the Manchester Fever Hospital amounted to 4618." (p. 11.) In Leeds, too, another manufacturing city, with a population at the last census of 123,393, the average annual number of patients treated in hospital during the last seven years, was only 274. In Newcastle and Gateshead, with a population of 57,917, the average number of cases admitted into the institution for the cure and prevention of contagious fever during the like period was only 39 annually. (Cowan, p. 11.) In Carlisle and its vicinity, a population of above 32,000 yielded, on an average of seventeen years before 1838, only 63 cases in the year to the House of Recovery; but in 1838 fever prevailed there epidemically, and affected about 600 persons, of whom 265 were received into the hospital. In Birmingham, where the population is now reckoned at nearly 150,000, the average annual number of cases of fever (including agues) has been for the last seven years only 69. In Sunderland, where the population amounts to nearly 50,000, only seventeen cases were taken into hospital in each year during 1836-7-8. In English towns where extensive manufactories do not exist, the proportion of fever cases is still smaller. In Oxford, for example, a population of 16,000 does not yield to the infirmary five cases of fever in the year.

In London fever has lately prevailed to an unusual extent, especially in 1838; and has affected chiefly those districts which are inhabited by the lowest classes of the population. The total population of these twenty unions is 851,229; which afforded, in 1838, 12,709 cases of fever, or only about one in sixty-seven. If we take the four districts which were the most affected with fever, St. George the Martyr, Bethnal Green, Whitechapel, and Lambeth, we have 6097 cases of continued fever in a population of 253,784, almost exactly that of Glasgow in 1837, where above 20,000 cases of fever occurred in that year of its unusual prevalence. In ordinary years the amount of fever in Glasgow, for nearly ten years past, has been very little short of the number which occurred in these unions during a period signalized as one of an uncommon or epidemic prevalence of fever.

That fever should prevail to a considerable extent in several of the

larger cities of England is not to us a matter of surprise. Even although destitution may not exist to *generate* fever, yet no superiority of food and clothing, we are persuaded, can ever protect a community from the extension of that disease by contagion, while the other parts of the United Kingdom are so constantly and extensively afflicted with the malady; for the migratory habits which necessity has imposed on the lowest classes of the population in those parts cannot fail to be the means of introducing the contagion from time to time among their more fortunate neighbours. The proofs of this are to be found, we conceive, in the notorious fact that the lodging-houses, and the dwellings to which the lower Irish have access, are in our towns the places to which fever is most commonly traced or referred. Local peculiarities, also, may give a facility to the diffusion of fever in England, which may detract somewhat, in appearance, from the arguments and views of Dr. Alison. Liverpool, perhaps, affords the most remarkable example having this tendency. That city, in 1836, with a population of 189,242, afforded to the hospital 1700 cases of fever. Considerable as this number is, it will be observed that it does not amount to more than two thirds of the number in Glasgow in 1836, in proportion to the population, and considerably less than half the proportion in Glasgow in 1837. The number which Liverpool would have yielded to the hospital, had its population been equal to that of Glasgow, would be 2272; the number of cases admitted into the Glasgow hospital in 1836 was 3125. The number of fever patients in Liverpool in that year was undoubtedly very great, and from what we have learnt of the comparative plenty which exists among the lower classes there, could not have depended on the destitution of the inhabitants. But there are two circumstances in the case of Liverpool which have caused it at all times to be more affected with fever than any other English town. Forty years ago, when its population did not exceed 63,000, Dr. Currie informs us that the average annual number of cases of fever in Liverpool was about 3000; and that, too, notwithstanding poor-rates that amounted, in 1801, to £28,000. At that time 7000 persons lived in cellars, and 9000 in back houses with very imperfect ventilation. At present it appears from the Parliamentary Report on the Health of Towns, that nearly 40,000 persons reside in the underground cellars, and 86,400 in courts so constructed that ventilation is extremely imperfect; or, as stated in the Report, as if "contrived for the purpose of preventing ventilation." The facilities which are thus afforded for the diffusion of a contagious disease are sufficiently obvious, and are illustrated very emphatically by the late prevalence of smallpox, as well as of fever, in Liverpool. Dr. Duncan, in his evidence, says, "I have facts pointing out the noxious effects of the cellars on the inhabitants with regard to fevers. The proportion of cases of fever occurring among the inhabitants of cellars is about thirty-five per cent. more than it ought to be, calculating the proportion of the inhabitants of the cellars to the whole working population; that shows that the inhabitants of the cellars are much more liable to be affected with fever than the inhabitants of other dwellings." (p. 144.) The other circumstance which renders Liverpool subject to fever is its peculiar exposure to the introduction of contagion by those who frequent the seaport, and by the multitudes of Irish who resort to the city. The fever prevailed to a considerable extent

among the seamen of the port; and the number of the Irish in Liverpool are estimated at 60,000. With all these disadvantages, however, fever has not been increasing in Liverpool, as in the Scottish cities. In the beginning of the present century the cases of fever, as we have stated, were annually 3000; in 1840 it is stated that "the average annual number attended during the last five years was upwards of 5000," besides some which, for particular reasons, are not included in this calculation. It would appear, therefore, that while the population of the city has increased from 63,000 to 250,000, the population of the parliamentary borough, that of the parish alone being 190,000, the amount of fever has been only doubled. A material decrease, therefore, has taken place in Liverpool, within the last forty years, in the proportion of fever patients, otherwise the number at present should have amounted to nearly 12,000 annually. This decrease of fever in Liverpool is doubtless to be ascribed both to the increased prosperity of the town and to the check which has been imposed on contagion since 1802, by the establishment of a fever hospital. For in Glasgow, Edinburgh, and Dundee, notwithstanding ample hospital accommodation, contagious fever has been on the increase out of all proportion to the increase of the population. Without such accommodation, we apprehend that the recent history of pestilence in those cities would have rivalled that of the worst European epidemics in the worst times.

We have already referred to the decrease of fever in Manchester since the early part of this century; and it appears from the notice taken of the subject by Dr. Haygarth and Dr. Ferriar, that this decrease is to be ascribed mainly to the influence of the House of Recovery, erected in 1796. That another circumstance should also be taken into account as illustrating the cause of the decline of fever in Manchester, will appear from what Dr. Ferriar says when referring to the causes of the epidemic which prevailed there in 1794. Among these causes were "the want of clothing, and failure even of necessary food, in many families, occasioned by the decay of trade, and the great number of workmen enlisted in the army, who left their children to the slender support which could be earned by the labour of the mother. In many instances I have found that for three or four days before the appearance of typhus in a family, consisting of several children, they had subsisted on little more than cold water. Many of these persons were strangers, and not entitled to, or unable to obtain, the pittance afforded by the poor laws. Even when that relief could be procured, it was very inadequate to the wants of a numerous family. Those who are accustomed to affluence and ease would shudder at the idea of supporting a sickly mother encumbered with the charge of four or five infants on an income of two shillings a week; this, however, is the parochial relief in cases of illness." (Med. Hist., tom. ii.) The diminished proportion of fever patients in Manchester is very strikingly illustrated by the facts, that in the first year of the existence of a fever hospital there were 623 fever patients admitted, and that forty years later, while the population has been increasing so rapidly as to have augmented above 36 per cent. between 1821 and 1831; the number received into hospital in 1836 amounted only to 780.

The decreased mortality in London from fever is very remarkable, as exhibited in the bills of mortality for the last hundred years and upwards.

In 1750 the deaths from fever in London were almost one fifth of the whole mortality; whereas lately, in an epidemic period too, they have not exceeded one twelfth. The annual average of the deaths from fever in London, for the whole of the last century, is above 3000 (including those from scarlet fever), from which we may presume the annual average of fever patients to have been about 30,000; while for the year ending March, 1838, a period of unusual prevalence of fever, we find the worst districts of the city, containing a population of above 850,000, yield only 13,972 cases of synochus, typhus, scarlatina, and intermittent fever: the remainder of the population doubtless yielded a much smaller proportion of cases. In making the comparison between this last period and the past century the difference of population is to be kept in mind. The total number of deaths from fever in 1838, as shown by the returns under the new registration act, was 4078, and from scarlatina 1524, at which time the population may be reckoned at 1,888,800: the mortality from fever was consequently 2·32 in 1000. The mortality from fever has slowly declined since 1838.*

That the improvement in the health of London is in part owing to the introduction of means of separating the sick cannot be doubted; but we must also in part ascribe it to the improved condition of the poor—to the better administration of parochial relief. The complaints on this head made by Willan in 1799, and by Lettsom more recently, leave no room for doubt respecting the favorable change which has since occurred.

The exemption of the large cities in the rest of Europe from epidemics of fever that can bear any comparison with those of Scotland and Ireland, is sufficiently well known; and, according to Dr. Alison, is attested by the great number of foreign medical men who resort to those kingdoms to study the disease with facilities which their own countries do not afford. If poverty can give rise to fever it might be presumed, by those who are ignorant of their condition and circumstances, that the labourers employed about Montfaucon should be subject to it in no ordinary degree, for it may very naturally be supposed that the occupations of that neighbourhood can hold out no attractions except to the destitute. But fever, as we have seen, is not a common disease there, and it is not a little important to know that destitution, contrary to what might have been reasonably conjectured, is not heard of among its population. According to Duchâtelet the people are always employed, to which circumstance he justly ascribes much of their healthiness; and besides, in matter of food, they have been accustomed to a liberal supply of an animal

* The following statement is from Mr. Farr's elaborate article on Vital Statistics, in Macculloch's work, (vol. ii., p. 579,) and may be fully relied on from the well-known accuracy of that able statistician.

MORTALITY IN LONDON.
Deaths to 100,000 living.

	1629-35	1660-79	1728-57	1771-80	1801-10	1831-5
Fever	636	785	785	621	264	111
Spotted Fever . .	45	90	...	...	...	...
Plague	125	1225	...	...	...	...
Scarlatina . . .	...	...	...	...	...	53
	806	2000	785	621	264	164

diet—horseflesh—not indeed the most inviting, but apparently wholesome enough.

That Dr. Alison's work may not fail to lessen the evils under which the lower classes of his native country suffer so much must be the wish of every lover of his fellow-men. That the details of misery and suffering which it adduces demand the early and anxious scrutiny of the legislature, appears to be undeniable. We heartily thank him for the zeal with which he has thrown himself into the cause of the poor, and fervently trust that a state of things so deplorable as that which he describes may soon cease to be the reproach of a Christian country. There may be differences of opinion about the way in which so desirable an event may be best achieved, but the necessity of some measure being adopted for the relief and protection of the destitute Scottish poor is placed beyond all doubt. Who cannot sympathize with the feeling of the following eloquent and touching passage?

"If it is the dream of an enthusiast to hope that the law may yet extend a similar protection to such sufferers with us as it does in various other countries, I can only say that it is a dream which will last my lifetime; and that I should wish, by what I have now written, to bequeath it to those who may come after me in the same path of duty, to cheer the hearts of others, as it has done mine, in many a solitary wandering through the abodes of poverty and suffering."

Note by the Editor.

It cannot be denied that the cause of fever is involved in much obscurity, and that we still want data on which to rest deductions which will carry with them *unhesitating* conviction. In the actual state of our knowledge it can only be said that the largest number of facts and the best arguments are on one side; but still there are facts and circumstances difficult to be explained on either hypothesis. It may, therefore, be asked, why, entertaining such views, we give a place in our pages to the foregoing article, or at least that part of it relating to malaria, —able, acute, and forcibly argued as all must admit it to be; because, taking as it does the view that fever is not caused by malaria, it might seem to have a tendency to damp the zeal of those who are labouring to improve our towns, by giving the impression that by making sewers, removing filth, opening thoroughfares through densely-populated neighbourhoods, and inducing habits of cleanliness, they were not thereby removing the causes of disease. If this were necessarily the tendency of the arguments of the contagionists, it would not only, in the present state of our information, be culpable to support them, but on this very account should their validity be suspected; but in a practical point of view both parties are in accordance. The one says, that by cleanliness and ventilation you remove at once the cause of the disease: the other, that you remove the causes which propagate the disease when once it is in existence; for that whatever tends to concentrate the poison, or to weaken in any degree the vital powers, renders the victims more numerous. The latter view of the subject is, therefore, if anything, perhaps the one which is more persuasive to an extended benevolence. Those who regard fever as dependent on malaria, would be more likely to pursue but one object; the cleansing of the poisonous atmosphere of filthy, crowded rooms and streets: the contagionist would go farther and say this is not enough, for unless you relieve that debilitated state of body and mind which arises from meager poverty, you leave unremedied one of the most powerful causes of the propagation of the malady.

ART. II.

Mémoires sur l'Empoisonnement. Par M. ORFILA. Mémoires de l'Académie Royale de Médecine. Tome viii.—*Paris*, 1840. 4to.

Memoirs on Poisoning. By M. ORFILA. From the Memoirs of the Royal Academy of Medicine. Vol. viii.—*Paris*, 1840. 4to.

THE Memoirs of M. Orfila present so much of what is new and interesting in relation to toxicology, that we have thought it our duty to extract the article from the Memoirs of the Academy, and treat it as a separate work. These Essays on Poisoning occupy about 200 pages, quarto, of the volume, and are divided into five chapters on Arsenic, one on Tartarized Antimony, and one on Copper. The main object of the author is to make public certain new processes which he has discovered for the detection of these poisons, as they exist in the bodies of individuals who have died under their operation.

The chapters on Arsenic are undoubtedly the most important; they abound with observations susceptible of the most extensive application for good or for evil in the practice of medical jurisprudence; they require cool and deliberate examination, and, above all, it is necessary for a reader to guard himself against being misled by the enthusiasm of the author. Already a case of remarkable interest has occurred in France, in which a practical application of M. Orfila's new views has been made; we allude to the recent trial of Madame Laffarge for poisoning her husband with arsenic. In this case, the chemical processes commonly employed and recommended for the detection of arsenic entirely failed, but M. Orfila satisfied himself that arsenic was present in various organs of the body of the deceased, although it had not been detected in the stomach. His evidence seems to have considerably influenced the jury, and to have led them to pronounce a verdict of guilty. We think we have said enough to show that these alleged discoveries cannot, if well founded, be too widely circulated; and it must rest with the profession to determine whether they are worthy of that confidence, in a judicial light, which the author so earnestly claims for them.

The facts upon which M. Orfila chiefly dwells, and which he considers are fully established by his experiments are: 1, the absorption of arsenic into the blood when applied externally or taken internally; 2, the presence of that poison in the heart, brain, lungs, and other organs of individuals who have died from its effects; 3, a new method of separating arsenic from organic matter, and of detecting it by Marsh's apparatus; 4, the presence of arsenic as a *natural* constituent of human bone, and its probable presence as such in the muscular system; 5, its non-existence in the soft organs, except in cases where the individual has been the subject of poison; 6, its presence in the soil of cemeteries, rich in the detritus of human bone.

It had been long suspected by toxicologists that arsenic entered into the blood by absorption; but this suspicion was founded on physiological inference, and not on any ascertained facts. In nearly all modern works on toxicology, the reasons why arsenic could not be detected in that liquid are assigned, and the search for it was regarded as hopeless. These reasons were chiefly, that the poison itself existed in but small quan-

tity, and that but a very small portion of blood could be operated on at once. M. Orfila has overcome both these difficulties, and a new argument in favour of the action of poisons by absorption has been thereby added to those which previously existed. In the first memoir there are given the details of thirty-two experiments, chiefly performed on animals, for the purpose of showing that arsenic actually enters into the blood. In some of these the poison was inclosed in small bags, which were placed in wounds made in various parts of the body; in others the poison was introduced into the stomach, the œsophagus being afterwards tied, and the blood of the animal at a certain period was abstracted and examined. The poison was detected by the use of Marsh's apparatus in all cases where a sufficiency of blood was operated on, or a sufficient quantity of the soft parts taken. In one case the blood was removed from the abdominal aorta of the living animal one hour and twenty-five minutes after the poison had been placed within the stomach, and the œsophagus tied; the blood of this animal yielded traces of arsenic: "the brain contained a little, the lungs rather more, the heart and kidneys still more, but the liver and spleen yielded the largest quantity." (Exp. 16, p. 386.)

In the course of these experiments, the author attempted to determine how much of the poison was required to destroy life. Thus, he dried and weighed the small bag containing the poison before he introduced it into the wound, and after the death of the animal he again dried and weighed it. There was a trifling loss of weight. (Exp. 2, p. 378.) As the author does not insist upon the practical application of these results, we shall not criticise the experiments further than to observe that, in this point of view, they are to our minds wholly inconclusive. The necessary admixture of the blood of the wound with the bag and its contents must render any inference, founded on a loss of weight before and after the experiment, fallacious. This alleged loss of weight in the quantity of poison employed seems to have been mainly instrumental in causing him to extend his researches, and to apply his processes for its detection in the soft organs of the body.

His first efforts to detect the poison were not very successful, owing to his having examined too small a quantity of the soft parts. By taking several organs and acting on them by one process, the evidence of the presence of poison became conclusive. Within a very short time after the announcement of Marsh's process for detecting arsenic, and before it could have become known in France, we employed the apparatus in order to detect arsenic in the blood of two animals which had been poisoned by it, but the result was unsatisfactory; no evidence of the presence of arsenic could be obtained. Since the publication of Orfila's experiments we are satisfied that the cause of failure was to be ascribed to two circumstances, both of which were an embarrassment to him in the outset: 1st, that too small a quantity of blood was taken; and 2d, that the minute portion present was probably concealed and protected from the action of the chemical reagents by the organic matter mixed with it. The last fact is interesting; it has hitherto escaped the attention of toxicologists, and we cannot but conclude with the author, "that arsenic may have been present in many cases where medical witnesses have pronounced it to be absent." (p. 422.) Orfila has removed this

difficulty by insisting on the preliminary process of incineration with nitrate of potash, thereby changing the arsenic into a fixed arseniate, or what he considers to be an improvement on his original plan, digesting the animal matter containing arsenic in nitric acid, and thereby oxidizing and destroying it. We shall advert to the details of this process hereafter. One other point requires to be noticed. If, on passing sulphuretted hydrogen gas into a suspected liquid, there should be no production of sesquisulphuret of arsenic, indicated by a change of colour, it would be affirmed by most toxicologists that arsenic was not present. Orfila contends that arsenic may be present in such a case, and that it may also exist in a liquid in which all the sulphuret has been thrown down by the gas. He has proved this by dissolving a quantity of nitrate of potash in the liquid, evaporating to dryness, incinerating and introducing the saline residue, previously washed with sulphuric acid, into the hydrogen apparatus. (p. 396.) It would be useless to enter into further particulars at present, since, in the third memoir, the author substitutes another process for that which he recommends in the first.

A large portion of this memoir is devoted to a description of Marsh's apparatus, and the precautions to be observed in using it. We agree with him, that the plan proposed by the inventor for detecting arsenic in organic liquids is complex and inconvenient of application; not to mention that the detection of the poison under these circumstances must be very uncertain. Thus, Mr. Marsh advises that the whole organic liquid, to the amount of three or four pints or more, should be placed in a vessel and hydrogen generated in the midst of it. Orfila found this method so unsatisfactory that he soon abandoned it. The chief difficulty to the disengagement of arsenuretted hydrogen he found to exist in the large quantity of froth produced by the presence of organic matter. He, in vain, endeavoured to prevent this by the use of a stratum of oil, as recommended by Mr. Marsh. Objections have been repeatedly made to the use of the apparatus on account of the froth thus formed, but we have found that the production of this is easily avoided by pouring a small quantity of pure alcohol on the surface of the liquid. In no part of Orfila's treatise is this simple remedy adverted to, although it is actually mentioned by Mr. Marsh in his original communication to the Society of Arts. Granting that the froth is got rid of, there is, as Orfila contends, an insuperable objection to operating on so large a quantity of organic liquid, in which there may be only traces of arsenic; and it is clear, if but a small quantity be taken, as in the employment of Marsh's bent tube, we may be deceived in our judgment respecting the presence of poison.

The entire destruction of the organic matter, the fixation of the arsenic during this process, and the consequent reduction in the bulk of the materials, have enabled Orfila to detect arsenic in the minutest traces, in a small apparatus, and without the production of any froth.

The apparatus of Orfila consists simply in a bottle, with a bent glass tube drawn to a point. It is undoubtedly susceptible of improvement: there ought to be a stopcock attached, to prevent the escape of gas. If any froth should form, alcohol may be used—not oil, since this is not merely inconvenient, but it is apt to absorb and retain a portion of the arsenuretted hydrogen produced. The only inconvenience resulting in

such a case from the employment of alcohol is, that it is apt to precipitate the sulphate of zinc produced in the process; an effect, however, which never takes place, unless the sulphuric acid is nearly saturated with oxide of zinc.

The method of obtaining a metallic sublimate or a deposit of arsenious acid must be so well known to our readers, that we need not follow our author in his description. There is here nothing new. Orfila places such implicit confidence in the results, that he considers any other process for the detection of arsenic, mixed with organic matter, superfluous. It is at this point, we think, he carries his views too far; and, as this involves a question of the highest importance in legal medicine, it will be necessary to examine the grounds on which he rests his exclusive opinions. The question in the most simple form is this: Supposing that there was no evidence of the presence of arsenic in an organic liquid on the application of the usual tests, while the hydrogen apparatus gave a brown stain on glass, is this a satisfactory proof on a criminal trial of the existence of the poison? According to Orfila, it would be deemed so under certain limitations; and we shall now describe the characters which he assigns to true arsenical stains, as well as the means which he proposes to distinguish them from others which resemble them. We may premise that, very soon after the announcement of Marsh's discovery, Mr. Lewis Thompson ascertained that antimony was liable to be taken up by a current of hydrogen, and to become deposited like arsenic on cool surfaces exposed to the flame.

"1. The stains of *arsenic* are of a brownish yellow colour, and resplendent; when the arsenic is in large quantity they are of a brown black; when the proportion is very small, or the poison is mixed with organic matter, they are of a canary yellow colour. The stains of *antimony* are of a deeper colour, blue and brilliant when thick, but of a yellow brown when the layer of metal is very thin. Neither the stains of arsenic nor those of antimony are volatile at common temperature; they are not deliquescent, nor do they redden litmus paper. 2. An *arsenical* stain, of whatever thickness, is entirely volatilized in from half a minute to a minute, when exposed to the flame of hydrogen gas, as in the common philosophical lamp. The antimonial stain, on the contrary, even when thin, is not volatilized on exposure to this flame until after the lapse of five or six minutes. The stain spreads, becomes lighter coloured, white oxide of antimony is formed and volatilized, but a grayish yellow spot always remains. 3. The stains of both metals are easily dissolved by a few drops of strong nitric acid; and, on evaporating to expel the excess of acid, a white or whitish yellow residue is obtained from arsenic (a mixture of arsenious and arsenic acids), and a yellowish residue with antimony (yellow oxide). If a drop of a solution of neutral nitrate of silver be poured on the *cooled* residues, the oxide of antimony undergoes no change, while the arsenical residue is converted into a brick-red arseniate, mixed with yellow arsenite of silver. If the brick-red arseniate be treated with a drop of ammonia, the red colour comes out clearer; the oxide of antimony, on the other hand, mixed with nitrate of silver, becomes blackened as soon as it is touched by a drop of that alkali. 5. If, after having used the last test, we can procure more metallic stains, these may be dissolved in diluted nitric acid:—the solution evaporated to dryness, and the white residue obtained dissolved in boiling water. The aqueous solution thus procured, acidulated with one or two drops of muriatic acid, may be treated with sulphuretted hydrogen; if the stains be *arsenical*, yellow sulphuret of arsenic will result, which will be particularly developed on boiling the liquid. Antimonial stains thus treated, yield with sulphuretted hydrogen an orange-red precipitate." (p. 405.)

M. Orfila thinks that a practised eye would easily distinguish by the colour the flame of arsenuretted from that of antimoniuretted hydrogen. He recommends that, on the performance of these experiments, the two gases should be produced and burnt in distinct apparatuses. The flames in each case give off a white vapour if the respective metals be in large proportion, but there is no white vapour when the quantity is small. (p. 408.)

The author declares that it is not necessary to procure the five characters above mentioned in order to affirm that the stains are of an arsenical nature.

"The stains which present the first three characters, and one of the two others, should be pronounced arsenical, and, à fortiori, when all the five characters exist, there can be no doubt of their being formed of arsenic. Strictly speaking, one who is much experienced in researches of this kind might decide on the presence of arsenic when he procured the *first three characters only*, especially when the stains have resulted from acting on the incinerated residue of the alimentary canal, liver, spleen, kidneys, lungs, or heart; for there is no substance which can yield stains possessing these three characters, when, after having been mixed with the organic tissues, it has undergone the carbonizing (?) action of concentrated nitric acid." (p. 408.)

Thus, then, we learn that M. Orfila would rely upon the appearance of the stain, its entire volatilization when exposed to a flame of hydrogen, and the colour of the residue left on evaporating a solution of the stain in concentrated nitric acid. We must remember that we are here called on to form an opinion, in which we assume there is no other evidence to assist us; and this opinion may turn the scale in the minds of a jury against an accused party. How cautious, then, should we be of grounding our judgment upon circumstances open to the least doubt or suspicion! We hope to show that, in these extreme instances in which ordinary chemical analysis fails, the criteria proposed by M. Orfila are not safe, and that they are wholly unfit for the guidance of a medical jurist in a case involving the question of the life or death of an accused party. We should hesitate to express ourselves thus strongly, were it not that, on more than one occasion recently, these views have been laid before the public, and have had considerable influence on the verdicts of juries. The reputation which our author has so deservedly acquired as a toxicologist, has led to his opinions being received, both by the public as well as many professional men, without that rigorous examination which opinions on such delicate subjects, and involving so serious an alternative to accused parties, would have met with had they emanated from men of less repute. After all, the question of poisoning in criminal jurisprudence is one of fact: the reputation of the witness cannot affect the facts on which his evidence is founded, while it may dispose us to receive them without sufficiently examining how far they warrant the opinion which he expresses. If any man could thus lead the judgment of others in those questions, it is the author of these Memoirs, and we therefore hold it to be our duty to determine from his own statements whether he is warranted to do so in the case before us.

In the first place, with regard to the appearance of the stains. Antimony never presents, according to our observation, the decided metallic lustre which often results from arsenic, although looking at the antimonial stain from the back of the glass, a metallic glimmering is apparent.

When, as it often happens, the arsenical stain is free from lustre, the appearance alone cannot distinguish it from antimony. From this it follows, that the antimonial stain may be often mistaken for the arsenical; the same black incrustation, destitute of metallic lustre, may exist in the case of either metal. We do not deny that those who have experimented much on this subject may be able to form a conjecture from the appearance; thus we have observed when the stains are taken on glass, and we view them by transmitted light both are opaque in the centre; but the arsenical stain is *brown* at the border, while the antimonial, is of deep gray *black*. Still legal evidence must not be based upon conjectures; and that there is strong room for doubt, is proved by the author himself advising us to search for other characters before we decide. Now what are these other characters? the rapid volatilization of the arsenical stain by heat is a weak criterion, for we have found that the antimonial stain is entirely volatile, and the difference is only one of time; the deposit in the latter case being driven off more slowly than in the former; indeed, if the flame should by any means meet the layer of antimony, it is soon mechanically dissipated by the current, while, if the arsenical stain is collected on a thick plate of glass, or held at a distance from the flame, it is very slow in disappearing. But allowing with Orfila that there is the difference of time mentioned by him in the two cases, is the bare fact of a sublimate being volatilized in one minute or in five minutes, sufficient to justify a medical witness in pronouncing an opinion on the presence of arsenic, in a question of life and death? According to our view, it is not: no man is justified in acting upon such niceties, and assuredly no evidence founded upon a fact of this kind would have any weight in an English court of justice. 3. The solution in nitric acid, with the colour of the residue obtained on evaporation, furnishes no safer ground of distinction, when such minute quantities are the subject of experiment. We might make the same remark with regard to all the other alleged distinctions, pointed out by the author. Perhaps the best of these is the *fifth;* when the crust first dissolved in dilute nitric acid is evaporated, again dissolved in water, and treated with sulphuretted hydrogen. A yellow sesquisulphuret results with arsenic, an orange red, with antimony. We may here remark, however, that if such a difference of colour can be observed in operating on the crust, there must be in general enough arsenic or antimony present, to allow of the free application of other tests, and to indicate the nature of the metal beyond all dispute. Our objections apply to cases in which we are obliged, if we form any opinion at all, to decide from the hydrogen test alone. How is it possible to obtain evidence, such as that which we are advised to seek for, from stains which may not weigh the thousandth of a grain? or who is always to distinguish in these minute pellicles of liquid, the colour produced by sulphuretted hydrogen in the case of antimony, from that formed when arsenic is present? The author obviously rests his decision chiefly on the first three criteria; but individually, we have proved these to be liable to fallacy, nor do we see how, when taken collectively, they can acquire such force as to become the basis of a sound medical opinion. Besides, in giving these statements to the world, it is not considered that the experiments will probably be performed by many, who, al-

though tolerably versed in chemistry, may not come up to the standard of experience indicated by M. Orfila. In such hands the most dangerous mistakes may arise, if too great a reliance be placed upon criteria, which, while they are themselves weak, require considerable nicety in their application.

But does it follow that we are without any means for distinguishing the arsenical from the antimonial crust? certainly not; we intend by these remarks to show that we must not rely on our tests, where only minute traces of the suspected substance can be procured. The chemical differences pointed out by M. Orfila are applicable with safety where the metallic sublimate is procured in moderate quantity; but after all we have found it better to test the liquid from which the sublimate is derived, than the sublimate itself; a slip of bibulous paper dipped into any organic liquid containing arsenic or antimony will clearly indicate the nature of the metal present, by the colour assumed on exposing it to a current of sulphuretted hydrogen gas, with the action of ammonia on the coloured sulphuret formed. If no change of colour take place, under these circumstances, we much doubt whether, in the event of a sublimate being procured by the hydrogen apparatus, the quantity would not be too small to allow of our forming a safe opinion.

We have thus thought it right to dwell on this part of M. Orfila's analytical process; because most of the new views which he has produced in these memoirs are materially affected by the means which he employs to identify the arsenical stains.

Antimony is not the only substance liable to give rise to doubt. The author candidly states, that he has observed stains to result even from organic matter only. In some cases, where no arsenic was present, brilliant stains of a brown colour were produced, which differed from those of arsenic in being less volatile, and having none of the chemical characters of that metal. He has found stains to result from the presence of phosphorus, iodine, bromine, sulphur, selenium, and tellurium, (p. 410.) It is true that chemical differences exist in all these cases; but in a practical light, we cannot escape the inference, that the mere production of a stain in a suspected case is insufficient; and that the application of tests to this stain, where it is small, and the experimentalist not well versed in chemistry, must always leave a doubt on the mind as to its real nature.

But the author, with his acknowledged skill, candidly admits that he has found himself at a loss to determine the nature of these metalloidal stains in certain cases. The same difficulty may easily occur to others, who rely too exclusively upon the hydrogen test. We shall therefore transcribe his statement:

"There are other stains more important than those just adverted to, because they are often produced and may be sometimes confounded with those which are really of an arsenical nature. They are particularly observed to result from the solution of the incinerated residue of muscles, which have been acted on by strong nitric acid. They assume different characters; they are white, opaque, immediately volatile in the flame of hydrogen, and gradually disappear after several hours' exposure to the common temperature of the air. In acting on organic liquids, these spots are especially liable to be produced if the flame is weak, and it be kept some time in contact with the porcelain. They are likewise

formed in certain cases in simply employing water, zinc, and some kinds of *distilled* sulphuric acid." (p. 412.)

He goes on to remark that they dissolve in strong nitric acid, but not so rapidly as those of arsenic; the residue obtained is yellow or white, and after cooling, on treating this with nitrate of silver, no brick-red arseniate results. He then continues:

"It is impossible to declare that these stains are arsenical, since they do not present all the necessary characters. What is their nature? It is difficult to say. They are probably composed of organic matter and a very minute quantity of arsenic, not susceptible of detection by the usual processes. It may be remarked, however, that about twenty real arsenical stains, of the size of those just mentioned, will yield with nitric acid and nitrate of silver, a brick-red precipitate. We cannot be too cautious when we are required to decide upon the nature of the stains obtained. A witness is not justified in pronouncing them to be arsenical, unless he has found them to possess the first three characters which I have assigned to them." (p. 412.)

We cannot fail to remark here a slight inconsistency, which is sufficient to show us that the author's mind is by no means made up as to the degree of reliance to be placed upon evidence of this kind. The stains to which he alludes in the first part of the preceding extract, and which he admits occur when there is no evidence of the presence of arsenic, actually possess, although in an imperfect degree, the three characters which he deems all-sufficient in the hands of an experienced man, to determine their real nature. It is true that nothing is said about metallic brilliancy, but those obtained from arsenic, as it is well known, do not always possess this. They are white and opaque, immediately volatilized by heat, are dissolved by nitric acid, and leave a white or yellowish residue on evaporation. The non-production of a brick-red precipitate on adding nitrate of silver, which he does not deem an essential character, is then here the only criterion for an opinion; and the fallacies to which this criterion is exposed, when we are operating on such minute quantities, need not be further adverted to. There is obviously no boundary line to be drawn between stains of this description and those actually resulting from arsenic. The author substantially admits this, in confessing that he is unable to form an opinion of their real nature.

M. Orfila then refers to the case of Soufflard, the first in which arsenic was actually detected in the blood and organs of the human body. (This man destroyed himself by swallowing a large dose of arsenic.) The blood used in the experiment, amounting to about eight ounces, was abstracted from the heart and vena cava. On simply boiling the blood in distilled water, scarcely any was procured; but on incinerating the residue of the clot with nitre, a large proportion was obtained. A parallel experiment on a similar quantity of blood taken from a female patient gave no arsenic, clearly showing that the metal existed in the body of Soufflard, and not in the reagents employed to extract it.

"The upper and lower extremities were boiled in distilled water with potash. The decoction yielded arsenic. A parallel experiment on the lower extremity of an adult, who had not been poisoned, gave no arsenic. The liver, spleen, and lungs of Soufflard treated with nitrate of potash and sulphuric acid were

also found to contain the poison; while the same organs of an adult, who had not been poisoned, contained none. It is true that in the last case brown stains resembling those of arsenic were procured; but on examination these were proved to be owing to impurity." (p. 416.)

This case seems to establish clearly that arsenic is absorbed into, and circulated with the blood of a person who has been poisoned by that substance.

The first memoir is terminated with a series of conclusions from the foregoing experiments and observations. We have incorporated the greater number of these in our remarks, and we shall now, therefore, only briefly extract what may be novel or interesting.

"Arsenic is to be found in the urine, as well as in the blood and viscera of those who have taken it. When it cannot be detected in the blood it may be in the viscera and urine, and when it begins to disappear from the viscera, it will be more abundantly found in the urine. It must not be supposed, however, that it is rapidly eliminated from the blood and viscera. In two cases in the human subject it was found in the blood fourteen hours after the ingestion of the poison, in such quantity as to render it probable that evidence of its presence in that liquid would have been obtainable twenty-five and thirty hours after the poison had been taken. The viscera will retain the poison for several days." (p. 418.)

From what has just been said, it is obvious a time must arrive when none will exist in the body, if the individual survives long enough. Hence it is necessary to reserve for analysis the urine of those who are supposed to have taken this poison. With respect to the blood, a few ounces are sufficient for determining the presence or absence of arsenic. Venesection will thus enable us to determine the fact of poisoning, in a person who survives the effects of arsenic. Orfila thinks that venesection should form a part of the treatment, since it not merely allays inflammation, but actually withdraws a portion of the poison from the circulating system. (p. 419.)

The following case, which occurs in the third memoir, will serve to illustrate the preceding remarks:

"Madame N., with the intention of destroying herself, swallowed a spoonful of finely-powdered arsenic. In twelve hours the symptoms were such as to render it necessary to bleed her. Venesection was performed and leeches were applied to the epigastrium. The next day M. Casimir Broussais sent me about ten ounces of blood......... The liquid obtained from the incinerated residue of this quantity when introduced into Marsh's apparatus, gave about twenty small arsenical stains, not much coloured, but brilliant. The patient was greatly relieved by the bleeding, and ultimately recovered." (p. 464.)

This is a case of singular interest. It will serve as a good precedent for medical jurists when they are anxious to obtain evidence of arsenical poisoning in the living subject.

2. The Second Memoir is devoted exclusively to an examination of the means by which the purity of the reagents employed in the process may be satisfactorily determined. It must strike every professional reader, and of course it would occur to an acute barrister, that in these refined chemical experiments the operator is much exposed to error, from the fact of arsenic being accidentally present in the tests or in the apparatus employed. When a witness infers the presence of the poison from the discovery of such minute traces, he is bound to show that no fallacy of this kind could possibly have existed. It is not our intention to follow

the author into the minute instructions which he gives on this subje
Sulphuric acid occasionally contains traces of arsenic, but we have tri many specimens of the kind commonly used by chemists in this count without meeting with one possessing this impurity. Neither nitric ac nor nitrate of potash is in general found to contain arsenic, but the zi of commerce occasionally presents traces of it. We have never met wi arsenic in using good laminated zinc; and the impurity is so rare, th Orfila only found it three times in making five hundred trials. Orfi does not appear to be aware of the fact, first noticed, we believe, l Dr. Geoghegan of Dublin, that arsenic is apt to attach itself to the zi employed when it has been frequently used, and hence the necessity f renewing the zinc in cases of great delicacy. No one, however, acquaint with Marsh's test, would proceed to analyze a liquid by it, until he ha first satisfied himself that the materials employed were entirely free fro traces of the poison. One or two negative results from the apparat and materials are not sufficient to establish this. Many must be made a case of importance, and at a certain interval from each other. W agree with the author, that the vessels of copper, iron, or porcelain, en ployed for making the decoctions of animal matter, are not likely to yiel a particle of arsenic under the circumstances.

There is one point connected with the impurity of tests whi M. Orfila does not appear to us to make sufficiently clear. It is we known, and he admits the fact, that no evidence of the presence of arsen may be obtained when we operate on one organ, while, by taking a larg portion of the viscera, the metal will be procured. No application of th important fact is made in the directions given for trying the purity of tl tests, while it is obvious that it must apply with as much force to them to the organs of the human body. Thus an ounce of nitrate of pota may yield no arsenic, while, by operating on several pounds, *traces* of tl poison might be procured. In Orfila's process, large quantities of nitr and sulphuric acid are used, and when by it only the *faintest traces* arsenic are procured, we must insist upon the necessity of examining f that poison so much of the tests as may have been actually employed the investigation. Nothing short of this will furnish satisfactory medic legal evidence of the real source of the poison.

3. The Third Memoir is perhaps the most important of the whole, sin it is here we find the latest improvements in the author's analytical pr cess. Many experiments were performed to determine whether nitr acid was preferable to nitrate of potash in decomposing organic matte as also the degree of heat and the length of time most favorable to tl process of incineration. We subjoin his conclusions, which appear to somewhat confused:

"1. In using nitric acid for detecting arsenic which has been absorbed, must avoid carbonizing the soft parts rapidly and with flame, or slowly and wi the extrication of a pyrogeneous odour; for in either case, the greater part, not the whole of the arsenic, will be lost.

"2. The loss of arsenic is great in proportion to the quantity of organic ma ter incinerated.

"3. If we wish to extract all the arsenic present, this will be best accomplish by incinerating the organs in an instant, without flame or ignition, and with tl production of a large quantity of thick smoke, and of a bulky and spon charcoal.

"4. That the organs, in order to undergo this form of carbonization, mu

be previously dried, and then digested with a fixed quantity of nitric acid, which however varies with each organ.

"5. That this process (with nitric acid) is preferable to that with nitrate of potash, since there is no perceptible loss of metallic arsenic." (p. 454.)

Our space will not allow us to enter into all the details relative to the author's process, but as no complete account of it has yet appeared in English, and it undoubtedly possesses advantages over all the methods hitherto recommended for detecting arsenic in complex organic substances, we feel it a duty to give such a description, as to render it easy to be followed by our readers.

The viscera cut into small pieces are to be boiled for about six hours in a porcelain vessel with distilled water, holding a few grains of pure potash dissolved. This aqueous decoction is filtered, acidulated by muriatic acid, and a current of sulphuretted hydrogen gas is then passed into it. If, instead of the viscera, we employ the upper and lower extremities for making the decoction, the fat must be separated by allowing the liquid to cool before the gas is passed in. After a longer or shorter period, sometimes several days, the yellow sulphuret of arsenic, mixed with animal matter, falls down. The precipitate, whatever may be its nature, is separated by filtration. The filtered liquid, although no longer affected by sulphuretted hydrogen, may still contain arsenic : to prove this, evaporate to dryness, and digest the residue in strong nitric acid, in a way to be presently described. The solid residue of the viscera, left after making the decoction in water, is to be perfectly dried, and then immediately digested in strong nitric acid. In general the decoction in water will have removed the whole of the arsenical compound ; but this subsequent treatment with nitric acid will ensure the entire extraction of any that may remain. Our readers will remark that, in speaking of Soufflard's case, the author stated that, by decoction in water, but little arsenic was obtained from the blood, the greater part being left in the solid residue. This discrepancy leaves us in uncertainty as to what is really the fact.

We will now resume the analysis of the precipitate supposed to contain sesquisulphuret of arsenic. This is of a yellow brown or gray colour : it is to be well washed with distilled water, and then digested several times in a weak solution of ammonia. The alkali dissolves out the sulphuret of arsenic, which is then precipitated by the addition of nitric acid. The sulphuret after this is of a brighter yellow, from having lost a portion of organic matter. It is allowed to subside in a porcelain capsule, and the supernatant liquid is withdrawn from it by means of a pipette. The sulphuret is now dried without removal : one portion of it may be reduced as usual by black flux, another portion may be reserved for decomposition in Marsh's apparatus. To effect this, the sulphuret must undergo a further change before its introduction, since nascent hydrogen cannot remove arsenic from the pure sesquisulphuret. A portion of the yellow powder is heated for a few moments with strong nitric acid, any organic matter present is thereby oxidized and destroyed; the sulphur is transformed to sulphuric, and the arsenic to *arsenic acid.* The surplus nitric and sulphuric acids are expelled by evaporating the liquid to dryness, and the dried residue (arsenic acid) is dissolved in distilled

water, and placed in the apparatus, when the evidence of the presenc arsenic is speedily obtained. (pp. 398, 456.)

There is here a very great improvement on the common metl Organic matter, mixed with the sulphuret, has hitherto much embarra the experimentalist in his attempts to reduce the metal, and no mear separating it have been devised equal to those above recommende Orfila. The arsenic acid, it is to be observed, will yield a sublir with black flux, much more readily than the pure sesquisulphuret.

The author avows that, in his view, the whole of these proceedings not absolutely necessary; and if he recommends the formation of a de tion by boiling the animal matter, and the use of sulphuretted hydro it is to bow to the general feeling of the profession, by applying an a tional test. So far from being necessary, he seldom resorts to it, but ceeds to act at once on the dried organ by nitric acid; or, if the parts be abundant, on an aqueous decoction of them evaporated to ness. (p. 456.)

Different proportions of nitric acid are recommended for operatin different organs,—for these we must refer to the work itself. The portions appear to us large; thus, for a dried liver weighing tw ounces, no less than *thirty-four* ounces of strong nitric acid are quired. (p. 457.)

The operation with nitric acid is very simple. The whole of the is placed in a porcelain vessel, and heated over a slow fire; the ani matter thoroughly dried, and reduced to a fine powder, or to fragment gradually added at short intervals. Deutoxide of nitrogen is abunda evolved, and the animal matter dissolves. If much of this be put in at o or it be not thoroughly dried when introduced, a considerable quantit froth is formed, which by boiling over may cause a loss of arsenic. liquid, which is at first yellow, becomes orange-coloured, and finally deep red; it is at the same time inspissated by evaporation. In continu the heat, the whole of the matter becomes carbonized; and when a tl black smoke begins to escape, the vessel is to be removed from the The whole of the arsenic is here converted to arsenic acid, and this is ea removed from the carbonaceous residue by digesting it in distilled wa The solution is, if necessary, concentrated by evaporation, and then int duced into Marsh's apparatus, when the evidence of arsenic will be tained. If the whole body is to be examined, separate decoctions of muscles and organs may be made, these evaporated to dryness, and dried residues united and digested in nitric acid in the manner describ (p. 458.)

The author has found some inconvenience from the froth produced introducing the solution of arsenic acid into Marsh's apparatus. He the fore advises the use of a large quantity of olive oil; we should prefer e ploying alcohol for reasons already stated. At the same time, if the resı ing solution of arsenic acid were so viscid as to produce much froth appears to us that this might be got rid of by evaporating it to dryn again digesting in nitric acid, evaporating, and then redissolving in tilled water.

"In these experiments, care must be taken that too much nitric acid is employed, that the heat is not too intense; and that, during carbonization,

contents do not burst into flame. Under any of these accidents, the arsenic might be entirely dissipated." (p. 460.)

The efficacy of this process was well tested as to time in the two following cases:

"The liver from the body of N. M., suspected to have died from poison, and which had been interred for five months, was carbonized by nitric acid. The aqueous decoction yielded an abundance of arsenical stains." (p. 463.)

"I obtained from the heart, mesentery, omentum, and a portion of the intestines of M. Cumant, a small quantity of arsenic. This person died in December, 1838: his body was exhumed in July, 1839, and the organs above mentioned were examined in the month of December following." (p. 464.)

When nitrate of potash is used for the process of incineration instead of nitric acid, then a large quantity of sulphuric acid is required to separate the alkali from the resulting carbonate and arseniate. The sulphate of potash formed, is separated by crystallization. The arsenic acid is freed from any excess of sulphuric acid by evaporation; but if Marsh's apparatus be used, this last step is not necessary.

4. The Fourth Memoir describes the discovery of the presence of arsenic, as a natural constituent of the human body. This is a startling novelty, and the evidence upon which the statement is based therefore requires close examination. The author affirms that arsenic exists in human bone, as well as in that of the dog, ox, and sheep. The arsenical compound existing in bone cannot be extracted by boiling that substance in water, hence no medico-legal difficulty can arise on that ground. The arsenic is lost when the bone has been intensely calcined, and when the calcination has taken place in air, or in immediate contact with the heated fuel. The presence of arsenic was detected

"By heating the bones above an intense fire without contact until they become friable and of a grayish-white colour. They were then finely powdered and sifted. *Eight ounces* of the powder were digested in distilled water, with three ounces of sulphuric acid; after four days more distilled water was added, the mixture boiled, and the loss by evaporation made up. The filtered liquid, placed in Marsh's apparatus, yielded numerous brown arsenical stains of great brilliancy and thickness." (p. 467.)

"Arsenic was also detected in bone which had been carbonized in a common crucible, at a high temperature, but without access of air." (*Ibid.*)

In boiling six ounces of common bone ash with distilled water, containing a small quantity of pure potash, a liquid was obtained which, on filtration and neutralization by sulphuric acid, gave arsenical stains with Marsh's apparatus. (p. 471.) We are at a loss to reconcile this statement with the precautions which the author had just before laid down as absolutely necessary for determining the presence of arsenic in bone. We have tried the bone ash of commerce without detecting any trace of arsenic.

When we look to the number of eminent chemists who have analyzed bone without finding any arsenic present, we are inclined to think that the experiments of M. Orfila require corroboration, before we admit the existence of arsenic in that substance as a natural constituent. His enthusiastic devotion to the subject may have led him to overlook some source of error. Besides, he appears to have relied upon the use of Marsh's apparatus alone, whereas, did arsenic exist so abundantly as his results

would lead us to suppose, other corroborative tests might be employed. May not the deposit have been some combination of phosphorus? At any rate we must remind him, when he relies so exclusively on one test, of what he is reported to have said on the recent trial of Madame Laffarge. "Marsh's apparatus is of recent date. It has not yet been perfectly studied by every one, and even those who have studied it frequently find themselves embarrassed in using it."

His observations on this subject require revision. Thus he has not settled whether the alleged arsenic exists in the animal or in the earthy part of bone, nor has he analyzed bone in the humid way, that is, in separating the animal matter by dilute muriatic acid. The arsenic in this case should be found in the acid menstruum, or in the separated gelatin.

In submitting the blood and viscera of subjects which had not been poisoned to these analytical processes, he has not been able to procure the smallest evidence of the presence of arsenic in them. At the same time he will not undertake to say positively that it is absent. The present means for its detection may not be sufficiently delicate; a more improved process, or the bare fact of operating at once on fifteen or twenty brains, or a like number of livers and lungs, might lead to the discovery of the metal as a constituent even of these viscera. M. Orfila might have added that by operating on subjects of different ages, different sexes, and different states of health, arsenic might in some instances be discovered, at least to the same extent that it exists in bone. This we consider to be the height of transcendental analysis. M. Orfila seems to be little aware from the uncertainty in which he leaves the subject, that he is giving the benefit of an *argumentum ex concesso* to those who are opposed to his views. To our minds it is left uncertain whether by more numerous experiments on different subjects he might not obtain the same evidence of the existence of arsenic in the blood and viscera, as in the skeletons of healthy subjects; and thus, all the practical benefit to science, derivable from his experiments, would be frittered away.

We have yet purposely avoided speaking of the *muscular* system. Do the muscles of the human body contain arsenic or not? M. Orfila declines answering the question one way or the other; all that he says is: "*It is not proved that they do.*" (p. 476.) Many experiments were performed by him to determine this point; and as a general result, he admits that he obtained from healthy muscle, treated by his process, white, opaque and volatile stains, some brown and dull, others small and brilliant, or as we understand it, "metallic." (pp. 476-81.) These stains are said to be not very soluble in cold nitric acid; but dissolved by it when diluted and boiling, leaving a white residue on evaporation. The author acknowledges that the stains may differ a little in character, owing to the arsenic (?) being in very small quantity, but he observed: "If I do not affirm that muscle contains arsenic, I will not conclude from my experiments that it does not contain any." (p. 482.) From his facts we feel bound to conclude, either that the rules by which he has determined the presence of minute traces of arsenic in other cases are erroneous, or that human muscle really does contain that poison as a natural constituent. With a larger quantity of muscle than he employed, or with an improved process, he thinks arsenic might be demonstrated to exist in the muscular system. (*Ib.*)

Although he contends that the stains from muscle may be distinguished from those which are truly arsenical, yet he advises a witness in searching for arsenic in a suspected case, to confine his analysis to the viscera. "He may thus avoid objections, which, although of little value, might still have an influence on the minds of certain juries." (!) (p. 485.)

The author appears to us to leave this part of his subject in great obscurity. At p. 476 seven experiments are related, in which stains resembling those of arsenic were procured in each case from muscle, although not obtained from poisoned subjects. At p. 416 we find that the production of similar stains from muscle in Soufflard's case, was taken as evidence of poison having been absorbed, and conjointly with this, the lower extremity of a normal subject was submitted to the same operation without any stains resulting! How are such differences to be reconciled? It seems to us clear from the experiments (p. 476) that the production of stains from muscle is no evidence of poisoning; while their non-production in the parallel experiment connected with Soufflard's case, is wholly opposed to the results of the seven experiments subsequently detailed.

The Fifth Memoir involves many new points which are likely, if corroborated, to give rise to some medico-legal discussion. The first question which the author proposes to examine is, whether arsenic ever exists in the soil of cemeteries or graveyards. He answers this in the affirmative; but the admission is confined to those cases in which the earth is greatly mixed with the fragments of bones. Indeed, it is his opinion that the arsenic is simply that which he alleges to be naturally contained in bone. It was not sufficient in these cases to digest the soil in hot or cold water and operate on the decoction. He found it necessary to macerate the earth for several days in dilute sulphuric acid, and then boil the whole for some hours in strong sulphuric acid. (p. 493.) Even in these cases the stains obtained, were extremely small, and the arsenic only in infinitesimal quantity. Assuming that arsenic really exists in the earth of cemeteries, we find many experiments performed for the purpose of ascertaining whether in such a case it is liable to be infused into a body by the draining of water, so as to give the idea of the person having died from poison. Without actually denying that this might occur, the author thinks it "difficult to admit that any soluble combination of arsenic could so penetrate into a body as to give the idea of poisoning: nevertheless, a serious mistake might be made, unless, before proceeding to the analysis, all the arsenical earth was removed from the exterior." (p. 499.) Then we find him suggesting that cases of this sort might be detected by making separate analyses of the exterior and interior of the body, and noting the relative proportions of arsenic in each!

Lastly, Is the body of a poisoned individual likely to lose the arsenic present in it, by the drainage of water after long interment. This question relates not so much to the poison existing in a free state in the viscera, as to that minute portion which has become absorbed into and incorporated with them. There are no facts to decide it; but the author thinks it very unlikely that this small portion of poison could be thus removed from the body, so long as any visible traces of members or viscera remain. (p. 503.)

Among the extraordinary conclusions in this memoir, we may notice the following:

"If in the soil of a cemetery we discover an arsenical compound, *soluble in cold water*, there would be strong reason to presume that it came from some bodies in the neighbourhood, *unless it were proved that that part of the cemetery had been watered with a solution of arsenious acid, or some other arsenical preparation.*" (p. 504.)

We have placed the last lines in italics to show how far the author deviates from what is practical, and how prone he is to work out his argument *ad infinitum*. It appears to us that it would have been only consistent in him to have taken into consideration the possible presence of arsenic in the iron or brass materials used about a coffin, and to have made further exceptions on this ground. Seriously, however, we must declare our conviction that these speculations are liable to do mischief instead of benefiting the science of medical jurisprudence. We know that the latter is really the author's object, and that the science owes him much; but we are sorry to find him advising that researches so crude and ill-digested should ever be made the basis of medical evidence.

All the reasoning in the last memoir depends on the admission of the presence of arsenic as a constituent of bone; but we do not think that this point is yet satisfactorily established. We allow that the author may have obtained stains resembling those of arsenic, not only from bone but from the soil of cemeteries: still we learn from his own statements that unless these stains be of a very decided character, there is no possibility of distinguishing them from those which are not arsenical. We think he is too much disposed to rely upon slight appearances, and to take what is barely probable for what is real: this is the only way in which we can explain several inconsistencies into which he has been led in these memoirs. We must here remind him of his own observation on a late memorable occasion in cases of poisoning. "Medical jurisprudence is not satisfied with suppositions, it requires positive proofs. The metal must be reproduced"—we would add—to the reasonable satisfaction of all chemists.

In these memoirs there are many sound practical remarks: the process for extracting arsenic from organic mixtures is the best in our judgment which has been hitherto devised. We could have wished to see some of the author's opinions based upon less questionable grounds, since they are likely to be extensively circulated, and have much influence on the practice of medical jurisprudence.

The Sixth Memoir refers to some facts connected with *tartarized antimony* as a poison. These are new and therefore require to be briefly adverted to. Antimony, as we have already seen, is susceptible of being reduced by hydrogen in Marsh's apparatus. Very soon after Mr. Thompson's discovery of this fact, the apparatus was used in this country as a test for antimony; but M. Orfila, has, we believe, the credit of first employing it to detect the presence of that metal in the organs of the body. Magendie had inferred from his physiological experiments that it was actually absorbed into the circulation, but there were no means of demonstrating its positive existence in the blood and secretions. The author having satisfied himself that antimony might be detected in or-

ganic mixtures by a process analogous to that which he had successfully employed for arsenic, proceeded to determine the question by chemical analysis.

As general results he found, "1, that tartarized antimony introduced into the stomach or applied to the cellular tissue is quickly carried into the substance of the viscera, where it remains but a short time, especially in the non-secretory organs; 2, that after having left these viscera, it is eliminated with the urine and probably also with the other secretions." (p. 517.)

We learn further that from the well-known emetic powers of this body it is likely to be soon expelled from the stomach; and chemical analysis may fail to detect it in that organ, when it will still be found in the other viscera, which it may have reached by absorption. The liver and the kidneys generally retain traces of the metal longer than the other organs, owing to the blood remaining longer in them. The salt appears to be only partially decomposed; for it is capable of being, to a certain extent, removed from the blood and soft parts by boiling water; but the process for its complete extraction is a little more complex. The viscera being thoroughly dried are to be decomposed by nitric acid, as in the case of arsenic. The resulting carbonaceous residue is to be boiled in muriatic acid with a few drops of nitric acid, the liquid filtered, and then introduced into Marsh's apparatus. In this case, the salt of antimony present is reduced, and chloride of antimony is formed by digesting the residue in nitro-muriatic acid. Should we be examining a liquid, we have only to evaporate to dryness, and proceed with the residue in the same way. The author finds that the metal may be discovered in the urine when it has left every other organ and secretion; but under all circumstances, it disappears from the body with much greater rapidity than arsenic.

The metallic stains suspected to be antimonial are to be identified by the means already pointed out.

In the Seventh and last Memoir we have some account of the salts of *copper*, and the method of detecting that metal in the viscera of the body. Before attempting to determine whether or not, in cases of poisoning, the metal copper was conveyed into the circulation, it was of course necessary to find out a sure and delicate process for detecting its presence. Copper being a fixed metal, a very different proceeding was here required to that adopted in the case of arsenic and antimony. After many trials, Orfila came to the conclusion, generally agreed upon among chemists, that the deposit of the metal on iron is the best test, when it exists only in minute traces.

The salts employed in the experiments were the acetate and the sulphate; and the object was to ascertain whether any portion of the metal had become absorbed and carried into the viscera. Dogs were selected for the experiments: in some cases the salts were introduced into the stomach, and the œsophagus tied; in others, the powdered salt was placed in a wound made in the skin on the inside of the thigh. The animals either died from the effects of the poison or were killed, and their viscera immediately examined.

The following plan was resorted to for detecting copper. The viscera

were cut into pieces and boiled for six hours in a quantity of distille water acidulated with a few drops of nitric acid. The decoction was fil tered and evaporated to dryness. The dry residue was carbonized b heating it in strong nitric acid; and the carbonaceous ash was the treated with muriatic, mixed with a few drops of nitric acid. This wa filtered and again evaporated to dryness; the residue was then dissolve in water acidulated with sulphuric acid. On introducing a piece o polished iron into this liquid, a certain quantity of copper was deposite on it in the course of an hour. (p. 533.) Many hours are sometimes re quired for this deposit to take place, where the quantity of metal i small.

In this way, copper was detected in the lungs, heart, liver, spleen, an kidneys of animals experimented on; nor did the results vary, whethe the analysis were performed on the organs extracted from the recentl killed animal, or from one which had been some time dead. It is worth of remark, that there was no satisfactory evidence of copper either in th blood or in the urine of these animals. (pp. 540-2.) The experiments however, clearly show that the metal in some form or other does find it way into the circulation.

But since copper is said to be a natural constituent of the healthy ani mal organs, the author deemed it necessary to perform some experiment in order to enable medical witnesses to distinguish this normal coppe from that introduced into the system as a poison.

In operating on the viscera of the healthy human body and dogs, i the way already described, not a trace of that metal could be found Water, especially at a boiling temperature, will dissolve the greater pa of the copper introduced into the viscera as a poison; but it will not re move any portion of that which is a normal constituent of the orga This normal copper can only be detected by drying and incinerating th greater number of the viscera of the *human* body, and then it is onl found in very minute traces. The same organs in the healthy dog, simi larly treated, do not yield any portion of the metal. This difference i ascribed by Orfila not to the entire absence of copper from the visce of the dog, but to the fact that they occupy much less bulk than in th human subject, and therefore that the proportion of copper is probabl too small to be detected by the same chemical process. (p. 549.)

We do not consider it necessary to enter into the question, relative t the transudation of the poison through the alimentary canal into the su rounding viscera. A question of this kind does not appear to us to hav any practical bearing. The fact of imbibition taking place after death i well known. The only possible case in which any doubt could arise fro such a circumstance is in the very improbable occurrence of the intro duction of copper into the stomach of a dead person. Here imbibitio might give rise to the idea of the poison having been circulated after ab sorption; but as, in such a case, we should have first to account for th presence of a salt of copper in the stomach, the minor would merge i the major question.

With this we conclude our notice of M. Orfila's late discoverie Many of these are in a medico-legal and physiological view of great valu and practical importance. We have already expressed our opinion

the Memoirs on Arsenic. The author has there shadowed out some lines of investigation, which we hope he will pursue with the same zeal which he has hitherto shown. The Essays on Antimony and Copper will be rendered more complete by his extending his experiments, whenever an opportunity occurs, to the human subject. Neither antimony nor copper has yet been found in the blood of man or animals poisoned by the salts of these metals. Antimony has been discovered in the urine of persons who had taken large medicinal doses of this substance, but copper has not been detected under these circumstances, in any of the human organs or secretions. However valuable the results of experiments on animals may be to the physiologist, observations on man are of infinitely greater value to the medical jurist.

ART. III.

A Practical Work on the Diseases of the Eye, and their Treatment, Medically, Topically, and by Operation. By FREDERICK TYRRELL, Senior Surgeon to the Royal London Ophthalmic Hospital; Surgeon to St. Thomas's Hospital; Professor of Anatomy and Surgery at the Royal College of Surgeons in London, &c. Two Volumes; *with Plates.—London,* 1840. 8vo, pp. 533, 566.

IN an introduction extending to considerable length, Mr. Tyrrell summarily states the general pathological and therapeutical views brought forward in the body of his work. The general treatment of ophthalmic diseases, according to Mr. Tyrrell, should have for its object, the regulation of the most important secretions, the regulation of the general power, and the correction of local morbid action.

"Regulation of the general power is, in my opinion, of the highest importance, for I am satisfied that a salutary and curative process cannot be established or maintained whilst the general power is, on the one hand, in excess, or *above par;* or, on the other hand, when it is very deficient or much *below par.*" (p. xxxvii.)

The means enumerated for effecting a diminution of power when it is in excess or *above par* are, principally, spare diet, free action upon the principal secretions, quietude, and abstraction of blood. In regard to the last, our author says (p. xxxviii.) that it "should be resorted to only in urgent and hazardous cases, and then it should be employed cautiously. I consider," he continues, "the general abstraction of blood to be unnecessary and improper, unless the pulse evinces a degree of *resistance* or *incompressibility*, besides an unusual degree of fulness or quickness." It seems to us that Mr. Tyrrell is too much afraid of venesection, and that he is too exclusive in his indications for it. Fully appreciating the importance of *incompressibility* of the pulse as an indication—a point so ably insisted on by Mr. Wardrop—we would have it remembered that, as this distinguished ophthalmological writer remarks, "there are other qualities perceptible in the radial pulse which also indicate the propriety of bloodletting," whether "taken singly or combined with other symptoms, more particularly local pain, hot skin, and white tongue." In the case of the eye, circumorbital pain and redness, it may be mentioned, are, even when taken singly, an excellent criterion; for

whenever these are present, the blood is buffy and the relief from venesection striking. Mr. Tyrrell mentions, as an exception to his rule, the slow, laboured, and compressible pulse which usually exists when there is cerebral mischief or disease. In this case the abstraction of blood, "by relieving excess of cerebral vascular action or pressure, favours the return of a proper degree of nervous power." We have repeatedly seen the same thing effected in the state of depression depending merely on the severity and continuance of circumorbital pain. The "decided and rapid relief of urgent local symptoms" we consider an object of so much consequence that it is to be sought for, even at the sacrifice of slightly protracting the cure. But we deny that the prostration of strength leading to this effect, much less to obstinacy or difficulty of cure, is necessarily produced by that extent of bloodletting which, along with the auxiliary remedies, usually procures the degree of relief above mentioned. It appears to us that Mr. Tyrrell in justly reprobating one extreme falls into the opposite. The following observations of Mr. Lawrence appear to us so completely to meet Mr. Tyrrell's partial objections to bloodletting, that we cannot refrain from quoting them, long as they are, particularly as we are persuaded that practice founded on such principles is much less likely in the hands of the many to produce mischief than the practice founded on the opposite principles which Mr Tyrrell would inculcate:

"I know of no criterion," says Mr. Lawrence, "by which we can deter mine in all cases, whether general or local bleeding should be employed If inflammatory fever coexist with the local disorder, we should abstract bloo from the system; but we cannot say conversely that if such fever be absent w ought to be content with local depletion; an inflammation of the eye, for ex ample, may require free general depletion, although it should not be attende with fever. (p. 105.) .... I never," he continues, "saw a person in jured by a single large bleeding performed for an active inflammation; whil generally the strength is completely restored in twelve or twenty-four hour even after bleeding to syncope. As the restoration of the digestive function and the secretions diminishes the symptoms, where inflammatory fever i present, they who are afraid of weakening by loss of blood recommend i preference aperient and diaphoretic medicines. If you examine the history c cases treated in this way, you find that two, three, or more days are employe in those indirect attempts. Purgatives are given which do not operate; dia phoretics are tried which bring on no discharge from the skin; the local in flammation increases; the general disturbance is aggravated, until the feve comes to an end, when the secretions and digestive functions are consequentl restored. Abstraction of blood to a proper amount accomplishes the desire objects at once. When you have thus removed the load that oppresses the sy tem, the suspended secretions are restored, evacuation of the bowels takes plac speedily, and the patient breaks out into a profuse perspiration. Thus the su ferings of the patient are materially abridged, while the duration of the loc disturbance is shortened; the latter being a very important point in the case c the eye." (p. 106.) (*Treatise on the Diseases of the Eye.*)

The means of promoting and maintaining power are principally die stimuli, and tonics. Mr. Tyrrell's remarks on tonics are good:

"The selection of the tonic should be regulated by the character of the d bility and the condition of the patient: thus when the cause of the debili has been loss of blood, or when the patient is very pallid, and has cold extr mities, and small, quick, but feeble pulse, the preparations of steel or zi

will, usually, prove most efficacious: when the patient has been exhausted by severe or protracted febrile disease, by diarrhœa, or by want of proper nourishment, the preparations of bark frequently promote the desired purpose better than the mineral preparations; in those of feeble power and scrofulous diathesis, or in such as have been suffering from specific taint, sarsaparilla, with minute doses of iodine or mercury, generally effect most good: and in such as evince unusual nervous susceptibility with feeble power, the addition of ammonia, valerian, castor, &c., to some of the tonics which I have mentioned, is, frequently, very serviceable." (p. xlii.)

In discussing "the correction of local morbid action by the influence of alterative medicines," Mr. Tyrrell considers at some length the use of mercury, which he justly characterizes as "perhaps, the most useful as well as the most powerful remedy in many of the most important ophthalmic diseases." Much, however, he remarks, depends on the mode of its administration. He dissents from the opinion that mercury acts most beneficially whilst the patient is kept upon a very abstemious diet, and lays it down as "a general rule, that whenever it is necessary to give mercury for the continuance of a few weeks, that it is essential to promote and maintain power at the same time." (p. xlv.)

Here it may be remarked that Mr. Tyrrell's doctrine about "power" strongly reminds us of the Brunonian theory. It appears to be equally founded on the supposition that the body is a sort of machine regulated by the mere abstraction or addition of stimuli. In urging so much as he does the promoting and maintaining of "power," Mr. Tyrrell seems not to reflect that the patient often cannot bear generous diet, because the stomach partakes of the general debility. Nor does he take into account, in laying down his indications and counter-indications for depletion at p. xxix., correct as they may be in the main, that cases do occur in which, notwithstanding the existence of debility, considerable depletion is the only means of arresting the destructive progress of inflammation.

Besides what has been just noticed, Mr. Tyrrell considers, in his introduction, several topics of great technical importance, such as the mode of examining the eye and the application of local remedies. On the latter topic we subjoin a few extracts and remarks.

Mr. Tyrrell bears testimony to the advantage of employing warmth and moisture by fomentations and steam. The latter, he says, "is perhaps preferable to the former when the affected organ is excessively tender." The mode of application recommended is to invert a large funnel over a jug containing the heated fluid, when "the steam which escapes from the narrow end of the tube can be received against the palpebræ, at a distance agreeable to the patient."

Lotions, Mr. Tyrrell recommends to be applied with a piece of soft linen to the surfaces of the eyelids gently closed; when sufficient of the fluid will penetrate the palpebral aperture, to effect the desired purpose. He properly objects (page xlviii.) "to the use of eyeglass, syringe, or other such method of applying lotions," telling us at the same time he has seen much mischief from them. We consider the eyeglass a most absurd implement. In applying drops to the eye, "the surgeon should depress, and slightly evert the inferior eyelid, by pressing on the integument of the cheek, and then pass the loaded brush between the lid and the surface of the globe." (p. xlix.) Though in this case the

fluid quickly passes over the surface of the conjunctiva of the lower eyelid and lower part of the eyeball, it does not affect the conjunctiva of the upper eyelid and upper part of the eyeball so completely as is desirable in many cases, because even the small quantity which, by the mode of application above recommended, makes its way between the upper eyelid and eyeball, is quickly washed away by the torrent of tears which follows the operation. We have, therefore, frequently found it of the greatest service either to evert the upper eyelid and pencil its inner surface, or, drawing it from its contact with the surface of the eyeball, allow the drops to flow freely underneath. We can also affirm from multiplied and comparative experience, that ointments introduced under the upper eyelid and diffused over the eye by rubbing the eyelid over the eyeball with the finger often proves of great service, though we infer that Mr. Tyrrell objects to this mode of employing ointments when he says (p. li.) that it is not "the intention of the application" that it should "penetrate in quantity between the palpebræ to the surface of the eye." And as a further objection to letting an ointment penetrate between the eyelids to the surface of the eye, he remarks that "the purest grease put into the eye produces irritation by acting as an extraneous matter." (p. li.) True; and so does even pure water, much more, stimulating drops. And if it be admissible and necessary to apply drops within the lower eyelid, it is so also to apply them within the upper eyelid; and if drops, ointments too: for the latter may be frequently substituted with advantage for the former.

Mr. Tyrrell approves of dry warmth, maintained by applying over the eye small bags lightly filled with chamomile flowers or bran or the farina of beans, and mixed or not, according to circumstances, with narcotics or aromatics. These vegetable bags, *Kräuterkissen*, are much used in Germany. Dry warmth thus applied, Mr. Tyrrell remarks, "affords great relief in some cases." (p. l.)

Mr. Tyrrell finds the outer surfaces of the eyelids or the cheek a little below the inferior palpebræ, the best places in which to apply leeches. He observes (page li.) that "they have very little effect in relieving the vessels of the conjunctiva, when placed upon the temple." He disapproves of the application of leeches to the conjunctiva itself, on the just ground that the point which has been bitten is left elevated and irregular, and thus irritates like a foreign body. We are not, however, prepared to agree with him that the same effect is produced by scarification of the conjunctiva in ordinary cases in a degree so great as to counterbalance its advantages, and therefore to justify the condemnation of it. When leeches cannot be procured, or when their use is forbidden, as when from idiosyncracy they excite inflammation of the skin, the angular vein, if well developed, may, Mr. Tyrrell says, sometimes be opened with advantage; otherwise he recommends cupping on the temple, or behind the ear, a little below the mastoid process.

As counter-irritants, Mr. Tyrrell generally employs "liniment of ammonia, mustard-plaster, tartar-emetic plaster, blister, and issue." Blisters are, according to Mr. Tyrrell's experience, more efficacious when frequently repeated than when kept open by irritating ointment. Amongst other objections to *issue or seton*, Mr. Tyrrell mentions that he has known instances of the indelible scar, which results being detri-

mental to the prospects of that class of persons destined for servitude by preventing them being accepted as servants. Mr. Tyrrell concludes his introduction with remarks on the relief obtained by position in acute diseases.

"I have often heard the sufferer observe, that he had been sitting up in bed the greater part of the night, and that he had been easy, or his pain had been very much lessened whilst he maintained such a position; but that his symptoms became aggravated as soon as he resumed the recumbent posture." (p. lviii.)

We believe the relief here mentioned is as much owing to the circumstance of the patient's head being cooler while sitting up in bed as to position. The painful heat attending conjunctivitis is in this way relieved; but above all, the circumorbital or temporal pain attending catarrho-rheumatic ophthalmia, iritis, &c., the peculiarity constantly remarked in regard to which is that it becomes worse as the patient gets warm in bed. Persons who have suffered from rheumatic toothach must have remarked the same thing, and, besides the sitting up in bed, how much a mouthful of cold water mitigates the pain.

In entering on the special part of our theme, we must premise that Mr. Tyrrell follows an anatomical arrangement, successively taking up the different parts of the eyeball from the conjunctiva inwards. By this plan the diseases of the ocular appendages would have come in last; but, "in order to prevent a great inequality in the size of the volumes," they are placed at the end of the first volume, "in preference to dividing the subject of Amaurosis, which must have been done had the arrangement originally intended been followed." The consideration of the diseases of each part is prefaced by a short sketch of its anatomy and physiology.

Anatomy of the Conjunctiva. The general disposition of the conjunctiva seems to us to be somewhat clumsily, and, even not very accurately described. The structure of the membrane, we are told (p. 4), appears to be chiefly cellular; and, although Mr. Tyrrell believes it to be properly classed with the mucous membranes, he says, "it does not exhibit in the natural state a villous appearance, which however becomes apparent in some states of disease." A villous, or rather papillary appearance on the palpebral conjunctiva, in the healthy state, may be perceived with the naked eye, and quite distinctly if a magnifying glass be used. Mr. Tyrrell passes over without notice the epithelium, with which the conjunctiva is invested.

The following statement sounds oddly, until we come to learn that Mr. Tyrrell means by *organization* the possession of blood-vessels, nerves, and absorbents: "The conjunctiva, in the natural state, exhibits but slight traces of its organization." (p. 4.) And the reason it does so, he informs us, is, that it is "chiefly supplied by serous vessels, very few being capable of circulating the red particles." Modern physiology assures us that "serous vessels" and vessels incapable "of circulating the red particles" are mere things of the imagination. The red globules follow one another perhaps in single or in double series in the conjunctival vessels, which, accordingly, are still invisible; but when more globules get alongside of each other then the vessels are seen.

Morbid Conditions of the Conjunctiva. In describing the diseases

of the conjunctiva, Mr. Tyrrell says (p. 9) he shall confine himself t "such divisions as lead to practical good, and render unnecessar technical minuteness;" and, in conformity with this resolve, he admi the following varieties of inflammation of the conjunctiva: to which may be remarked, in passing, he confines the term "Ophthalmia"— "Simple ophthalmia; pustular ophthalmia; catarrhal ophthalmia; puru lent ophthalmia; chronic ophthalmia, not as a result of acute diseas but originating in chronic form; strumous or scrofulous ophthalmia and exanthematous ophthalmia. The last two are modifications of mo of the above varieties."

Simple Acute Ophthalmia. The simplest example of this would b traumatic conjunctivitis; but as the inflammation of the conjunctiva excited by injury, may, according to the age and state of health of th patient, be puro-mucous, pustular, or phlyctenular, it is obvious tha simple acute ophthalmia, or, as it is otherwise designated, "simpl taraxis," or the "milder form of external ophthalmia," must frequentl vary in character. Accordingly, we find that Dr. Mackenzie does no give it a separate place in his list at all, but describes it as a milder forn of catarrhal ophthalmia; while Mr. Tyrrell has brought together, unde this head, examples of different kinds of disease, or, as he conside them, modifications of simple acute ophthalmia, depending on disorde of stomach, liver, skin, or uterus, or on general debility.

The following is the manner Mr. Tyrrell arranges his descriptions 1, the definition of the disease; 2, the synonymes; 3, the local symp toms (local subjective symptoms); 4, appearances (objective symptoms) 5, constitutional symptoms; 6, direct causes; 7, predisposing causes 8, persons liable to; 9, modifications; 10, treatment.

We highly approve of systematic arrangements of this kind; but w find that too strict an adherence to his plan has occasionally led M Tyrrell into difficulties. Thus, in describing the *local symptoms* of entro pium (vol. i. p. 438,) he successively enumerates "a constant state o irritation, as if from the presence of an extraneous body;" pain "espe cially experienced on moving the globe;" lacrymation, incapacity o directing the vision to any useful purpose and intolerance of bright ligh —as if these were at all pathognomic, and as if the nature of the disorde could not be at once recognized on inspection and the local symptom inferred.

Mr. Tyrrell lays down the treatment of simple acute ophthalmia ver judiciously, and well observes, that "it must be recollected that import ant functional derangement is very frequently a predisposing cause o ophthalmia, and rarely the direct cause; but, when once the disease i excited, that which has predisposed modifies it, and continues to have material influence over it." (p. 13). Fully appreciating the propriet of Mr. Tyrrell's treatment, we think that, in relating the illustrativ cases, he exhibits too great an inclination to the *post hoc propter hc* mode of reasoning; inasmuch as he appears to refer all to the last, an to lose sight of the influence of the treatment previously adopted.

Mr. Tyrrell describes very well (p. 23) those cases, so frequently me with in consultations, of obstinate ophthalmia resulting from too long continuance of depletion; or, perhaps, when this has been properl enough regulated, from too long a delay in giving strengthening di

and medicines. But, in dwelling so much on the rapidity of cure consequent on their exhibition, and attributing so much to them, he seems to forget that here, also, the way was prepared by the previous depletion. It may be remarked, that, in some cases, a cure of the local complaint is effected with an equal rapidity merely by one or two applications of some stimulant to the part. He relates several interesting cases of "simple ophthalmia," illustrating the utility of change of air and strengthening treatment in persons labouring under a peculiar condition of debility, independent of any important functional derangement (p. 28); in other words, the *Cachexia Londinensis* of Sir James Clark. In this state of health sometimes the eye, sometimes another organ may suffer; Mr. Tyrrell does not lose sight of this, but very properly collates the disease of the eye with analogous cases in general surgery, curable by the same means, such as "inflammation of the ligaments or synovial membrane of a joint, of the mucous membrane of the urethra, of the vagina, of the nose," &c. &c. (p. 35.)

Pustular Ophthalmia. Mr. Tyrrell mentions that the pustules in this disease occur on the sclerotic conjunctiva, at a short distance from the cornea, at the margin of the cornea, and on the cornea itself. Taking such an indiscriminate view of pustular ophthalmia, it is no wonder he should have sometimes met with rapid cure from the application of a solution of nitrate of silver, but more frequently failure. We believe it is not the remedy which is so "uncertain" as his diagnosis; for if he had used the nitrate of silver in solution or ointment, or indeed any other analogous application, as the red precipitate ointment, in those cases only in which the aphtha or pustule was on the sclerotic conjunctiva a tenth or twentieth of an inch from the margin of the cornea, he would seldom have found the remedy fail. When the pustule is on the cornea or its margin, the curability of the complaint is entirely different. The pustular ophthalmia, so called when the pustules are over the sclerotica, being, as Dr. Mackenzie well points out (p. 430), "less dangerous and more tractable than the phlyctenular (or scrofulous ophthalmia, properly so called,) into which, or into the scrofulo-catarrhal, it sometimes has a tendency to pass." Mr. Tyrrell has not only mixed up these in his description of "pustular ophthalmia," but also certain forms of exanthematous ophthalmia. Pustular ophthalmia, Mr. Tyrrell says, is occasionally combined with catarrho-rheumatic ophthalmia. It is true that, in catarrho-rheumatic ophthalmia, ulceration of the cornea is very apt to succeed a phlyctenula or onyx; but we do not find pustular ophthalmia, as commonly and properly understood, combined with catarrho-rheumatic. And it is to be remarked, that the subjects of the former affection are for the most part children or young adults, whereas the subjects of the latter are generally old persons. We cannot but consider that, in his chapter on pustular ophthalmia, our author makes one of the most simple matters in the whole range of the diseases of the eye confused and difficult.

Catarrhal Ophthalmia. What Mr. Tyrrell describes under this head is the severer form of the disease, as given by Dr. Mackenzie. In describing the evening exacerbation, Mr. Tyrrell mentions that the patient "usually experiences *headach and pains in the orbit.*" Orbital pain, aggravated at night, is a very important symptom in all inflammations

of the eye; because, as we have already mentioned, its existence is in general a very good indication of the necessity for venesection. Orbital pain should therefore always be carefully distinguished from mere headach, and especially from that pain and sense of weight about the frontal sinuses and antrum, met with in severe cases of catarrh. It is this kind of headach which may be found in catarrhal ophthalmia, not pain around the orbit or in the temples. Where the latter pain exists, it will be found that to the inflammation of the conjunctiva there is joined inflammation of the sclerotica or iris.

It is true that "the catarrhal disease is usually very tractable, and does not require severe remedies to subdue it" (p. 48); but it ought to be borne in mind that it is not so always, nor everywhere. It is a disease which ought to be got speedily under, on account of its great tendency to leave the conjunctiva in a thickened, if not granular state; a state from which it does not readily recover. We believe that, in the severer cases, well-timed abstraction of blood by leeches or venesection, and local stimulants, will not fail the practitioner. We agree with Mr. Tyrrell in his objections (p. 46) to the employment of cold in catarrhal ophthalmia, but we cannot subscribe to his indiscriminate objections to strong stimulating applications, which, judiciously employed, we consider of the greatest value.

Purulent Ophthalmia. Mr. Tyrrell considers Egyptian ophthalmia, gonorrhœal ophthalmia, and ophthalmia neonatorum, as "virtually the same in all instances; only modified by circumstances of age, climate, mode of origin, &c." And although he describes, under different heads, the purulent ophthalmia of the adult, and that which occurs in the infant, it is "solely because, from the conditions of the patients, some difference in treatment is requisite." This is, after all, just doing what other authors do.

Mr. Tyrrell's plan of treating chemosis by incisions in the conjunctiva, radiating from the cornea, forms the leading feature of this chapter; but, as we touched upon this subject in our last Number, we shall not here return to it.

Purulent Ophthalmia in the Infant. There is nothing in this chapter calling for particular notice. It may merely be mentioned that Mr. Tyrrell believes the causes of the diseases to be threefold: First, and most commonly, leucorrhœa in the mother; secondly, less frequently, "a purulent secretion from the urethra of the mother (gonorrhœa);" thirdly, exposure to cold and damp. He thus omits one of the most frequent causes; viz., the intrusion of soap, spirits, &c. into the infant's eyes.

Chronic Inflammation of the Conjunctiva, following Purulent and Catarrhal Ophthalmia. In this chapter, what is called *granular conjunctiva*, or the *granulated eyelid* is considered. At p. 122, Mr. Tyrrell mentions the sclerotic and even the corneal conjunctiva as presenting the granular state; but this is an erroneous view of the matter. The appearances to which Mr. Tyrrell alludes, as being presented by the ocular conjunctiva, are not of the same nature; for the anatomical reason, that that part of the conjunctiva does not possess a papillary or villous structure, similar to that which, in the palpebral conjunctiva, forms the peculiar seat of the granular prominences.

Dr. Mackenzie correctly says that the ocular conjunctiva "may present a red and swollen appearance, but is not really granular" (p. 553), that is, presenting hypertrophied papillæ. The granular appearance sometimes observed on the cornea is owing rather to real granulations.

Mr. Tyrrell's account of the varieties of granulations on the palpebral conjunctiva is short, but sufficiently comprehensive and correct for practical purposes. "The villi are red, firm, and elastic, like the granulations of a healthy ulcer; or they are soft and flaccid: or small, hard, and of a light colour. Where the first or last varieties exist, the power of the patient is seldom much impaired; but when the second is found, the constitutional vigour of the patient is generally feeble." (p. 131.)

In our last Number we slightly touched upon the circumstance, that because the sclerotic conjunctiva may have become free from redness, it is not to be supposed, as Dr. Mackenzie remarks (p. 376), that the inflammation is completely subdued, for the palpebral conjunctiva may still remain in a morbid state. "The inside of the eyelids, and especially of the upper, ought daily to be inspected." This point is very well stated by Mr. Tyrrell:

"The heading of the section shows that I consider the disease which I have described as the result of purulent and catarrhal ophthalmia; but I deem it important to explain how, and why it occurs. I have shown that these diseases commence in the palpebral division of the conjunctiva, and from thence extend to the ocular portion. They disappear in the contrary order; leaving, first, the ocular part of the membrane, or that in which they appear last, and linger in the palpebral portion of the tunic, or that in which they first appeared; and, in this division of the membrane, the morbid action may remain in so trifling a degree as to escape the observation of the careless practitioner, who may be satisfied with the perfect restoration of vision, independently of slight occasional interruption, from a collection of superabundant secretion. In order to prevent the occurrence of the chronic affection, the palpebral conjunctiva should be carefully examined when the acute disease appears to have been completely subdued, and if the membrane of the eyelid has not perfectly recovered its natural aspect, the remedies should be continued until all morbid appearance be subdued. The examination should extend to the conjunctiva of both eyelids." (p. 124.)

With reference to the *treatment*, Mr. Tyrrell recognizes two stages of the diseases. The first stage he "would limit to the period before which the cornea becomes nebulous and vascular, or that in which the palpebral conjunctiva only presents a morbid aspect." In the second stage, the disease has produced a nebulous and vascular state of the cornea. The first stage, Mr. Tyrrell says, "can usually be subdued with very moderate care." The second "requires much more care and patience for its relief." In the treatment of the first stage, Mr. Tyrrell properly recommends good diet, tonics, and protection from the changes of weather, &c. Locally, he employs counter-irritation and stimulants. In regard to the use of the latter, we have found from much experience that it is best that they should be pretty strong, and applied, whether ointment or liquid, directly to the affected surface once a day; bedtime is the most convenient: we have also found it advantageous occasionally to intermit their use.

Mr. Tyrrell says (p. 130), "The treatment should be persevered in until the conjunctiva of the eyelids has regained its *natural appearance*, which it will not do till some time after the morbid secretions have dis-

appeared." We wish we could say our experience bears out Mr. Tyrrel in this statement; we fear that it will be found that, even under the mos favorable circumstances, and after the mildest form of disease, th *natural appearance* of the conjunctiva is not so readily regained. I the second stage, Mr. Tyrrell recommends the same general remedie as the first; but the local means proposed for it, he says, "will hardl be found sufficient." He thus expresses the principle of the local treat ment he has found most effective: "Whenever there was evidence o congestion, the vessels were relieved by the application of the leech and when this congestion had subsided, the contraction of the vessel was promoted, and the action of the absorbents excited, by application having astringent and stimulating proportions." (p. 134.) He has occa sionally substituted scarification for the leech, but does not consider th former much preferable to the latter, "excepting that it is more rapid i its effects, and is not likely to create extravasation or swelling, which th leech is apt to do." It has too often happened, that, in the local treat ment, caustics have been sadly abused. On the whole, the plan c treating granular conjunctiva, recommended by Mr. Tyrrell, is what i now most resorted to, or rather what is now considered allowable.

Simple Chronic Ophthalmia. The disease which Mr. Tyrrell desig nates by this name affects chiefly adults, whose occupation expose them to cold and damp, especially if addicted to intemperance. Th disease, again, which he describes at p. 489, under the head of "tine ciliaris," "usually occurs in children, and very rarely commences afte the age of puberty. I have known it to arise in the adult." (p. 491. Dr. Mackenzie appears to comprehend both under the name of "inflam mation of the edges of the eyelids, or ophthalmia tarsi;" though his de scription bears most upon the disease occurring in youth. In a practica point of view, the separate consideration of the two forms of disease is t be preferred. This section of Mr. Tyrrell's work is excellent.

Scrofulous Ophthalmia. Mr. Tyrrell does not consider that there i any inflammation of the conjunctiva peculiar to scrofulous persons, bu that all are occasionally modified by the scrofulous constitution, an suggests that the phlyctenular affections of the mucous membrane appea to be peculiarly scrofulous, merely on account of their greater frequency The general appearances or physiognomy of the disease is very well deli neated, only the symptoms are said to be aggravated at night, wherea the peculiarity of scrofulous ophthalmia is, that the symptoms remit to wards evening. Our author gives a good account of the disordered stat of health with which scrofulous ophthalmia is so generally accompanied especially of the disorder of the digestive organs, already so well describe by the late Dr. T. J. Todd in the Cyclopædia of Medicine, under th name of strumous dyspepsia.

Under the head of *Causes*, Mr. Tyrrell very well remarks:

> "The conjunctival affection may, and probably does, in a large majority c these cases, result from the ordinary causes, which I have enumerated under th subjects of simple and pustular ophthalmia; but, when once induced, it become influenced, promoted, and sustained by some functional disorder, combine with the peculiarity of constitution." (p 155.)

Mr. Tyrrell very properly places his main reliance in the cure of thi disease on constitutional treatment; but he says nothing of emetics

which generally prove so valuable at the commencement. The repeated application of small blisters, simple tepid water, or decoction of poppy heads or chamomile flowers as an eye-water, and occasionally a few leeches to the eyelids, make up his local treatment. Concerning the application of cold, he remarks, that it is sometimes grateful to the sensation when first used, but seldom fails to augment the suffering if it be continued. Mr. Tyrrell does not notice, among his remedies for scrofulous ophthalmia, conium, hyosciamus, and belladonna; which, especially the latter, are of so much service in subduing the intolerance of light.

Chronic Scrofulous Ophthalmia. The disease described under this name appears to be scrofulo-catarrhal ophthalmia, become chronic and fixed in the conjunctiva corneæ. The treatment recommended by Mr. Tyrrell contrasts advantageously with the frittering and exhausting—the indefinite and inefficient treatment too often employed; still, we think, he makes rather too much of it: harping about "power," "function," and the like. We consider general treatment of paramount importance; but our own experience convinces us that in a chronic disease of the conjunctiva corneæ, such as this, some local stimulant, properly applied, and that even two or three times only, is capable of producing in a few days, as striking an effect as that mentioned by Mr. Tyrrell, arising from the restoration of uterine function, or months of general treatment alone. Of course, we do not confine ourselves to the mere local treatment, but endeavour to improve the health, and thus prevent the tendency to relapse.

There being nothing to arrest attention in the chapter on "exanthematous ophthalmia," we proceed to notice some of the organic diseases of the conjunctiva.

Mr. Tyrrell successively considers "Excrescences of the Conjunctiva," "Pterygium," which, he says, has generally a *conical* form; fræna, by which is meant symblepharon, though this or any other synonyme is not given. In regard to fræna, Mr. T. tells us that he is now convinced, after repeated trials, that operations for their prevention or removal are worse than useless. He describes, under the name of "Adipose tumours of the conjunctiva," what are commonly called *pingueculæ* or *pterygia pinguia*, though here Mr. Tyrrell again omits his practice of giving synonymes. "The tumour," he says, "consists of a deposit of fatty or adipose matter." Weller analyzed the matter, and assures us that it is not fatty, but rather a substance of a nature betwixt albumen and gelatine. Has Mr. Tyrrell ascertained that they are really fatty tumours, to warrant him to lay aside the conventional, though erroneous Latin names, and, by rendering them into English, to give them, as it were, a more literal force?

In speaking of injury of the eye from strong acids, Mr. Tyrrell fortunately does not manifest much individual knowledge of the very destructive effects, immediate or remote, sometimes observed. "After sulphuric acid," says Dr. Mackenzie (p. 226), "has been thrown into the eyes (a piece of diabolical malice), the effects of which I have repeatedly had occasion to witness, the conjunctiva almost appears scarred, being white, soft, and swollen. It afterwards peels off,* while the cornea

* As this is passing through the press, we observe that, in a communication made to

rapidly becoming disorganized by infiltration of pus, ulceration, and sometimes sloughing, a raw surface is left both on the ball and on the inside of the lids, ready to unite and close the eye by an incurable and almost total symblepharon.....It is worthy of remark, that dangerous symptoms, as onyx and iritis, are apt to occur, in such cases, week from the receipt of the injury, and after the immediate effects have subsided."

Nothing very novel appears under the head of local injuries to the eye That this organ can bear a great deal is proved by the strong stimulant frequently applied to it by way of treatment. Dr. Mackenzie quote from Ammon's Zeitschrift the case of a man who had a drop of melted pitch fall directly on the cornea, where it stuck so fast that it could no be loosened until softened by the application of olive oil, when it quitted the eye without leaving any visible injury. And Mr. Tyrrell informs us that he has "several times taken out, from beneath the palpebræ, portions of lead weighing many grains, which had evidently entered the eye in a fluid state, as they have been perfectly moulded to the surfaces of the globe and palpebræ, yet the conjunctiva has remained free from all injury, except slight inflammatory action." (p. 204.)

Diseases of the Cornea. The anatomical sketch which precedes the consideration of these is not characterized by much accuracy. Passing it over, and passing over for the present the diseases described by Mr. Tyrrell under the name of the "acute form of inflammation of the cornea, terminating in suppuration," when occurring idiopathically, and "inflammation of the cornea with vesication," as they appear to be identical with the diseases usually grouped under the name of catarrho-rheumatic ophthalmia, we come to Mr. Tyrrell's chapter on "Inflammation of the cornea, with deposition of earthy matter." The expressions employed in the description lead us to suppose that the earthy depositions were on the surface of the cornea, but it is not mentioned whether there was any ulceration underneath. The depositions he has met with, Mr. Tyrrell thinks, are the same as those described by "several eminent ophthalmic surgeons," as deposits from eye-waters containing acetate of lead. Deposits like wet chalk on ulcers of the cornea were described and represented many years ago by Mr. Wardrop in his Morbid Anatomy of the Eye, and Beer had mentioned his having observed that the use of lead lotions rendered the cornea opaque; but it was Dr. Jacob, we believe, who first particularly called attention to the circumstance, that if a solution of acetate of lead be applied to any part of the conjunctiva in an excoriated or ulcerated state, the acetate is decomposed, and a white precipitate is deposited, which adheres tenaciously to the conjunctiva, and, as the membrane heals, becomes fixed in the cicatrice. Dr. Jacob describes and delineates depositions in the palpebral conjunctiva, as well as on the cornea. He says (Dublin Hospital Reports, vol. v. p. 370,) "The opacity appears to be produced at once by a single application. I have seen it the day after a drop of solution of acetate of lead had been put into the eye by

the British Association at Glasgow, the epidermic part of the conjunctiva corneæ, which is rendered opaque and peels off, is erroneously supposed to be a false membrane. The author, Dr. R. D. Thomson, does not seem to be aware of the difference in the chemical composition of the epidermic layer, and the proper substance of the cornea; and consequently that the reagents which render opaque the former have not the same effect on the latter.

mistake." He further says, "I do not think that I can state positively the precise condition of the ulcer which causes this deposit, but it does not take place in all cases where the acetate of lead is used." The most marked examples we remember to have seen were in cases of ulcer in catarrho-rheumatic ophthalmia. Mr. Tyrrell considers that the depositions in the cases observed by him were not from any metallic salt, though he does not deny that opacity may occasionally result from the adherence of an insoluble precipitate on an ulcerated surface. He adds, however, "I must confess that I have never seen a satisfactory case of the kind." Be this as it may, it is quite clear that the grounds on which Mr. Tyrrell would establish "Inflammation of the cornea, with deposition of earthy matter," as a distinct form of disease, are quite unsatisfactory.

Corneitis is the synonyme given by Mr. Tyrrell to "Inflammation of the cornea, with effusion of adhesive matter or fibrine." In the treatment of this affection, Mr. Tyrrel recommends mercury; which, considering that the "disease most frequently occurs in young persons of scrofulous habit and feeble power," must be cautiously administered. Besides the mercurial, some tonic, selected according to the circumstances already mentioned in regard to the exhibition of tonics generally, is judiciously recommended.

Inflammation of the Cornea, terminating in Suppuration. Mr. Tyrrell recognizes two forms of this, the acute and chronic. As a preliminary to his description of these, he enters into a learned disquisition on the meaning of the terms *Onyx* and *Hypopyon*, and quotes the authority of St. Ives to show that we now-a-days most perversely use the one word for the other.

Mr. Tyrrell's "chronic suppuration of the cornea" is the effect of inflammation occurring in exhausted states of the system. He mentions that he has seen it most frequently in children who have suffered from febrile disease, as measles, scarlatina, &c., and also in old persons after injury. We have seen it in a child in a state of great exhaustion after typhus fever, and we have seen it occur in both eyes in an old man who was subjected to the operation for cataract.

Ulcers of the Cornea. Of these Mr. Tyrrell describes three principal forms—the *healthy*, the *inflammatory*, and the *indolent*. The character of the healthy ulcer is, that its surface and circumference are hazy or opaque; the inflammatory ulcer may be recognized by the appearance of small vessels carrying red blood, in addition to the haziness or opacity observed in the healthy ulcer; the indolent ulcer appears clear and transparent, "merely a small dent presenting itself on a close inspection of the surface, as if a small portion had been cleanly excised by some sharp instrument." Mr. Tyrrell's healthy ulcer might, with perhaps more propriety, be called the healing ulcer, because it presents its peculiar characteristics as the inflammation which produced it is subsiding; and the inflammatory ulcer might be called active ulceration. When the inflammation is checked, the progress of the ulcer is in general also checked; the vessels running into it gradually diminish and retract, and in proportion to this the ulcer fills up and cicatrizes.

It is incorrect to say, in an unqualified manner, that "in the inflammatory ulcer the necessity for depletory measures is clearly indicated, and these must be steadily pursued until the visible vascularity of the

ulcer is subdued." The state of the ophthalmia in general, of which the ulcer is an appearance, must alone regulate the depletion. Even after the ophthalmia is checked, the process of the disappearance of the vessels takes up some time. When it has commenced it is unnecessary to push it by depletion, this not being otherwise required. On the contrary, notwithstanding what Mr. Tyrrell says, we have seen the well-timed employment of a stimulating application hasten the retraction of the vessels and the cicatrization of the ulcer.

In speaking of the discoloration of the conjunctiva produced by the prolonged employment of nitrate of silver solution, Mr. Tyrrell says (p. 266), " it always commences at the lower part, where the membrane is reflected from the lid to the globe, and from this it gradually extends to the ocular and palpebral surfaces." The cause of this is, that the solution is always applied first to the lower part, and consequently in its most concentrated state, and is diffused in a diluted form over the rest of the conjunctiva. The wash should therefore be always put into the eye in such a manner, that it may, as we have already pointed out, be equally diffused under the upper and lower eyelids.

Staphyloma Corneæ. Mr. Tyrrell adopts the pathological views regarding the formation of staphyloma corneæ, at least in all their essential particulars, which were, we believe, first propounded by Mr. Wharton Jones. An abstract of Mr. Jones's paper, first published in the Medical Gazette, is given in our Fifth Volume, p. 612. To this, however, Mr. Tyrrell does not refer, and makes use of expressions in his description which would lead to the supposition, that, if the views alluded to have not hitherto been those universally received, they are his own.

" When the destruction of the cornea," says Mr. Tyrrell (p. 270), " has been so extensive as to allow of the protrusion of the iris, &c. which I have described as giving rise to the staphyloma, much may be done on the part of the surgeon to check the protrusion..... He should endeavour," Mr. Tyrrell says, " to excite the deposition of fibrine by the use of local stimuli, as the solutions of sulphate of zinc or nitrate of silver." This is treatment little likely to effect the desired object, and what was not to have been expected from one who appears in other cases so chary of local stimulants.

Escharotics have been principally used for reducing the size of partial staphylomata, and butter of antimony has been that most recommended. The apex of the staphylomatous projection has been the place to which it was usually applied. The following is the mode of treating partial staphyloma, practised by Mr. Tyrrell.

" Partial staphyloma," says Mr. Tyrrell, p. 273, " often exists under circumstances, which render its reduction a matter of importance or anxiety; as when there is sufficient of the healthy cornea left to enable the surgeon to form an artificial pupil beneath, or when the projection gives rise to much irritation or deformity. I have," Mr. Tyrrell says, " succeeded, in several instances, in effecting a reduction of partial staphyloma, by the careful application of nitrate of silver, or hydrate of potash, in substance. I have applied the escharotic first at the base of the projection, taking care not to injure the remaining sound cornea—the effect has been the separation of a small slough; but previous to such separation, a deposit of fibrine beneath, by which the deeper part has become more solid and strengthened. After the part has recovered from one application I have made a second close to but not upon the same spot, and nearer to

the summit of the projection. Again and again I have repeated this operation, acting upon the more prominent part, until a considerable or perfect reduction of the staphyloma has been accomplished; and this has enabled me, in a few cases, to form an artificial pupil, subsequently, of much more utility to the patient. I prefer the hydrate of potash, unless the projection be very small; for its use is followed by a much larger deposit of fibrine than results from the nitrate of silver." (p. 273.)

Conical Cornea. Mr. Tyrrell has found, contrary to the experience of most others, the local use of stimuli retard, if not prevent, further increase of conical cornea; but he has never known any diminution occur. An operation for this disease is noticed by Mr. Middlemore (vol. i. p. 538,) in the following terms: "Where the point of the conical cornea has become opaque, and vision is thereby rendered much more obscure than it would otherwise be, it has been proposed to make an artificial pupil near to the margin of the cornea, which it is said will have two important advantages, namely, removing the pupil from the opaque part of the cornea, and allowing the light to be transmitted through the least convex part of that membrane" This operation, Mr. Tyrrell tells us, he has performed with more or less advantage in seven or eight cases.

Anatomy of the Sclerotic. Mr. Tyrrell here gives a somewhat inaccurate estimation of the relative extent of the cornea and sclerotica. We believe that, in a section of the eye, the sclerotic forms nearer $\frac{5}{6}$ths, and the cornea $\frac{1}{6}$th of the circumference of the exterior proper tunic of the globe than about $\frac{4}{5}$ths and $\frac{1}{5}$th respectively. Of course the cornea forms a much less part, and the sclerotica a much greater part of the sphere. As to the accuracy of the assertion, that "the part of the sclerotic which immediately overlaps the cornea receives a separate and independent supply from its conjunctival covering," we may well entertain some doubt, seeing that this part is at the most $\frac{1}{20}$th of an inch broad and $\frac{1}{80}$th of an inch in mean thickness, and that it is placed at the confluence of the arteries of the conjunctiva and anterior ciliaries.

There is nothing in Mr. Tyrrell's account of the Diseases of the Sclerotic requiring particular notice. Mr. Tyrrell describes the membrane lining the posterior chamber as the same with that lining the anterior chamber. This opinion, we think, might be justly objected to, as the two membranes have not an origin in common, and as they do not manifest exactly the same morbid phenomena. The membrane lining the posterior chamber much more readily throws out lymph and forms adhesions than that lining the anterior. Dr. von Ammon, as we mentioned in a former Number, has traced the membrane of the anterior chamber to the margin of the pupil, but never into the posterior chamber. Dr. von Ammon is also of opinion that the membrane investing the anterior surface of the iris secretes the aqueous humour, and that the lining of the cornea absorbs it. In regard to the membrane investing the posterior surface of the iris, he thinks it secretes the pigment. We would remark, however, that it is between the membrane lining the posterior chamber and the substance of the iris that the pigment of the uvea is deposited, and it is doubtless secreted by its own cells in the same way as the pigment of the membrane on the inner surface of the choroid. That the membrane of the posterior chamber secretes aqueous humour is proved by what takes places in staphyloma, in which that part of the

membrane of the anterior chamber lining the cornea is gone, and tha investing the surface of the iris is covered over with the tissue o cicatrice.

Mr. Tyrrell appears to include in his account of "inflammation o the aqueous membrane with deposition of fibrine, or aquo-capsulitis;" the iritis serosa of authors. Under iritis he appears to admit only iriti *parenchymatosa*. In his description of "Inflammation of the aqueou membrane producing effusion of pus without ulceration," there is n evidence adduced that the aqueous membrane is its primary seat. In deed, Mr. Tyrrell says that it is "always in connexion with sever inflammation of many other textures of the globe." Mr. Tyrrell neglect to give a synonyme, but the disease appears to be what is usually calle inflammation of the whole eye, or *panophthalmitis*.

For the affection named "Inflammation and ulceration of the aqueou membrane," Mr. Tyrrell again gives us no synonyme, and leaves us t determine whether this be an undescribed form of disease, or merely a occasional occurrence in various inflammatory affections of the eye. I appears to us to come nearest to abscess of the cornea bursting into th anterior chamber, and forming what has been called spurious hypopyon Although Mr. Tyrrell gives no synonyme of his "Inflammation of th aqueous membrane, with increased secretion," it is evident that th disease he here describes is the inflammation of the capsule of th aqueous humour; to which Mr. Wardrop first called attention in th fourth volume of the Medico-Chirurgical Transactions, as being so re markably benefited by the evacuation of the aqueous humour. Th case related by Mr. Tyrrell at p. 327, is what is usually placed in th category of dropsy of the aqueous chambers. We think the break ing up of inflammation of the aqueous membrane into so many divisions as Mr. Tyrrell has done, was scarcely called for by practical utility.

The "Anatomy of the Iris" is the best of Mr. Tyrrell's sketches. I is, however, not free from defects and omissions. He makes no mentio of the *sinus circularis iridis* situated on the inner surface of the scle rotica where it joins the cornea. Professor Schlemm, of Berlin, wa the first to point out this sinus, though Professor Arnold, of Zurich, ha attempted to show that Hovius described it. This, however, is an erro on the part of Professor Arnold, as what Hovius described under th name of "circulus venosus" in the eye of the ox, &c., is composed of th trunks of the choroideal veins, *vasa vorticosa*, which by their anastomose form a circular vessel or sinus in the substance of the *choroid* abou one fifth of an inch behind the canal in question. There is no suc "circulus venosus" in the human eye, as the *vasa vorticosa* do not joi together until where these trunks are about to pass out through the scle rotica.

Inflammation of the Iris. "Iritis," it is well remarked by Dr. Vo Ammon, "morbus est summi quidem momenti et totius ophthalmo-pa thologiæ cardo; uti enim iris in medio bulbo sita est et cum gravissimi hujus organi partibus intimum habet commercium, sic doctrina de iritid centrum occupat totius artis ophthalmiatricæ." (p. 6.) In our Nineteent Number, in mentioning Dr. Von Ammon's divisions and subdivisions c iritis, though we remarked that there was manifested in them too muc

wire-drawing, and though in some of his forms of iritis the disease appears to be seated as much in other parts of the eye as the iris, we would by no means have it inferred that we differ from him in the following statement: "Pro varia vero inflammationis sede, gradu et natura, pro diversa causarum ratione, pro varia ægroti ætate, sexu et constitutione multiplices oriuntur iritidis differentiæ, quas singulas distinguere medicus potest et debet. Longissimam constituunt mutationum seriem varii gradus morbi, quæ inter symphoresin iridis simplicem et iritidem acutam jacent." (p. 8.) Mr. Tyrrell, however, though he has made what we consider some unnecessary divisions in the diseases of other parts (e. g. of the aqueous membrane), is of a very different opinion in regard to the affection under notice:

"Most modern authors," he says, "describe several varieties of this disease; but I deem such division of the subject to be of no practical utility, with the exception of a division into acute and chronic, which I shall therefore adopt. In fact I do not admit of the distinctions which have been attempted; but I consider inflammation of the iris to be the same, whatever may be its mode of origin; it may and does vary in intensity and in rapidity of progress; and these circumstances are depending more upon the condition of the constitutional power of the party affected than upon the mode of origin; a specific taint, by its influence upon the system, no doubt, in many cases modifies the local disease." (p. 337.)

We have already mentioned that Mr. Tyrrell appears to include the iritis serosa of authors in his inflammation of the aqueous membrane with deposition of fibrine, whilst Von Ammon, in describing his iritis serosa, says, "this inflammation of the serous investment of the anterior surface of the iris readily passes into keratitis serosa." This author further says his iritis serosa rheumatica may pass into kerato-iritis rheumatica and also into iritis parenchymatosa. Under the head of iritis, Mr. Tyrrell appears to describe iritis parenchymatosa only. We shall, however, find, by and bye, that what Von Ammon calls uveitis also finds a place; so that Mr. Tyrrell does not in reality differ so much as he would appear to do.

Treatment. We have already had occasion to mention how sparing Mr. Tyrrell is in the recommendation of bloodletting in the diseases of the eye, and how liberal he is in the employment of whatever is calculated to keep up the strength, as if the subjects of diseases of the eye were always peculiarly weak, and as if abstraction of blood, notwithstanding some degree of general weakness, were not occasionally called for by the importance of the organ at stake. Mr. Tyrrell very properly praises the mercurial treatment in iritis; but in contrasting it with simple depletion, he says nothing of the combination of the two kinds. In support of his views, he adduces cases in which depletion had already been employed, and that in an indiscriminate manner, before he commenced his mercurial treatment, which proved so efficacious. But this is hardly a fair way of discussing the subject. The fact is, there may be a few cases of iritis in which abstraction of blood is not required or may be dispensed with; but in most it will promote, in a very striking manner, the beneficial action of the mercury, frequently eliciting the action when this does not manifest itself, and render less of the mineral necessary. Abstraction of blood, followed up by a diaphoretic dose of calomel and opium or Dover's powder, relieves the distressing circumorbital pain. On this Mr. Tyrrell dwells very little, and for its relief is contented with

recommending inunction of mercurial ointment with opium, which to often proves a very imperfect anodyne, no better, as a patient, himself medical man, lately remarked to us, than so much water.

Traumatic Iritis. Mr. Tyrrell occupies most of his chapter on "Iriti from injury" with two cases, in which a cyst, of the size of a small pea and glistening like tendon, had formed in connexion with the iris. Th subject of the first case was a boy, an apprentice to a blacksmith. Th cyst appeared some months after severe inflammation, produced by small particle of hot iron, which penetrated the cornea and lodged in th iris. A fair and beautiful girl, about nine years of age, was the subjec of the other case, in which the disease occurred a few months after in flammation brought on by the eye being struck with some bearded corn The first case was treated by Mr. Scott, the other by Mr. Tyrrell. I both the cyst was removed. Mr. Scott's patient, Mr. Tyrrell believes "did not retain useful vision afterwards." (p. 369.) In Mr. Tyrrell own case iritis came on, to which was soon joined sympathetic iritis i the other eye. After a long persistence in the mercurial treatment, th inflammation was stopped; the eye secondarily affected recovered; bu that on which the operation had been performed retained the power t perceive large objects only. (p. 372.)

The opening of the cornea, in these cases, the drawing out the cy and part of the iris to which it was attached, and then the cutting off portion of the iris and the cyst with a pair of fine scissors; and all thi in scrofulous children whose eyes had but recently suffered from inflam mation, appears to us, to say the least, treatment of very doubtful pro priety, and which, we think, contrasts rather unfavorably with tha adopted by Dr. Mackenzie in the following somewhat similar case:

"A lady was affected with considerable pain in one of her eyes, which pre sented the appearance of a small vesicle pushing into the anterior chamber fro under the ciliary margin of the iris behind the lower edge of the cornea. Th vesicle gradually increased, separating the iris more and more from the choroi and the pain became severe. I punctured the vesicle or encysted tumour wi the iris knife through the cornea. A minute quantity of fluid was discharge from the cyst, which immediately contracted so much that it was no long visible. The pain was removed. The wound made in the cyst healed, it fill again with fluid, and again appeared in its former situation, but larger than be fore. I punctured it a second and a third time at intervals of six and eig weeks. After the third puncture it did not fill again. The iris returned to i natural place; the pain ceased entirely; and vision was preserved." (Treatis p. 604.)

Under the head of Compound Ophthalmiæ, Mr. Tyrrell instances tw kinds which are common, and which require special consideration, namel inflammation of the conjunctiva and sclerotic or catarrho-rheumati ophthalmia, and inflammation of the sclerotic and iris.

We have already mentioned that we regard the affections described b Mr. Tyrrell in a former part of his work, under the name of "acute in flammation of the cornea with suppuration" and "inflammation of th cornea with vesication," as merely forms of catarrho-rheumatic ophtha mia. What Mr. Tyrrell describes under this name is partly the "go norrhœal inflammation of the external tunics and iris of Mr. Lawrenc and partly a milder form of common catarrho-rheumatic ophthalmi The only peculiarity in his description is, that he says (p. 379) that, i

what he calls catarrho-rheumatic ophthalmia, "it is very rare to find mischief to the cornea by ulceration or sloughing." For this, as regards ulceration, we can account only by supposing that Mr. Tyrrell refers the case, when ulceration, &c. occurs in the cornea, to one of the forms of disease above mentioned, which he considers under the head of inflammation of the cornea, but which we think are advantageously grouped together by authors under the head of catarrho-rheumatic ophthalmia.

We have in a former Number remarked that the names of the inflammations of the eye are in a great degree conventional. Let it not, therefore, be said that because affection of the cornea forms so marked a symptom of what is described by authors under the name of catarrho-rheumatic ophthalmia, we should with Mr. Tyrrell class the greater number of the cases under the head of diseases of the cornea. We say the greater number, because, as Dr. Mackenzie remarks (p. 443), "there is no ophthalmia, to which adults are exposed, in which ulcer of the cornea and onyx are so frequent as in the catarrho-rheumatic."

If we admitted Mr. Tyrrell's reasons for viewing the above forms of disease as essentially seated in the cornea to be valid, it would be equally correct to consider the inflammations of the conjunctiva we have already passed in review as more properly affections of the cornea, for, to use the words of Dr. Mackenzie, "The cornea is liable to suffer, more or less directly, in most of the ophthalmiæ already considered. Its superficial layer is apt to become vascular and nebulous in the chronic stage of the puro-mucous ophthalmiæ; it is the seat of phlyctenulæ in scrofulous ophthalmia; and is often extensively destroyed by ulceration, in consequence of injuries and other causes. The spongy or lamellar substance of the cornea becomes infiltrated with pus, in catarrho-rheumatic ophthalmia; in certain cases it is perforated, layer after layer, by ulceration; and is sometimes almost entirely destroyed by suppuration or by sloughing." (p. 447.)

The type of Mr. Tyrrell's "*acute suppuration of the cornea*" is a traumatic inflammation—that which is occasionally met with as the result of abrasion of the surface of the cornea produced by scratches, as from the finger nails, but especially from the ears of corn, an accident to which reapers are much exposed. Traumatic inflammation in any of the textures of the eye imitates, so to speak, that which occurs in the same texture excited without any evident mechanical or chemical injury. The traumatic disease under consideration is very dangerous, and unless timely and actively treated by depletion and mercurialization, the internal textures of the eye become involved and the cornea infiltrated with pus. "The symptoms altogether," Dr. Mackenzie remarks, "bear a close resemblance to those of catarrho-rheumatic ophthalmia." (p. 223.) It is to be remembered, however, that the membrane of the aqueous humour and the iris very readily participate in the disease.

Mr. Tyrrell defines his "inflammation of the cornea with vesication" as "a partial separation of the conjunctival layer of the cornea by effusion of serous fluid." (p. 241.) Mr. Tyrrell thus views what is merely an appearance in some cases of catarrho-rheumatic ophthalmia as a separate disease. The following description of the ulceration which takes place in catarrho-rheumatic ophthalmia by Mackenzie, it will be seen, refers to the same thing as Mr. Tyrrell's *vesication*: "The ulcer

is generally peculiar, in so far as it is apt to spread over the surface and rarely penetrates deeply into the substance of the cornea. It seems the result of exfoliation of a considerable portion of the corneal conjunctiva. I have seen such a portion loose and raised up apparently by the intervention of a fluid. It must be this appearance which Beer describes as a phlyctenula; but it is more extensive than a phlyctenula, and is neither so circular nor so circumscribed." (p. 443.)

The other appearances enumerated show further that the disease Mr. Tyrrell here describes is just one of the forms commonly included under catarrho-rheumatic ophthalmia; thus there is evidence of increased action in the conjunctiva and sclerotic, many of their vessels being filled with red blood; only few of the latter but very many of the former being perceptible. Farther, Mr. Tyrrell tells us that in most instances there has been a tendency to rheumatic affections.

Mr. Tyrrell says that this form of disease is to be relieved or cured by general means only, and he mentions having found the warm bath particularly serviceable. We can readily understand how nearly all kinds of local remedies might be "employed without any beneficial result;" but this we do not consider an argument against their judicious employment, after well-directed general treatment has subdued more particularly the sclerotic and corneal part of the affection. And we find Mr. Tyrrell himself recommending, under the head of catarrho-rheumatic ophthalmia, astringent applications to the conjunctiva. As to the warm bath, it will be observed that in Mr. Tyrrell's three cases it was had recourse to only after the disease of the eye had existed, and been treated for some time, it can therefore be looked upon as having acted in those instances merely as an auxiliary.

Inflammation of the Sclerotic and Iris. Mr. Tyrrell gives as synonymes of this, "sclero-iritis, rheumatic-iritis, arthritic-iritis," and defines it to be "inflammatory action attacking the sclerotic and iris simultaneously." Farther on (at p. 410) he says, "In sclero-iritis the affection of the iris is usually secondary, consequent on and in some measure depending upon the sclerotitis." In iritis, on the contrary, the affection of the sclerotic, he says, is secondary, being an extension of the disease from the iris. What Mr. Tyrrell describes as iritis, then, he ought to have called "irido-sclerotitis." The fact is there is generally attendant on sclerotitis a degree of iritis, and on iritis more or less sclerotitis; and the predominance of one or other has been held sufficient to give name to the disease. We see no reason for deviating from this convention, particularly as no difference of treatment is implied in the distinction.

Both in catarrho-rheumatic ophthalmia and in rheumatic iritis Mr. Tyrrell is, we think, too chary of venesection. Most practical surgeons must know, and surely Mr. Tyrrell must have had occasion often to observe, how quickly severe internal inflammation of the eye is got under by a well-timed and well-supported bleeding, how rapidly the patient rallies, and how beneficially the use of tonics comes in afterwards.

In regard to the use of tonics in rheumatic iritis we entirely coincide with the views of Dr. Mackenzie, as expressed in the following quotation from his work:

"*Cinchona* is undoubtedly a remedy of considerable utility in the treatment of rheumatic iritis. I am as much opposed, however, to the idea of trusting to

it almost alone, as I am to the plan of confiding solely in the antiphlogistic and sorbefacient power of mercury in this disease to the neglect of bloodletting and other depletory means of cure. In an inflammation of so dangerous a nature as iritis we should be ready to avail ourselves of every remedy, and never allow ourselves to be beguiled into bad practice by an affectation of simplicity." (Dr. Mackenzie's Treatise, p. 465.)

Amaurosis. Had the arrangement Mr. Tyrrell originally intended been followed, the subject of amaurosis should have come in here; but he has preferred placing the description of the diseases of the ocular appendages at the end of the first volume, and commencing the second with amaurosis. Under the head of amaurosis, Mr. Tyrrell comprehends not only the various diseases of the nervous apparatus of the eye, but also those of the choroid, orbit, vitreous body, &c. It is not our intention here to follow Mr. Tyrrell on amaurosis, because we have already had occasion to notice (Vol. V., p. 40,) his article on the subject in the first part of the Cyclopædia of Surgery. We therefore proceed with the diseases of the eyelids.

Entropium. Mr. Tyrrell describes a form of entropium as depending on, besides a lax condition of the skin, a partial thickening of the conjunctiva at the point of reflexion from the lid to the globe, by which the orbital portion of the lid is pushed forwards, and the free margin consequently tilted inwards. Mr. Tyrrell appears here to have overlooked the most efficient cause of the inversion, that is, a swollen or œdematous state of the eyelid. The operation Mr. Tyrrell performs for entropium depending on contracted tarsus, is that which was employed by Ware, and consists in making a perpendicular section of the whole substance of the lid near its centre. This relieving the tension of the lid will, in some cases, be followed by a rapid removal of the inversion; but in other cases it is necessary, in order to complete the relief, to remove or destroy a part of the integument. The perpendicular section of the lid is immediately followed by a separation of the edges of the wound; and it presents an outline similar to that of the letter V; wide at the ciliary margin, and terminating in an acute point in the opposite direction. This wound is afterwards gradually filled by granulations, and very little deformity results. Mr. Tyrrell tells us he has performed this operation both on the superior and inferior palpebra; and in every case, hitherto, with perfect success. We are, therefore, inclined to recommend this operation in preference to that of Crampton, which is more complex, and it is to be confessed not always followed by perfect success. When entropium depends on a curved state of the tarsal cartilage, Mr. Tyrrell believes that little good can be done, by any method, short of the removal of the tarsus itself; or of so much of its free margin and the textures in connexion with it, as will include the cilia and the follicles from which they grow (p. 448); an opinion to which many have been driven by the frequent failure of all the nicer operations invented by Vacca, Jäger, and others.

Ectropium. In several severe cases of ectropium after burns, Mr. Tyrrell has adopted transplantation of a portion of skin, and in every instance with considerable advantage. (p. 456.) He gives two cases. In the second of these, two lower eyelids, a new nose, and a large extent

of mouth were made, so that Mr. Tyrrell has reason to boast of having "formed many of the striking features of Master Frost's face."

Passing over Mr. Tyrrell's first five articles under the heading of "Tumours of the Palpebræ," we stop at the sixth, which is entitled "Of Encysted Tumours of the Palpebræ unconnected with the Tarsus." These tumours are frequently congenital; they contain a fatty or glairy matter, sometimes mixed with hairs, and often adhere to the periosteum. "In those cases," says Mr. Tyrrell, "in which the cyst is intimately connected with the periosteum, and this is usually indicated by the indentation of the bone, I feel little disposed to meddle with them, as I have seen very extensive mischief result." (p. 480.) Mr. Tyrrell mentions two cases in which extensive exfoliation of the bone followed unsuccessful attempts to remove the cysts. When the subject of the first case, a lady, came under Mr. Tyrrell's notice, he "could touch some extent of the dura mater, from the loss of a large part of the roof of the orbit." He adds that eventually he "succeeded in closing the wound, and, fortunately, but little deformity resulted."

Nævus. Mr. Tyrrell has treated nævi by injecting them with stimulating fluid; but his proceeding differs from that of Mr. Lloyd in this, that he first injects the surrounding sound cellular tissue, which becomes consolidated by the consequent inflammation. He then injects the nævus itself. Mr. Lloyd, on the contrary, injects the nævus itself alone, having previously emptied it of blood by pressure, and warns us against injecting the surrounding cellular tissue; and for this purpose he recommends pressure to be made around the nævus by means of the cover of a pill-box, with a notch on it for the passage of the point of the syringe, during the act of injecting. We thus see that what Mr. Tyrrell courts, Mr. Lloyd dreads. "A much more serious accident, however," remarks Dr. Mackenzie (p. 161), "than the injection of the cellular tissue is apt to attend this method of treating nævus, namely, the passage of some of the fluid into the veins and thence to the heart. There is strong reason to suspect that this was the cause of instant death in a child nearly two years old, in whom a nævus, situated over the angle of the jaw, was injected with diluted aqua ammoniæ."

At p. 487, Mr. Tyrrell has a chapter entitled "Of Enlargement of the Meibomian glands;" but we believe that what he describes is nothing more than a circumscribed villous state of the conjunctiva. Enlargement of the Meibomian glands is felt distinctly under the skin like a string; for being, as Dr. Zeis, of Dresden, has shown, situated in the substance of the tarsal cartilage, they become prominent as well externally as internally.

Mr. Tyrrell's next chapter is on what is called "Tinea Ciliaris" at the London Ophthalmic Hospital, elsewhere psorophthalmia, ophthalmia tarsi, &c. Having noticed the subject in connexion with Mr. Tyrrell's simple chronic ophthalmia we pass it over, as also the chapter on Trichiasis, Distichiasis, and Phtheiriasis, as containing nothing to fix the attention. We therefore come to the

Diseases of the Lacrymal Apparatus. Mr. Tyrrell gives but a very slight anatomical sketch of the lacrymal apparatus. His account of the lacrymal gland and its ducts is not very accurate and complete. The

ducts do not open so high up as Mr. Tyrrell's description, which was certainly not taken from nature, would have them; and in regard to the gland, no mention is made of the lower mass (glandulæ congregatæ of Monro), which is situated in the substance of the upper eyelid.

Mr. Tyrrell tells us (p. 503), he has seen very few well-marked cases of disease of the lacrymal gland. At p. 505 he relates a case in which a surgeon excised the lacrymal gland by mistake along with a steatomatous tumour.

In discussing diseases of the caruncle, Mr. Tyrrell very properly warns the surgeon not to proceed hurriedly, in cases of enlargement of the body, to remove any part of it before giving his remedies a full and fair trial; "for should he do so, and the remaining part afterwards recover its proper condition, the caruncle does not present sufficient bulk to support the inner junction of the tarsi; consequently, the puncta lacrymalia become displaced, and the secretions are not properly carried away from the surface of the eye, therefore epiphora results." To this we might add, that, in puro-mucous conjunctivitis, the semilunar fold is apt to become very much enlarged; and we have seen a considerable piece of it cut away, under an impression that it was an excrescence from the conjunctiva. Small red excrescences, or polypi, grow from the caruncle. At the moment we write, we have under our eye a case in which a small red excrescence is seated, not on the caruncle, but in the depression between its upper part and the semilunar fold. Some authors speak of scirrhus primarily affecting the caruncle; but such a disease must be rare, if it occurs at all. We do not know of any recorded case. Mr. Tyrrell remarks:

"I have known the caruncle to be implicated in malignant disease, both cancerous and fungoid, such disease beginning in the lids or immediate neighbourhood; but I have never seen malignant disease commence in this body." (p. 507.)

Epiphora and stillicidium lacrymarum are used synonymously by some authors; by others, the former is employed to denote a flow of tears from the lacrymal gland, the latter the dropping of tears over the cheeks, in consequence of an impediment to their being carried off by the derivative lacrymal organs. In this latter sense, however, Mr. Tyrrell uses the word *epiphora*.

The affection Mr. Tyrrell describes under the name of contracted state of the puncta, and in which he says he has usually found most benefit derived from such general means as tend to reduce the attendant state of debility and lessen the nervous irritation, is we think rather an inordinate flow of tears from the lacrymal gland. Though he heads his chapter "*Of the contracted state of the puncta*," and tells us of their being found, on minute inspection, of so small a diameter as to be with difficulty recognized, he does not tell us what the nature of the contraction is. In relation to this subject, the following observations by Mr. Lawrence are worthy of being quoted here:

"Injuries of the lacrymal canals are mentioned as causes of stillicidium lacrymarum. [Mr. Tyrrell, it is to be remembered, uses the word epiphora for this.] My experience leads me to doubt the correctness of the statement. In a case of carcinomatous ulceration affecting the lower lid, I removed the inferior lacrymal canal; no watering of the eye ensued. I saw a gentleman with

ectropium of the lower lid, consequent on a lacerated wound.....The inferior punctum, on first view, seemed obliterated; but, on close search, it was found so reduced in size as to be barely visible; from this circumstance, together with the ectropium, it was obviously incapable of absorption. There was not the slightest stillicidium." (Lawrence's Treatise, p. 702.)

Mr. Tyrrell's description of acute inflammation of the lacrymal sac is distinguished for simplicity, clearness, and brevity. The following is his etiology of chronic inflammation of this part:

"Either extension of similar disease by continuity of surface, from the conjunctival layer of the palpebræ to that of the puncta, and thence to the sac, or otherwise, the irritation of a morbid secretion, resulting from chronic disease of the palpebræ, which is conveyed through the puncta to the sac, where it creates a similar disease. It may also arise in the sac, or mucous membrane of the sac, without any previous affection of the palpebræ; but I consider such cases extremely rare, unless produced by external injury. The disease occurs at all periods of life, but is much more frequent in those of scrofulous habit than otherwise." (p. 519.)

The second of the causes enumerated above, it will be seen, is that first mentioned by Scarpa. It is rejected, and we think justly, by Lawrence and Mackenzie. The former author expresses his conviction that "the lacrymal sac and nasal duct are the original and essential seat of the disease, while the palpebral affection, when it exists, is only secondary." (Treatise, p. 708.) We quote Dr. Mackenzie's account at greater length:

"Chronic dacryocystitis is not unfrequently complicated, either locally or constitutionally. Locally, it may be connected with catarrhal inflammation of the Schneiderian membrane, or continued disorder of the Meibomian glands and conjunctiva; although, certainly, the doctrine of Scarpa, that the general cause of this disease is the absorption of puro-mucous fluid from the lids by the puncta is incorrect. It will, in many cases, be found that chronic dacryocystitis is modified by some faulty state of the general health, and often by scrofula.....In other cases, chronic inflammation of the excreting lacrymal organs appears to depend on the weakly constitution of the patient, although he be free from scrofula; and in others it is evidently kept up, and in some appears to be produced by the disordered state of the digestive organs. Smallpox, measles, and scarlet fever frequently call into action an occult scrofulous disposition, and, at the same time, give rise to the particular local disease which forms the subject of this section." (Treatise, p. 252.)

In the treatment of obstructed nasal duct, Mr. Tyrrell has tried the punctum probe, the probe or bougie from the nose, injections of water by Anel's syringe, but without any decided relief. He has also "repeatedly endeavoured to overcome the obstruction by introducing a fine *silver* tube, connected with a narrow glass cylinder capable of holding a considerable column of mercury, through one of the puncta into the sac; so as to throw the weight of the column upon the duct; but this plan has not proved of any more service than those just detailed." (p. 522.) The above, the reader will perceive, is the plan which was described by Sir William Blizard in a communication made many years ago to the Royal Society, (Philosophical Transactions for 1780; Vol. lxx., Part i., p. 239,) but we believe Sir William's instrument had a *steel** tube and

* The intelligent reader need not be informed that a *silver* tube would be incompatible with *quicksilver*.

stopcock—an instrument in fact the same as Mascagni's for injecting the lymphatics. The result of Mr. Tyrrell's experience leads him to recommend the practice which we believe is generally followed, namely, that after a fair trial of leeches, followed by astringents and stimulants, has proved unsuccessful, an operation should be performed for the purpose of introducing either a style or a tube. Mr. Tyrrell prefers the former, principally because it can be so readily removed, if irritation or inconvenience result. Mr. Tyrrell uses the old-fashioned clumsy tube, which "requires much force to fix it properly in its place; the projection just below the cup or head being of greater diameter than the duct itself: in pressing the instrument into its place, the force employed *breaks the unguis*, ... it subsequently becomes fixed by its projection as the unguis unites, and remains hid in the sac and duct." Mr. Tyrrell tells us he has introduced above fifty of these tubes and with very excellent success. "I have only," he says, "had to remove two of them, and know of one other having been taken out by another surgeon: in two other cases the tube became obstructed; but I regularly cleared it by a punctum probe passed through one of the puncta into the tube." (p. 527.) Mr. Tyrrell does not tell us of what metal his tubes were made nor how long he continued to see his patients after the operation. Silver tubes are frequently decomposed and fall out in pieces through the nose.

Omitting, as we warned the reader we would, everything comprehended by Mr. Tyrrell under the subject of amaurosis and its congeners, we come to the morbid condition of the crystalline lens and its capsule.

Inflammation of the Lens. Mr. Tyrrell alludes to inflammation of the anterior wall of the capsule of the lens, and speaks of it merely as occurring in connexion with iritis generally; which shows he has never distinguished properly those cases described by Walther, and after him by Mackenzie and others under the name "Inflammation of the anterior hemisphere of the capsule," and by Dr. von Ammon under that of *Iridoperiphakitis*. The subjects of the cases, few in number, from which Mr. Tyrrell drew his description of inflammation of the lens were young. The disease appears to be of the same nature as that noticed by Mr. Lawrence (Treatise, p. 405,) in the following quotation. After mentioning the increased size of the lens, Mr. Lawrence says, "The capsulo-lenticular cataract is frequently caused by a chronic and almost insensible inflammation, or, at least, determination of blood to the eye, accompanied, not unfrequently, with symptoms of congestion in the head." What Mr. Lawrence here alludes to, we suppose, are the symptoms mentioned by Mr. Tyrrell, namely, "a sense of fulness in the organ, with uneasiness about the eyebrow or forehead, and the appearance of dark or gray muscæ, and occasional scintillations or flashing of light," and considered by him evidences of congestion in the choroid and retina.

Cataract. In regard to the different kinds of cataract, Mr. Tyrrell remarks, with very great truth, that the only useful divisions are such as tend to practical good. In this respect, therefore, he thinks, as concerns lenticular cataract, it is necessary to make two distinctions only, soft and hard. "This division I shall adopt," he says, "but in my description of these I shall endeavour to point out the principal varieties of

the disease, and explain the modification of treatment required in each He further says, and with great propriety, of capsular cataract, th "there is little utility in any division, further than as regards the orig or cause of the disease, which materially influences the prognosis, an requires a modification in treatment by operation." (p. 351.)

Muscæ volitantes sometimes occur before or during the formation cataract; but, to prove that they are not owing to the disease of t lens, Mr. Tyrrell adduces the fact, that he has many times removed cat racts from patients who have complained of muscæ, and he has inv riably found that the muscæ continued after the cataract had been g rid of. We may mention that Mr. Tyrrell is of opinion that the appea ance of spots or muscæ, or sparks or flashes, result from an affection the choroid or retina; he therefore recommends they should be carefu noted, as they require distinct treatment, and render the prognosis of t case, as regards the success of operation, more uncertain than when t cataract forms independent of them.

Diagnosis. It is well known to dissectors of the eye, that the crysta line lens, after lying a short time in water, becomes slightly opaque, pr senting a peculiar mother-of-pearl colour, or a glistening appearan somewhat like tendon. The same appearance presents itself in commen ing lenticular cataract. Sir David Brewster has shown that the mothe of-pearl appearance depends upon the fibrous structure of the lens. Th appearance, Mr. Tyrrell says, "has been supposed by many, and is d scribed in most works upon the subject, as indicative of opacity of th capsule," and claims to have first proved it to be in the lens; so th he has "no hesitation in declaring that it is not an indication of cap sular opacity." Mr. Lawrence's description (p. 403) of the opaque spo or streaks in capsular cataract being of a *glistening*, *chalky*, or *pear white*, affords some colouring to Mr. Tyrrell's statement, though it do not justify it; for the chalky, pearly whiteness Mr. Lawrence describes quite different from the mother-of-pearl or tendinous appearance in th lens alluded to by Mr. Tyrrell. Dr. Mackenzie gives the same descrip tion of capsular cataract as Mr. Lawrence at p. 649 of his Treatise; b in describing lenticular cataract, says, "sometimes it has a *pear shining appearance*, occasionally it is marked by radii from its cent towards its circumference, the lens already tending to break into suc divisions as we see it fall into when left to putrefy or to undergo desicc tion." (p. 648.) We would ask Mr. Tyrrell wherein this description Dr. Mackenzie differs essentially from his own? For ourselves, we hav long been perfectly aware of the seat of the glistening tendinous ap pearance in lenticular cataract.

At p. 363, Mr. Tyrrell notices what he considers a prevalent erro in regard to the state of the lens which he describes in the following ex tract: "In rare instances, when the cataract commences from the ci cumference, and proceeds by radii towards the centre, these radii are first confined to the posterior hemisphere of the lens." Mr. Tyrrell quite correct in stating the supposition to be erroneous, that this sta is indicative of posterior capsular opacity; but when he asserts indiscr minately that it is a prevailing opinion, we must say he lies under a mi take. "It is not an uncommon appearance," says Dr. Mackenz (p. 648), "to see opaque striæ stretching from the circumference of th

ens a short way into its substance. If such striæ run upon the back of the lens towards its centre, they seem to be situated in the vitreous humour; but this is an optical illusion." We might adduce farther, were it necessary, the same author's account of posterior capsular opacity, to show that there is nothing even in it to justify Mr. Tyrrell in his implied pretension to be considered as the corrector of the erroneous opinion he alleges to prevail.

Operations for Cataract. Speaking of operating for congenital cataract, Mr. Tyrrell says (p. 374), "From the experience which I have had, and which has been very great, I should recommend that the operation be performed not later than the third month, provided that the health of the child be good. I have operated in the fourth week after birth, and with perfect success."

The operation of division through the cornea was that generally preferred by Mr. Saunders in the case of infants; and, as Mr. Lawrence remarks (p. 447), "the operation of keratonyxis was well enough suited to the particular object which Mr. Saunders attempted to accomplish in congenital cataracts, namely, the formation of an opening in the opaque capsule." Mr. Tyrrell (p. 455), with others, prefers the posterior operation for solution in cases of congenital lenticular cataract, provided the patient be under two or three years of age. The anterior operation he adopts in cases of soft lenticular cataract, whether congenital, idiopathic, or traumatic, unaccompanied by any other disease, after the period of infancy. (p. 452.)

In connexion with the operation for congenital cataract, Mr. Tyrrell prominently sets forth the name of Mr. Saunders, as if no other one of his time but he had laboured in the same field. We, however, should be forgetful of our duty did we omit this opportunity to express the obligations this department of surgery lies under to the late Mr. Gibson, of Manchester, for the valuable contributions he made to it; but, above all, for the readiness with which he *made his experience public*, after having fully ascertained the advantages of his mode of operating. We would particularly refer the reader to Mr. Gibson's paper "On the use of the couching-needle in infants of a few months old," in the seventh volume of the Edinburgh Medical and Surgical Journal (1811), where he will find some account of the mode in which Mr. Saunders's experience was first promulgated.

Regarding the use of belladonna, for the purpose of keeping the pupil dilated, in cases whether of central cataract or opacity of the cornea opposite the pupil, Mr. Tyrrell adds to the list of cases in which the medicine has been constantly employed for years together, without any injurious effect and without losing its influence upon the iris. In one case, he relates, it has been employed for eighteen years, the application being made four or five times in the day. After this long-continued use of the narcotic, Mr. Tyrrell tells us he had an opportunity of examining the eye, the gentleman having omitted the application of it for forty-eight hours. "I then found," says Mr. Tyrrell, "the pupil of natural size, and the motions of the iris as rapid and perfect as if the belladonna had never been employed."

Mr. Tyrrell mentions (p. 381) that the cases of congenital cataract which he notices as being improved by the use of belladonna are unfa-

vorable for operation, it having been found at the London Ophthalmic Hospital that inflammation is much more frequent after operation than in the ordinary forms of cataract. When the operation is resorted to, he recommends one eye only to be done at a time, and that with great caution and delicacy. "It is right to observe," he says, "that most of the instances in which the operation has been productive of injurious effects have occurred in patients above the age of puberty; whereas in children, little mischief has resulted under my own observation." This is not exactly in accordance with our own experience. We have frequently operated on persons about and above the age of puberty, and have found scarcely any inflammation at all ensue.

Besides the operations by extraction, by depression, and by division or solution, "I," says Mr. Tyrrell, "shall describe two others; one of *drilling* the cataract; and another in which the operations of solution and extraction, or of solution and depression, may be advantageously combined in treating the same case." After noticing various circumstances, both local and general, which may contribute to render extraction so extremely difficult and hazardous, that it should not, in Mr. Tyrrell's opinion, be attempted, he proceeds to consider the arrangements necessary to be made when the operation has been determined upon, and many of his hints will be found well worthy of attention.

Mr. Tyrrell considers it advantageous to have the patient in a recumbent posture during the operation, whilst he himself sits at the head and takes command of the upper eyelid—with the left hand if the right eye be the subject of operation, with the right hand in the contrary case; he thus uses the knife either with the right or left hand, which we, with him, consider a great advantage, especially when an intelligent assistant to take charge of the upper eyelid is not at hand.

"I have given fair trial," says Mr. Tyrrell, "to three different sections of the cornea: first, I performed that usually adopted at the time I commenced my operations on the eye—it was downwards; secondly, I made the section upwards in a great number of cases; and thirdly, I have tried an oblique section, so as to make the flap of the cornea in a direction downwards and outwards. Necessity alone would compel me to make the flap downwards again by the lower section. Of the two other plans I rather prefer the last; when prolapsus of the iris takes place after the operation to such an extent as to displace the pupil, vision is more useful and perfect if the pupil be drawn downwards and outwards, than when it is drawn upwards." (p. 400.)

This, if the assertion be correct, is a matter of great importance. The section downwards and outwards preferred by Mr. Tyrrell was that generally adopted by the first Wenzel, and has lately been pretty extensively practised by Professor Rosas, of Vienna, who uses, as Wenzel did, a double-edged knife for the purpose. The knife of Rosas is simply one of Beer's, sharpened on the back.

Mr. Tyrrell does not approve of dilating the pupil with belladonna previously to the operation—a proceeding Mr. Guthrie some time ago lauded as the essential condition of success. Our own experience would lead us to believe it to be indifferent. It is certainly of no use in facilitating the exit of the lens; as after the section of the cornea is made, and the aqueous humour has escaped, the pupil does not continue dilated, but falls into a middle state.

Mr. Tyrrell gives a chapter on the difficulties of the operation, which

is well worthy of being studied by the young operator; for he must be prepared to expect the same difficulties over and over again; and a difficulty is always much less when a person is prepared for it. Mr. Tyrrell's account is true to nature. The marginal titles, "Escape of aqueous fluid," "Superior lid not properly secured," "Iris adherent to the capsule of the lens," "Lens dislocated," "Lens sinks in vitreous humour," "Protrusion of iris," "Protrusion of hyaloid membrane," &c., recall to mind untoward circumstances, often beyond the control of the operator, but, nevertheless, demanding the greatest coolness and circumspection on his part.

In regard to hemorrhage from the eye, Mr. Tyrrell says, "In some cases in which I have had to remove the displaced lens, there has been subsequent hemorrhage to such an extent as to destroy the eye." He thinks this hemorrhage arises from the artery of the posterior capsule of the lens. We doubt this for two reasons: 1st. That the artery in question is in the adult of extreme minuteness; and 2d, that hemorrhage ought to be a much more common occurrence in needle operations if this were its source. Mr. Tyrrell relates three cases of hemorrhage: in one of them he was not aware that the iris was wounded; in another he was satisfied it was not; in the third everything went on well during the operation, and the hemorrhage was discovered only after the bandages had been applied, and the patient raised from the recumbent posture. In our opinion the hemorrhage is more likely to come from the ciliary processes or the iris. From the latter we have seen it in consequence of a mere scratch with the curette. In this instance, as in all those observed by Mr. Tyrrell, the cataract had been formed for a long period.

Mr. Tyrrell does not think it proper to operate upon one eye of an elderly person whilst the other remains perfect; for it may happen that the second eye does not become affected, though this is very rare. The condition of the disease most favorable for operation, in Mr. Tyrrell's opinion, is when the cataract is perfectly formed, or nearly so, in one eye, and the vision somewhat misty in the other. Under such circumstances he extracts the perfectly-formed cataract. He objects to waiting until both cataracts are fully formed, and then operating on both eyes at once.

Treatment after the Operation. When there is a careful attendant, Mr. Tyrrell prefers that the patient should not go to bed, but be placed in an easy chair, or upon a sofa, in a half erect posture, when he can be more readily amused, till his usual period of rest; otherwise he is apt to sleep or doze, so much, as to prevent his having a tranquil night. If the patient have been in the habit of taking an opiate, or if he be inclined to restlessness, Mr. Tyrrell considers it prudent to administer a full dose in the evening after the operation; but remarks that this would be improper, if narcotics disagree with the patient. If symptoms of acute inflammation come on, Mr. Tyrrell recommends the abstraction of blood, generally or locally, according to the power of the patient. Besides this, he says, that "all the ordinary means to check inflammatory action must be further resorted to, if the symptoms of inflammation continue; excepting two, namely, the use of mercury, so as to affect the system, or the use of nauseating medicines. The one would prevent the union of the section of the cornea, by checking the adhesive process; the other,

by occasioning vomiting, might cause the loss of vitreous humour; therefore purgatives with abstinence, and a repetition of bleeding, must be principally relied on." In regard to mercury, Mr. Tyrrell gives no case of its bad effects under the circumstances, and appears to speak rather from theory than observation. Two or three days after the operation we have put the patient under the use of mercury, with as much advantage as in an ordinary case of iritis, and certainly without the result dreaded by Mr. Tyrrell. Surely Mr. Tyrrell must have seen ulcers of the cornea healing while the mouth was affected with mercury.

Mr. Tyrrell distinguishes between the acute and subacute inflammation apt to arise after the operation. In the former, the eyelids are swollen, florid, red, and tender to the touch, with a thick yellow secretion. The conjunctiva is in a state of inflammatory chemosis. In the latter, the eyelids are rather œdematous and the secretion about the cilia thin. The conjunctiva is in a state of serous chemosis. In the subacute disease the pain is sometimes as great as in the acute; and the former is just as likely to destroy the eye as the latter, unless properly and timely cared for. In the treatment of the subacute disease, as there is great weakness and depression, Mr. Tyrrell trusts much more to nutritive diet and ordinary stimuli, than to medicinal stimuli. (p. 427.)

Mr. Tyrrell recommends that the eye should not be examined until the expiration of forty-eight hours, but thinks it better to wait seventy-two. "The reparative process," he well remarks, "is so various in different individuals, that it is impossible to determine at what period the section of the cornea may be united." (p. 437.) A good rule is, that if the eye is easy, it should not be examined for eight days.

The operation of depression, like that of extraction, is applicable to cases of hard cataract; but should, in Mr. Tyrrell's opinion, be performed only when the latter operation is impracticable or hazardous. As one of the principal circumstances to be avoided in the operation of depression, Mr. Tyrrell mentions injury to the ciliary processes; but we doubt if this can be avoided by entering the needle as he directs, so close to the junction of the cornea and sclerotica as $\frac{1}{16}$th of an inch. Mr. Tyrrell combines reclination and depression in his operation. "The result of the operation of depression," he says, "has not, under my observation, been so successful as extraction; for which reason I adopt the latter, when the case admits of it, in preference to the former." (p. 447.)

The operation of dividing the cataract in order that it may be dissolved and absorbed, is performed by introducing the needle either through the cornea or through the sclerotica; Mr. Tyrrell recommends the former mode of proceeding, "when the surgeon wishes to lacerate the anterior capsule of the lens, or simply to puncture it;" by the latter mode he thinks the division of the lens into small pieces is best effected: He calls the one the anterior, and the other the posterior needle-operation, and gives as the synonyme of the former *keratonyxis;* of the latter, *hyalonyxis.* It is worthy of remark, however, that the coiners of these words did not use them in the sense Mr. Tyrrell does. Buchhorn* was the first who introduced the term keratonyxis, but he meant by it simply

* De Keratonyxide; Halæ, 1806. De Keratonyxide, nova cataractæ aliisque oculorum morbis medendi methodo chirurgica. Magdeburg, 1810; and also Die Keratonyxis eine neue gefahrlosere Methode den grauen Staar zu operiren. Magdeburg, 1811.

nctio corneæ, and says expressly that the lens may, by keratonyxis, be ot only divided but reclined; and that by it also an artificial pupil ay be formed, whether by iridotomy or iridodialysis. As to the word *hy-lonyxis*, we never saw or heard it used in the sense Mr. Tyrrell employs . The name is not classical at all. It was introduced by an itinerant culist of the name of Bowen, who published a small book describing a ind of operation by reclination, to which he gave the name in question. His proceeding consisted in introducing the needle through the sclerotica, t a distance of four lines from the cornea, and traversing the vitreous umour towards the posterior capsule, which he incised and then drew the ens out of its place, and pressed it down in the substance of the vitreous umour. Care was taken not to touch the anterior wall of the capsule. It may be added, that the word *sclerotonyxis* is used in an analogously xtended sense, and as a counterpart of *keratonyxis*.

In the operation of division through the cornea, Mr. Tyrrell makes use f a straight needle,* merely pointed to make it penetrate with facility, ut without cutting edges. He introduces it at the temporal side of the cornea near its junction with the sclerotica, the flat surface being opposed to the iris. As has been said, he limits his proceedings in this case like Conradi, who first introduced the operation, to laceration of the capsule simply. Mr. Tyrrell has observed vomiting to occur after the operation, in many cases of division through the cornea; in all cases, with one exception, when the cataract has been fluid. The vomiting generally subsided in two or three hours, but he has known it continue almost without interruption for forty-eight. "There has not been local inflammation of any importance in any of these cases."

In the operation of division through the sclerotica, or as Mr. Tyrrell calls it the posterior-needle operation, he uses a straight needle, cutting with its edges to the extent of about an eighth of an inch from the shoulder, with which, as has been mentioned, he divides the lens into small pieces. The operation he considers applicable principally to cases of congenital lenticular cataract, provided it be performed before the patient have passed the age of two or three years. "The posterior needle-operation," Mr. Tyrrell says, "should be also resorted to in cases of capsular cataract, when the lens has been removed, either by extraction or solution, resulting from operation or accident." He then goes on to prove what we believe is not denied, viz. that opaque capsule is not absorbed. By careful management, Mr. Tyrrell believes that the surgeon can restore vision in all the cases in which it is obscured by opaque capsule only, and that he may effect this with the needle. All that Mr. Tyrrell says on this subject, (pp. 461-2,) has already been very fully discussed by Mackenzie, (pp. 699-725,) but the former differs from the latter author in disapproving of extraction of an opaque capsule. The mode of proceeding, however, which Mr. Tyrrell appears to have had in view differs from that of Mackenzie.

We now come to the modifications in the operation for cataract, viz. drilling, and a combination of solution and extraction which Mr. Tyrrell describes as new. They may be so at the scene of his labours, but they are certainly not new in the history of ophthalmic surgery. Drilling is

* Dr. Jacob's needle must be known to most of our readers.

neither more nor less than division through the cornea in cases of cataract complicated with synechia posterior and contraction of the pupil, with such additions and combinations as the circumstances of the case suggest; for instance, incision of the iris subsequently with Maunoir's scissors, in order to enlarge the pupil. The operation of drilling, Mr. Tyrrell informs us, he adopted after much careful consideration, as he found the plan followed at the Ophthalmic Hospital some years since, so unsuccessful that he dreaded to undertake the operation. The plan here alluded to appears from what Mr. Tyrrell says, (p. 464,) to have been that which is illustrated by a case recorded by Maunoir, in the Med. Chir. Trans., vol. ix. p. 287, London, 1810. Cheselden's operation for artificial pupil, by incision, combined with division or depression of the lens we have sometimes found applicable in such cases. It has the merit of being more simple than Maunoir's proceeding, and less tedious than Mr. Tyrrell's drilling.

The combination of solution and extraction is the plan which was first described by the late Mr. Gibson, of Manchester, to whom, however, Mr. Tyrrell does not allude, nor to Mr. Travers, who has written expressly on the same subject. This omission we must confess to be unfair, and by no means compensated for by Mr. Tyrrell's general acknowledgments to other authors in his dedicatory address, particularly as the language of the description is such as to lead to the supposition that the plan is original.

What Mr. Tyrrell says in his chapter entitled "Remarks on the Operations for Cataract," (p. 481,) is very judicious, but such as need not detain us, being much the same as is to be found in other authors. We cannot refrain however, from making the following extract, and adding our voice to that of Mr. Tyrrell in reprobation of the proceedings to which he alludes.

"The operation for solution is not suited to cases of hard cataract, excepting to the extent I have described: months, and even years may elapse before the aqueous humour completely dissolves the hard mass; and independent of the tedium of the cure, there is constant liability to displacement of the indurated centre, as soon as the softer circumference has become removed, and then extraction must be performed at great risk, or destructive deep-seated inflammation will ensue, and the eye become disorganized. I have seen many such unfortunate terminations to cases of hard cataract, treated by the operation of solution; and in several patients operated upon by an oculist, who unblushingly states that his operation is nearly infallible in all cases of cataract: ought not such false statements to be publicly exposed?" (p. 484.)

Loss of the power of adapting the Eye to near and distinct Vision. Mr. Tyrrell thinks that adaptation of the eye is owing to some change in the figure of the lens. He further believes that the power which produces the change in the lens is not in the lens itself, but is probably connected with the ciliary processes, though he does not pretend to be able to offer proof of the accuracy of such an opinion. At any rate, this opinion is an old one: (see Porterfield on the Eye.)

If, however, any change takes place in the figure of the lens it can scarcely be by means of the ciliary processes, because they have no immediate connexion with the lens. The only extrinsic structure to the action of which a change of form in the lens can be at all attributed appears to be that first described, if we are not mistaken, by Sir Everard Home, in the Philosophical Transactions, and more recently by Dr. Von

ion. Sir Everard Home considers the structure muscular, and thus ibes it in his Croonian Lecture, published in the Philosophical sactions, Part I., 1822, p. 77. "Between these membraneous pro- s (ciliary processes) there are bundles of muscular fibres of $\frac{25}{100}$ths inch in length, which I believe have not before been described: they iate all round from the capsule of the vitreous humour, pass forward the edge of the lens, are attached firmly to its capsule, and there nate." Dr. Von Ammon calls the structure orbiculus capsulo-ciliaris, describes it as consisting of a corona of filaments extending from nner or posterior surface of the ciliary processes to the capsule of ens." (See Mackenzie, p. xxxi.)

ie of the cases Mr. Tyrrell relates in illustration of his description ars to have something in common with those generally described r the name of mydriasis. It is not mentioned whether the subject se 137 could converge the axes of the eyes on a near object, nor her the defect of vision was as great when one eye only was used ien both. This case appears to be similar to one of sudden and orary occurrence of presbyopia in a young boy lately reported aburgh Med. and Surg. Journal,) with great ability by Dr. J. Hunter.

tificial Pupil. Mr. Tyrrell subdivides this chapter into the follow- ections: 1st. Changing the natural position of the pupil. 2d. En- ment or extension of a diminished pupil. 3d. Forming a new pupil i the natural one has been destroyed, the lens and capsule remaining ct. 4th. Formation of a new pupil when the original opening in ris has been destroyed, and where the crystalline has been lost. iave seen that Mr. Tyrrell recommends changing the natural position e pupil in conical cornea; he also recommends it in cases in which entral portion of the cornea has been rendered permanently opaque. y, of Göttingen, we believe, was the first who introduced the prac- he opened the cornea, *prolapsed* a portion of the iris, and thus ght the pupil opposite the clear portion of the cornea. In many this dislocation of the pupil does not suffice; it is then necessary cise a portion of the iris. The operative proceeding advised by Mr. ell for this purpose is that first had recourse to by Beer and after- s by Gibson, and which is now the one most commonly employed iever the case admits of it. The only thing in which Mr. Tyrrell eculiar is in the use of "a fine blunt hook, with a long bend, *rell's Hook*; see plate ix.,)" p. 500. With this hook he catches of the pupillary margin of the iris in order to draw it out through orneal puncture. We have no doubt that Mr. Tyrrell's hook answers ourpose intended as well as he tells us it does; but we have also no t that the same thing can be effected, and with the same safety to capsule of the lens, with a fine forceps.

i order to effect enlargement or extension of a diminished pupil, Mr. ell recommends the operation of cutting out a piece of the iris. He irks that on operating in some of these cases, he has found that a le fissure has resulted in attempting to draw out the iris. Under i circumstances, he has performed a second operation on a similar i, making the opening in the cornea in a new position, so that the re resulting from the first might be readily extended. He adds that e may be, perhaps, rather more difficulty in avoiding injury to the

capsule of the lens, in the cases under consideration, than when the iris is free; but he says he has never yet been the cause of such mischief.

Sometimes the disease, which destroys part of the cornea, and allows of escape of the aqueous fluid, prolapse of the iris, &c., is also followed by partial staphyloma. This, he justly remarks, should, if possible, be remedied before an attempt be made to extend the pupil. In illustration of this, he relates (p. 507) a very interesting and successful case, in which he first diminished the partial staphylomatous projection on the nasal side of the cornea with lunar caustic in the manner already mentioned under the head of staphyloma, and then enlarged the pupil downwards and outwards. In twenty-four hours after the operation the patient could see to read readily without the aid of a glass, and soon returned to his duties as a customhouse clerk. The right eye had been entirely destroyed by the acute purulent ophthalmia which had produced the partial staphyloma in the left.

Of Forming a New Pupil when the natural one has been destroyed, the lens and capsule remaining perfect. The cases Mr. Tyrrell includes under this head are those in which the whole pupillary margin of the iris is involved in a cicatrice of the cornea. He introduces his broad needle through the margin of the cornea into the anterior chamber, and pierces with it the iris close to its adhesion to the cornea, being careful not to pass the point of the instrument backward, for fear of wounding the capsule of the lens. Having thus made a very small opening in the iris with the needle, he withdraws it and then passes the blunt hook into the anterior chamber, and hooks the iris through the opening previously made in it, and gently withdrawing the instrument, usually tears the iris from the point which has been caught with the hook, "and such a quantity of the membrane can be brought through the opening in the cornea by the hook as will effect a sufficient aperture in the iris; but, sometimes only a fissure results, as in the operation before explained." Under such circumstances, Mr. Tyrrell has made, after the eye had recovered from the first operation, a second opening through the cornea, a little above the centre, and seizing the upper margin of the fissure in the iris with the hook, he has drawn it to and through the puncture of the cornea, and thus formed a triangular-shaped opening in the iris. "The principal risk in these cases," he remarks, "arises from being obliged to use a pointed instrument to effect an opening in the iris, to permit the passage of the hook, the proximity of the capsule of the lens being so close to the iris, that it is easily injured, when cataract follows." ...... Mr. Tyrrell, however, says he has "very rarely inflicted injury to the lens in the operation first described." (pp. 511-12.)

Of Formation of a New Pupil when the original opening in the Iris has been destroyed, and where the crystalline lens has been lost. "The success of an operation for readily forming artificial pupil in all these cases," Mr. Tyrrell remarks, "depends upon the fibres of the iris being tense, so that they contract when divided." (p. 513.) He recommends Maunoir's operation by incision; but says nothing of Cheselden's, which is quite applicable in some cases and certainly more simple. In cases in which "a very small portion of the cornea has retained its healthy character, the larger part having been destroyed by slough or ulceration effecting an opening through which the lens has

escaped," Mr. Tyrrell has tried two modes of making an artificial pupil: One is a very imperfect attempt at the separation of the iris from its ciliary attachment; the other, which he tells us he has found more effective and less liable to promote inflammation than the former, which after several trials he has abandoned, is described in the following words: "Using the broad needle and hook; making an opening with the former, for the passage of the latter, through the opaque cicatrix, and another in the iris, at its point of adhesion to the cicatrix; and then with the hook tearing away a portion of the iris, as before stated." (p. 517.)

We have remarked that the operation of excising a portion of the iris which Mr. Tyrrell describes is no other than that of Beer* and Gibson.† At page 43 of Mr. Gibson's work, we read: "From the principle upon which this operation is performed, it is obvious that all injury of the crystalline lens or its capsule is avoided—a most material requisite to success." (See also p. 70.) At p. 731 of Mackenzie's work it is said, in reference to remedying central opacity of the cornea by the formation of artificial pupil by excision: "It would evidently be impossible, however, to do this by incision, excision, or separation, according to the modes already described, as having been adopted by Cheselden, Wenzel, and Scarpa, without injuring the crystalline lens and thereby producing cataract. This of course must be avoided, and hence have arisen certain necessary changes in the methods of forming an artificial pupil, according to the condition of the cornea and crystalline lens." (See also p. 749.)

After all this, and after having, in our own operations, attended to the injunctions laid down in the above quotations, the reader may guess the surprise with which we read at p. 518 of Mr. Tyrrell's second volume the following, the penultimate paragraph of his chapter on artificial pupil: "Very great advantage has resulted from the introduction of the small blunt hook, inasmuch as it enables the surgeon, in many cases, to form a pupil without injury to the lens, *which was not, I believe, previously attempted.*" . . . . . .

From this minute examination of Mr. Tyrrell's volumes, our readers will judge of the importance we attach to any work proceeding from a man of his long and extensive experience, and high and influential station in the profession; they will also come to the conclusion, which we have done, that in the present publication great excellence is allied with considerable imperfection. Of Mr. Tyrrell as a practical man, all that we have heard has been in favorable testimony of his qualifications. Our remarks, therefore, have reference merely to what may be judged of by the perusal of his work. And this perusal has satisfied us that experience, however great, unless aided and directed by the most accurate knowledge of the minute anatomy and physiology of the eye, and a critical acquaintance with the accumulated experience of others—however it may form a skilful practitioner—is insufficient to prepare a man to appear with advantage as an author on ophthalmic medicine and surgery.

Though we have entered so fully into detail, regarding the matter of

* Ansicht der staphylomatösen Metamorphosen des Auges, Wien, 1806.

† Practical Observations on the Formation of an Artificial Pupil in several deranged states of the Eye; to which are annexed remarks on the extraction of soft cataracts and those of the membraneous kind through a puncture in the cornea. London, 1811.

Mr. Tyrrell's work, we have as yet said nothing of the manner in which it is composed. We always feel it to be a most agreeable task to have to praise the style of medical writings, because excellence in this respect is indicative of those acquirements in literature which the public have a right to look for in members of our profession. We are sorry to say, however, that this task cannot be ours on the present occasion—not, we presume, from incapacity, but from inattention on the part of the author. It is impossible to believe that the literary faults so abounding in Mr. Tyrrell's volumes—not merely inelegancies and incorrectness of language, but mistakes as to the force and application of words, tautology, and even bad grammar,—could have any other source than that disregard to the niceties of composition which men of a practical turn of mind are too apt to fall into. This, however, is a fault and an evil, which it is the duty of those who hold the office we fill, to do their best to correct and deprecate.

The first ten pages of this article having been, by mistake, printed off without revisal, some errors will be found in them, as the following: p. 58, line 13, for 'proves' read 'prove,'—line 32, for 'in' read 'on;' p. 61, last line, for 'symptom in all inflammations' read 'symptom, whenever it is present, in inflammation;' p. 62, line 36, for 'diseases' read 'disease;' p. 63, line 12, for 'last' read 'July,'—line 37, for 'diseases' read 'disease;' p. 64, line 6, after 'as' insert 'in,'—line 12, for 'proportions' read 'properties.'

ART. IV.

1. *On Instinct.* By W. P. ALISON, M.D., &c. (*Cyclopædia of Anatomy.* Part XIX.)—*London*, 1840. Royal 8vo, pp. 29.
2. *The Philosophy of Instinct and Reason.* By J. STEVENSON BUSHNAN, M.D., F.L.S.—*Edinburgh*, 1837. *With Eight Lithographic Plates.* 12mo, pp. 316.
3. *On the Habits and Instincts of Animals.* By WILLIAM SWAINSON, F.R.S., &c. (*Cabinet Cyclopædia.*)—*London*, 1840. *With Ninety-one Woodcuts.* Small 8vo, pp. 375.

THE designation *instinct*, it is well observed by Dr. Alison at the commencement of his able article, "is often applied to the mental acts of the lower animals, as if it were truly applicable to the whole of those acts, but a little consideration will show, *first*, that this word in its more approved and correct acceptation, is applicable only to a *part* of the mental operations which may be inferred from the observation of the actions and habits of animals; and, *secondly*, that in this restricted sense the term is applicable to a part of the operations of the human mind itself, and that the subject of instinct cannot be thoroughly understood, unless information regarding it is sought in the consciousness of our own minds, as well as in the observation of other living beings. The study of this subject is therefore equally important as a part of natural history, of mental philosophy, and of human physiology, and is a good illustration of the necessity of this latter science being based on the observation and generalization of the laws and conditions of vital action throughout the whole extent of the animal creation." Believing that, on these accounts, our readers will feel interested in the subject, we shall offer them a short account of Dr. Alison's views, and some remarks of our own thereupon.

The term instinct, according to Dr. Alison, stands in opposition, not to the *will* but to the *reason* of man. "The most correct expression of the difference between an action prompted by instinct and one prompted by reason is, that in the first case the will acts in obedience

to an impulse which is directly consequent upon certain sensations or emotions felt or remembered; in the last, it acts in obedience to an impulse which results from acts of reasoning and imagination." (p. 3.) With the general view which this proposition is intended to enunciate, we fully accord, but we must differ from Dr. Alison in regard to the opinion that the *will* is concerned in the production of *instinctive* actions. It will be remembered, that on a former occasion we made use of this term to include all those actions "which are performed in direct respondence to an external stimulus," (Vol. V. p. 491;) and we accordingly give it a much more extended application than does Dr. Alison, who restricts it to what we deem the highest class of such actions. But even using it in *his* sense, we cannot regard instinctive actions as produced through the will, *when instinct is acting alone.* In a great majority of cases, the instincts of man suggest motives to his judgment, and the resulting action is voluntary, although first stimulated by the instinctive propensity. But, in animals whose instincts predominate over the reasoning powers (the contrary being the case in man), we are of opinion that these instincts act *primarily* on the motor nerves. The case seems to us exactly parallel with that of the emotions, on some of which the instinctive propensities border very closely. There is no question that the emotions can act on the muscular system, through the efferent nerves, independently of the will, and even in opposition to it; and that they operate through a distinct set of nervous filaments appears proved by the fact that the muscles may be paralyzed to one set of impulses, and not to the other. But, though the emotions *can* act immediately on the motor nerves, it is rarely that they do so in man, for they usually, that is, when only moderately excited, influence the current of thought, supply motives to the judgment, and therefore operate through the will. Physiologically speaking, the two channels are perfectly distinct, and the question is, to which we are to refer the instinctive actions "in which the impulse is directly consequent upon sensations or emotions." We maintain that it is most correct to refer them to the involuntary class, and to apply the term *will* only to that action of mind upon muscle which "results from acts of reasoning and imagination." Perhaps it may be said that this is a question of words only, involving nothing more than the signification of the term *will*, which may be extended or not at the pleasure of the writer. But though it may be so to the metaphysician, it is not to the physiologist, who seeks to allocate in the nervous system the different psychical powers, of which, as a whole, it is unquestionably the instrument.

We shall, for the sake of convenience, employ the term instinct for the present in Dr. Alison's sense; excluding from the group of actions we formerly associated under it all save those in which *sensation* participates. The question now arises whether the lower animals are so formed that *all* their actions are instinctive, that is, are performed in direct respondence to a stimulus acting through the sensorium; or whether any of them involve the operation of reasoning powers. According to Dr. Alison, the latter is the occasional exception; the former the general rule. Still he admits that the examples which he cites, and which we forbear to quote because previously in print, "leave no reasonable ground for doubt that, on certain subjects at least, some animals are

capable of short and simple processes of reasoning or of imagination, which appear to imply the perception of general truths; and that the difference between the operations of their minds and ours in that respect is one of degree only, not of kind." That a deficiency of the power of forming *general notions*, and a limitation of their minds to particulars, and hence an incapacity for that adaptation of means to ends which implies a knowledge of the effect of these means founded upon general principles, is characteristic of the lower animals, we readily allow; but we do not think that Dr. Alison has given them quite as much credit for reasoning powers as some of them deserve. The following circumstance, communicated to us by several eye-witnesses, is, we think, a very good illustration of the reasoning power of brutes. Some horses kept in a paddock were supplied with water by a trough which was occasionally filled from a pump—not, however, as often as the horses seem to have wished; for one of them learned (*suâ sponte*) to supply himself and his companions by taking the pump-handle between his teeth and working it with his head. The others, however, appear to have been less clever or more lazy, and finding that this one had the power of supplying their wants they would teaze him, by biting, kicking, &c. until he had pumped for them, and would not allow him to drink until they were satisfied. Now it may be said that this was a mere act of imitation; but we think the contrary. When brutes *imitate* the actions of man, it is by a movement of the corresponding parts of the body, as when a monkey plays a knife and fork and takes his glass of wine like "any chrisom child." We can scarcely avoid the belief that this clever horse had formed a *general notion* that the action of working the pump-handle up and down would cause the flow of water into the trough, and that he adapted his means to the end in view with a sagacity which would have done credit to many a human being. If, following him, the other horses had done the same, we should have ascribed it to imitation *in them;* just as when sheep follow their leader through a gap he has discovered in a hedge.

Another instance, which also exemplifies the formation of general notions, we shall relate on the authority of an eye-witness. A wren built its nest in rather a dangerous situation in the quarries of Penrhyn, so as to be liable to great disturbance from the occasional explosions. It soon learned, however, to quit its nest and fly to a little distance, on the ringing of the bell which warned the workmen. This was noticed, and demonstrated to visitors, so that the poor wren suffered many needless alarms. It at last learned, however, that the first general notion it had formed—of the ringing of the bell being followed by an explosion—was liable to exceptions, and it formed another more correct. For it was observed after a time that the wren did not leave its nest unless the ringing of the bell was followed by the moving away of the workmen. Dr. Alison further states that "none of those still more general or abstract notions which continually suggest themselves to the human mind in the course of the operations that are excited by the senses, such, for example, as space, time, number, power, &c. are indicated by any manifestations we see of the mental acts of the lower animals." In this, too, we scarcely think that he does them justice. Many instances are on record which show such a remarkable knowledge of the passage of time, and the period of recurrence of particular epochs, that we can scarcely avoid the

conclusion that some such general notions exist. At least we see as much reason for inferring them from their actions as we should from the actions of man, apart from our own consciousness. A case of this sort, as interesting as any that we have heard of, has fallen within our own experience. A number of young ladies, pupils in a boarding-school, are accustomed to go daily into their playground at a quarter past twelve, and there to eat their luncheon. They return to the schoolroom after about a quarter of an hour, leaving, of course, a good many crumbs on the ground behind them. The sparrows in the neighbourhood have learned to expect these, and for a quarter of an hour before the daily "run out," as well as during that period, they may be seen congregated on the walls of the playground ready to pounce down as soon as the pupils have left it. Their punctuality is remarkable. But on Sundays, when, on account of an earlier dinner-hour, no luncheon is eaten, the birds do not expect it, for no congregation of them takes place. The case would be less remarkable if the birds only collected *during* the time when the pupils are in the ground, as the omission on Sundays would then be accounted for by the want of the usual signal; but they assemble regularly on week-days *before* any such signal is given, as we have ourselves witnessed many times.

We are disposed to believe, then, that in the higher classes of those commonly denominated the lower animals, reasoning powers exist rather inferior to those of men in degree than differing in kind; and that, as we ascend the scale, the instincts are gradually subordinated to them; so that, in the highest among them in point of intelligence, the general conduct is rather governed by the will, acting in obedience to intellectual operations (which may be originally stimulated by the instinctive propensity) than in direct respondence to the stimulus of sensation. The dog and the elephant are usually regarded as the most intelligent of the lower animals; but it may be questioned whether their intelligence is *really* greater than in other mammalia, the quadrumana for example; or whether it does not appear so to us in consequence of their instinctive attachment to man and the accommodation of their actions to his. Of the *reasoning* power of the elephant we shall adduce an interesting example from the work of Mrs. Postans on British India, which will be probably new to most of our readers. It was communicated to the authoress by the owner of the animal, an officer in the Bengal service. This gentleman "possessed a handsome elephant, which he was accustomed to see fed with a certain allowance of grain daily. Business requiring his absence, he confided the care of his favorite to a worthless keeper, who, in the interim, stole and appropriated a large portion of the grain intended for the elephant's use. The poor animal daily grew more spare and feeble, missing at his usual feeding-time the abundant feast supplied by his kind and generous master. My friend returned, hastened to his stable, observed the emaciated state of his favorite, and having had no previous reason to suspect the honesty of the servant, was at a loss to discover the cause of the evident alteration. The poor elephant, delighted at his master's return, trumpeted his welcome, raised his trunk as a salaam, and moved about, affording, in his mute but expressive manner, every demonstration of joy. His feeding-time approached, and the full allowance of grain was placed at his feet by his dishonest and cruel keeper. The elephant, satisfied of his *master's* attention, industriously

separated it into two distinct heaps, and having eagerly devoured th one left that which remained and quietly walked to the opposite side c the stable. The truth, thus conveyed by the gestures of the intelligen brute, flashed upon the mind of his master. The keeper, on being ac cused of the theft, and finding his unworthiness exposed, fell at the fee of his employer, acknowledging the aggression." Now, we would ask could any mute human being more justly reason or more cleverly adap his means to the end in view? "The native keepers of elephants," add our authoress, "will not allow that the animals are influenced by th passion of fear, but declare their obedience to be an impulse of gratitud and believe them to possess the reason peculiar to human nature." On more instance of rationality, with a degree of self-control which it ma be wished that every human being would imitate, we shall quote fro Dr. Davy, (Anatomical and Physiological Researches, vol. i., p. 884 who himself witnessed it. The animal had a deep abscess in the bacl which it was necessary to lay open in order to effect a cure. "He wa kneeling down, for the convenience of the operator, not tied, his keep was at his head. He did not flinch, but rather inclined towards the su geon, uttering a low suppressed groan. He seemed conscious that wh was doing was intended for his good; no human being similarly situate could have behaved better. I think it right thus to record this instanc which I witnessed myself, of this animal's (may I call it) reflectin power and conduct, which it is difficult to consider otherwise tha rational. And so confident were the natives that he would behave as h did that they never thought of tying him."

It can scarcely be questioned, we think, that the *general* charact of the intellectual powers of the lower animals is such as Locke repr sented it; a very limited enjoyment of the faculty of *abstraction*, b which the mind is enabled to single out the different qualities or relatio of the individual objects of sense, and make them the subject of abstra thought, by which general notions are formed.

"The two noblest and most characteristic of the faculties of the human min as defined by logicians," remarks Dr. Alison, "those of reasoning and imag nation (the latter of which is truly applicable to every kind of *contrivance* adaptation of means to ends of which we are capable), evidently imply tl existence of a mental power of forming and dwelling on general ideas, whi are equally applicable to many individual cases; and if animals possessed th latter power, we might confidently expect to see them exhibit indications of tl two former. Why is it, for example, that the monkeys, who have been observ to assemble around the fires which savages have made in the forests, and bei gratified by the warmth, have never been seen to gather sticks and rekind them when expiring? Not, certainly, because they are incapable of unde standing that the fire which warmed them formerly will do so again, but becau they are incapable of abstracting and reflecting on that *quality* of wood and th *relation* of wood to fires already existing, which must be comprehended, in ord that the action of renewing the fire may be suggested by what is properly call an effort of reason. Or why is it that the different classes of predaceous an mals, although surrounded by the materials out of which the human race h manufactured so many implements of warfare, have never been able to ava themselves of any of them, in aid of the instruments of destruction with whi nature has furnished them? Apparently, because they are incapable of formin such general notions of the qualities and relations of external things, as a essential to the processes of imagination and reasoning, by which men are l to the contrivance and guided to the use of artificial weapons." (p. 2.)

Fully admitting the *general* correctness of these remarks, we think that there is not enough allowance made for the intellectual differences between the various classes and orders and genera and species of the lower animals, which, we are convinced, are as striking and as important as their corporeal diversities. We are of opinion that every kind of animal has its psychical as well as its physical powers, adapted to the kind of life which nature (in other words its Creator) intended that it should lead. There can be no doubt that this is the case in regard to the *instinctive* propensities; and this general law of adaptation to the natural habits of the animal appears to govern the hereditary transmission of *acquired* instincts; those being transmitted which have such a relation, and those being lost which are purely the result of human agency.* And if we attentively compare the *intellectual* faculties of each species with those of man, who possesses them all in the state of highest development, we think that, as in different individuals of the human race, we should find different faculties predominating in the several tribes; a view which was well stated by Gall, although, in working it out, he fell into many errors. Thus, in one individual the sensations are acute and the perceptions dull. In another, the intuitive perceptions are strong and ready; but there is a deficiency in the powers of reflection, owing to the want of the faculty of abstraction. And, in a third, the mind is capable of profound generalizations, but is not quick at observing. When the faculties of abstraction, or, in Hoffbauer's language, *intensity*, is wanting, silly imbecility is the result. When the perceptions are very slow, or the mind is deficient in *extensity*, the power of bringing itself into relation with external things, stupid imbecility or idiocy displays itself. Now the intellect of the monkey we regard as peculiarly deficient in *intensity*; and that it should be so, the habits of the animal, and the circumstances in which it exists, afford quite sufficient reason. Of what use, for example, would be the power of making a fire to an animal that lives only in warm latitudes? And, to refer to Dr. Alison's other instance, of what use would the power of constructing weapons of offence be to animals already supplied by nature with all that is necessary for their support, and physically incapable of using them, if they were put (we were going to say "into their hands,") into their possession? And yet in their mode of lying in wait for their prey, and in the variety of stratagems they use to obtain it, these animals show a high amount of reasoning power, its limited scope being adapted to their peculiar circumstances. We shall presently endeavour to exhibit a little more in detail the gradation that may be traced in the different classes of animals in regard to the development of their intellectual powers, and the subordination to them of the instinctive.

There is one remarkable deficiency in the psychical powers of the lower animals, which we do not see expressly noticed by Dr. Alison. It is that of regulating or controlling the succession of thought. Metaphysicians differ as to the mode in which this power is exercised; but all agree that man possesses it by a direct or indirect operation of the will. "It may not be going too far to assert," it is justly remarked by Dr. Abercrombie, "that our condition in the scale both of moral and intellectual beings is, in a great measure, determined by the control which we have acquired over the succession of our thoughts, and by the sub-

* See Carpenter's Principles of Physiology, § 550, and the authorities there referred to.

jects on which they are habitually exercised." As far as we can infer the psychical powers of the lower animals from their actions, we do not think that there is any reason to believe in their possession of this faculty. And they thus stand in relation to those weak-minded persons of the human race who are deficient in it, all their trains of thought being obviously dependent on, and entirely governed by, either present sensations or the memory of past.

With respect to the *moral feelings* of animals, we have always regarded it as difficult to distinguish those exhibited by the highest among them from those of man; and have been accustomed to consider *his* possession of a notion of a spiritual being, in the light of an intuitive perception, immediately suggested to his mind by external occurrences without any act of reasoning, as the peculiar characteristic of his moral faculties, as the power of directing his current of thought is of his intellect. We are much pleased to find Dr. Alison entertaining a similar view.

"Although many of the lower animals are susceptible of the emotions of joy, and to a certain degree of gratitude and attachment, founded on the sense and recollection of benefits, none of them seem capable of forming the slightest notion of that Divine Power which has suggested itself to the human intellect in all ages and even in the rudest conditions of human existence; we should regard any act of praise or prayer as an infallible indication of a mental capacity of the same rank as our own." (p. 2.) The attachment of the dog to man, however, is evidently influenced by feelings of parallel character; but, like those of the young child, they are not susceptible of relation with any being not the object of sense,—"Man," it was expressively said by Burns, "is the god of the dog."

Having thus pointed out the existence, in many of the lower animals, of psychical endowments corresponding to those which we term the intellectual powers and moral feelings of man, we shall proceed to enquire what are the characteristics of those actions which are purely instinctive. These are well laid down by Dr. Alison; but to avoid confusion, we shall slightly modify his language, where it implies the participation of the will in actions purely instinctive. In all cases, those actions which are entitled to the appellation of instinctive, are generally understood to be characterized by two marks, quite sufficient to distinguish them from the effects of voluntary power guided by reason: 1. That, although in many cases experience is required to give the will command over the muscles concerned in its operations, no experience or education is required in order that the different actions which result from an instinctive impulse may follow one another with unerring precision. 2. That these actions are always performed by the same species of animal nearly, if not exactly in the same manner; presenting no such variation of the means applied to the object in view, and admitting of no such improvements in the progress of life or in the succession of ages, as we observe in the habits of individual men, or in the manners and customs of nations adapted to the attainment of any particular ends by those voluntary efforts which are guided by reason. A third distinctive mark, naturally resulting from the last, is at least equally characteristic, although much less generally observable: that these instinctive actions are seen to be performed in circumstances which reason informs us to be such as to render them nugatory for the ends which are usually accomplished by them, and for which they are obviously designed. The efforts made by migratory birds,

ren when confined, at their usual period of migration, the mistake of the esh-fly, who deposits her eggs on the carrion plant instead of a piece f meat, or of the hen who sits on a pebble instead of an egg, are so iany proofs that an instinctive action is prompted by an impulse, which sults merely from a particular sensation or emotion being felt, not by nticipation of the effect which the action will produce.

If, with these views, we take a general survey of the habits of the different tribes of animals—distinguishing those actions which are invariably erformed by all the individuals of one species under the same circumstances from those which exhibit a difference of physical character nongst them, and separating those which show a highly-refined adaptaon of means to ends, such as we can scarcely conceive to exist among ie lower classes, from those which give evidence of reasoning processes f a simple character—we cannot avoid the conclusion that the instincts re most remarkable, and most unaffected by any reasoning processes i the class of insects, and preeminently so in the order hymenoptera, which includes most of those that live in perfect societies, such as the ees, wasps, and ants. Now it is a little remarkable that, of all animals, nsects are most remarkable for their *locomotive* powers, estimating these ccording to their size; and that, of all insects, the hymenoptera possess these powers in the highest degree. These facts we shall presently turn to further account. There can scarcely be a more extraordinary instance of the adaptation of means to ends, without (as there is every reason to believe) any *intention* on the part of the animal, than the peculiar construction of the comb of the bee. The substitution of the hexagonal form of the cell for the circular, by which a great deal of material is saved, whilst the purpose of the cell is equally answered, is but a comparatively simple example of this. It is in the mode in which the cells on the opposite side of the comb bear upon one another, that we see the most refined contrivance. The two sets of cells are so arranged that the centre of the bottom of one cell is opposite the point at which the partitions of three cells meet on the other side; and consequently the bottom of each cell is opposite a portion of the base of three cells opposed to it, instead of directly meeting the base of one, an arrangement which obviously adds great strength. But even this is not all. The bottom of the cell is not a flat partition, but is formed by the union of three planes at a certain angle; each of these planes forming part of the base of one of the opposite cells. The human mathematician has discovered, what would not be at once apparent to the mere observer, that, by the meeting of these planes at an angle, greater strength, in proportion to the amount of material employed, is obtained than if they formed part of one continuous surface. The determination of the particular angle at which these objects should be most effectually fulfilled, was long ago attempted by Maraldi. It is a problem requiring the highest powers of the mathematician to solve. The result obtained by him differed by only two minutes of a degree from the result of the admeasurement of the angle actually employed by the bee; and this approximation has been usually considered quite sufficiently close; the chances of error in the admeasurement of an angle made by so small a surface being considerable. More recently, however, Lord Brougham has retraced Maraldi's ground; and, by the application of still higher

modes of investigation, he has determined that the bees were right and the mathematician wrong, having brought the result of his calculations to agree perfectly with the actual admeasurement!

No one, we think, will venture to say that the bees go through such a calculation. It is evident that the operations by which this remarkable effect is produced must be independent of any judgment or foresight on their parts, but must be as necessary a result of their physical conformation, as their mode of obtaining their food, or any other purely instinctive action. The same may be said of their other habits, in which the utmost foresight and contrivance is exhibited; but it can scarcely be regarded as *their* foresight and contrivance. It is in the *deviations* of the instincts of insects, and their accommodation to circumstances, as Mr. Spence justly remarks, that the exquisiteness of these faculties is most decidedly manifested. The instincts of the larger animals seem to undergo but slight modifications; or, when modified, it is rather by their reasoning powers. But insects (as is well known to those who have observed their habits, especially those of the social tribes,) often exhibit the most ingenious resources, their instincts surprisingly accommodating themselves to the new circumstances under which they are placed, in a manner even more wonderful and incomprehensible than their ordinary operations. But, it will be asked, do not these variations indicate the existence of reason in the insect tribes, and in what do they differ from those which the higher animals exhibit? We shall answer this enquiry in Dr. Alison's words:

"When such facts are duly considered, we cannot be surprised to find so intelligent a naturalist as Mr. Spence acknowledging that he had at one time arranged them as indications of reason in these animals. But, on further consideration, we shall probably see cause to acquiesce in his later and more matured judgment, which ascribes them to strictly instinctive, though singularly varying propensities; chiefly on two grounds, which exactly correspond with what have been previously stated as the most distinctive characters of instinct: 1. That, although various contrivances are fallen on by *all* bees, to enable them to continue their usual operations under varying external circumstances, yet there is no such variety observed, either in the conduct of individuals of the species, or in the conduct of different communities, as we cannot doubt must occur if the inhabitants of every hive were guided, on such unusual occasions, by processes of reasoning, by observation of the laws of nature, by experience, and by the anticipation of the effects of their actions. If such mental processes were their guide, we should certainly observe a difference in the conduct of experienced workers, and of those just emerged from their pupæ; and we should observe some variety in the expedients adopted in different hives, for meeting such accidents or difficulties. 2. While the varying operations of these animals for one particular end, the preservation of their own lives and the perpetuation of their species, are planned and combined in such a manner as to indicate consummate intelligence as to what is essential for that purpose, all these indications of instinct are limited to that object, and we see no evidence of the exercise of their senses suggesting to them any other trains of thought, or exciting them to the prosecution of other objects, such as a number of human intellects capable of planning and executing such works would certainly, sooner or later, attempt to accomplish. The degree of uniformity seen in their operations, and the limitation of the objects on which their faculties are exerted, are therefore our reason for thinking (though we do not wish to express ourselves with absolute confidence on the subject) that the mental processes concerned, even in those the most elaborate and artificial of the works of animals, belong to the same class as those notions of man which are prompted by his instinctive propensities as distinguished from his reason." (p. 21.)

At the same time it must be admitted that many acts of individuals and of communities among insects appear to indicate the guidance of some amount of reasoning power. But we must take leave to doubt some of the cases upon which this assertion rests. The narration of them is often coloured by the preconceived views of the individual, however desirous he may be of stating the bare facts; and in many instances we feel confident that a fuller knowledge of the natural habits and instincts of the species would show *that* to be a part of them which has been regarded as evidencing the operations of a reasoning power. Dr. Darwin's anecdote of a wasp is a case precisely in point. We shall first quote his own account of it, (Zoonomia, vol. i. p. 263:) "A wasp, on a gravel walk, had caught a fly nearly as large as himself; kneeling on the ground, I observed him separate the tail and head from the body part, to which the wings were attached. He then took the body part in his paws, and rose about two feet from the ground with it; but a gentle breeze wafting the wings of the fly turned him round in the air, and he settled again with his prey on the gravel. I then distinctly observed him cut off with his mouth, first one of the wings, and then the other, after which he flew away with it unmolested by the wind." Now on this we remark that, as we have been informed by two competent authorities, it is a *constant* habit of the wasps to cut off the head and wings of insects which they attempt to carry through the air. Even supposing (which we are disposed to question) that the wasp did really rise into the air without separating the wings, its alighting again to do so is no proof that it foreknew that its flight would be thus rendered easier, since it merely acted in accordance with its usual instinct, which some circumstance had at first caused it to forego. But, by subsequent narrators, this story, already quite good enough, has been further embellished. Dr. Alison quotes it, we are sorry to say, not from the original, but from Mr. Duncan's Tract on Instinct, where it is thus related: "Dr. Darwin observed a wasp with a large fly, nearly as big as itself. *Finding it too heavy*, it cut off the head and abdomen, and then carried off the remainder, with the wings attached to it, into the air; but *again* finding the breeze act on the wings, and impede its progress, it descended, and deliberately cut off the wings." Here we are given to understand that the wasp made *two* attempts; and it is obvious that this, were it the fact, would have an important influence on the view we take of the question on which it bears.

It would occupy too much space if we were to follow Dr. Alison through his very interesting review of the different groups of instinctive actions. He considers that they may be best classified under the following heads, according to their final causes:

1. Instincts designed for the preservation of the individuals exhibiting them. In this division are comprised those which relate to their means of escaping or repelling injury or violence; those by which food is procured; those which lead to construction of habitations; and those connected with hybernation.

2. Instincts for the propagation and support of offspring. Under this head may be placed, besides the mere sexual propensity and those immediately connected with it, those migrations of animals which are performed in furtherance of the object of propagation: the choice of a

suitable place for depositing their eggs, nidification, incubation, an the nourishment of the young.

3. Instinctive propensities, the object of which is the advantage of th race, or of the animal creation generally, rather than of the individu or his progeny; together with some of which the object is still obscur Among these may be mentioned the instinct of *congregation*, the *at tachments* of animals to man, the tendency to *imitation*, &c.

We shall now offer a few remarks on the correspondence between th instincts of animals, and those tendencies to particular modes of actio in the human mind, which go under the name of animal propensitie passions, and moral feelings. In regard to some of these there can b no kind of doubt. That the tendency to obtain food is an *instinct* i man, and parallel in its nature to that which prompts so many of th actions of the lower animals, will, we think, be generally admitted. this be granted, we may take it as the basis of our enquiries into th nature of instinctive actions in general. If we analyze the train of ac tions to which it gives rise in adult man, we see that, like an emotion the mind, it operates upon the reason, and sets it to work to provide th means of gratification. The present sense of hunger, caused by the con dition of the stomach and of the corporeal system at large, acting upo the reason, and thus through the will, gives rise to those movements whic convey into the stomach the food placed before the individual; and th anticipation of his future wants causes him to exert his whole psychic powers to provide the means of supplying them. But is it so in th infant? Very far from it. The intellectual powers are not yet deve loped by active employment. Experience has not given to the mind an data upon which to work. And, if the Creator had not provided a muc more ready and unerring mode of supplying the wants of the corpore system, it must speedily perish. The infant is purely under the guidan of instinct. When the system requires food, the contact of the nippl or of any soft substance resembling it, with the lips is sufficient to oc casion the action of sucking; and when the stomach is satisfied th action ceases. Now it has been sufficiently proved by observation an experiment, that the *brain* is not necessary to this action; but that it i one of those denominated by Dr. M. Hall, excito-motor, or reflex, de pending upon the spinal cord and its nerves only. Can we, then, ca such an action *voluntary*? Have we not sufficient evidence that a *voluntary* action originates in the brain? On this point we feel entirel satisfied. Now, if there are any animals whose instincts predominate s completely over the reasoning powers as they do in the human infan we think it is equally evident that the actions prompted by them ar involuntary. This is especially the case in insects; and there it is ob served that many of the actions peculiar to the species are performe after the cephalic ganglia have been removed, as perfectly as they ca be expected to do when no longer guided by the special sensations trans mitted through them. But, we again repeat, in proportion as the rea soning powers predominate, will the actions stimulated by the instinctiv propensity become of a voluntary character. It is quite possible eve by them, however, for the propensity to exist in such an exaggerated for as, for the time at least, to overcome the will, even where that is stron and formed upon the basis of a sound judgment. This has been the ca

in nymphomania and satyriasis; perhaps also in bulimia: the stimulus arising from the excited state of the generative or digestive system overpowering all voluntary efforts to control the actions, just as the respiratory stimulus will do, when temporarily checked in its operations. This view we deem of some importance in regard to the philosophy of insanity; since it is evident that it applies to many other propensities besides those we have quoted.

The connexion of the passions and emotions with the instinctive propensities is sufficiently apparent. The lower kinds of the former, such as anger, fear, &c. border very closely upon the latter, and, like them, can operate immediately upon the corporeal system, without the intervention of the will. One of the instances quoted by Dr. Alison, as an example of the instinct of self-preservation, will show how closely the two classes are blended. We refer to the discharge, by the cuttle-fish, of the contents of its inkbag, when pursued; by which the water around it is so deeply coloured, that it escapes under a cloud. Now it will scarcely be doubted that this is an instinctive rather than an intelligent action; for if the animal had to learn it, the power would be of little use to him. From a comparison of the position and contents of its inkbag with the organs of other animals, we learn that it is in effect its urinary bladder. Now we know well that the discharge of this secretion, under the influence of fear, is a very common occurrence with many animals, and even with man; and personal experience may have assured some of our readers that the relaxation of the sphincters in this condition is completely involuntary.

The higher moral feelings would seem referrible to the same class; but they do not seem to have any *direct* power over the corporeal system. We might call them *mental instincts*. A closer analysis of these, distinguishing the different parts of the *morale*, according as they have or have not any immediate influence on the nerves, is much wanting. The deficiency will not long, we hope, remain unsupplied. The physician and metaphysician have worked in too great ignorance of each other's researches; and we do not regard it as a trifling benefit of the study of phrenology that it has tended to bring them together. But we are compelled to remark that, among the professed cultivators of the latter science, there are very few who give due weight to the results obtained by recent physiological investigations on the nervous system at large; and most of them seem to take it for granted that the brain does many things which are the real province of the spinal cord.

Rigidly maintaining, then, the essential superiority of the *intellect* of man over that of all other animals, it can scarcely be denied that the greater number of the *active* powers of the human mind, which furnish the chief motives to action, are on the same footing with those which operate on the lower animals. Not only are our appetites similar to theirs, but the greater number of the desires of which we are conscious are either shared with us by them, or at least would seem to belong to the same class as their instincts. "It is interesting," remarks Dr. Alison, "to reflect on the different powers, to the operation of which we can trace the unceasing changes continually taking place around us, and particularly on the gradation, and very gradual transition that may be observed, from those by which inanimate matter is continually moved and changed, up to those which emanate from the intellect of man."

We shall follow Dr. Alison's account of this gradation, with some modifications and additions of our own.

By the original impulse given to the world, and by the laws of gravitation and of motion impressed on all matter, the greater and more striking movements of the inanimate world around us are continually determined; and by the laws of chemistry, those movements are made subservient to constant changes in the composition of the organic world. Again, by the laws which were impressed on the lower class of living beings at the time of their introduction into the world, a constant succession of living vegetable structures is determined, merely by the agency of air and water, heat and light, on those already existing. The organized structures into which these elements are converted are found to exhibit properties peculiar to themselves; and among these is the power of contracting upon the application of a stimulus. In this manner are the sensible motions produced which are occasionally observed in plants. We shall, then, regard vegetables as peculiarly, but not entirely, constituting *the kingdom of organic life*. In immediate but still obscure connexion with the lowest of the vegetable creation are the lowest of the animal tribes. Here we see indications of the same simple contractility which plants enjoy, but having a still more important relation to the well-being of the individual; and in addition to the movements thus produced, we witness others which appear to be consequent upon sensations, although none which clearly evidence intelligence and design on the part of the animals themselves. Proceeding still higher, we observe these instinctive movements becoming more complex in their character, and more refined and special in their objects; until we arrive at the class of insects which, as already observed, seem to possess the highest amount of instinctive or intuitional faculties, of any animals known. But we by no means find *intelligence* increasing in the same ratio. On the contrary, it remains very low. On the other hand, we observe those motions which are produced only by the direct contractility of the tissues stimulated, bearing a smaller and smaller proportion to the whole; and at last restricted to the organic functions, with which they always remain associated. Ascending from the class of insects through the vertebrated series, we observe a gradually increasing development of the reasoning powers, or intelligence; and a gradually fading away of the instincts, which become subordinate to the higher psychical faculties; until in man, the pure uncontrolled operation of instinct is only seen in infancy, when these faculties are still dormant.

Now it is evident that the whole class of instinctive actions has for its sole object the maintenance of *animal life;* and this in combination with the power of locomotion, also peculiar to animals. It will be recollected that, in the hymenopterous insects, *both* these powers are developed in the highest degree. Hence we should be led to regard these as typical of the *kingdom of animal life;* and to regard *man's* place in that kingdom as not being at its head or in its centre, but as being at one of the extremes; at the point, in fact, at which we may believe it to touch another kingdom, one of *pure intelligence*. And as the infant, in its progress to the vigour of manhood, exhibits those phases of psychical development which may be traced in ascending the animal scale, so do we find that, in the still further progress of years, whilst the body decays, the spirit, rightly disciplined and steadily elevated, approaches

nearer and nearer to the type which we presume to consider as characterizing more exalted beings.

Let not these speculations be regarded as misplaced. They tend to show the true nobility of man's rational and moral nature; and the mode in which he may most effectually fulfil the ends for which his Creator designed him. From these alone he might learn the evil of yielding to the "fleshy lusts which war against the soul;" and the dignity of those pursuits which exercise the intellect, and expand and strengthen those lofty moral feelings which are peculiar to man.

Many other such thoughts are suggested to us by the concluding passages of Dr. Alison's very interesting article, in which the important question of "free will" is briefly but clearly treated. There are also some remarks upon Mr. Whewell's views in regard to the use of the study of final causes in physiology, which harmonize closely with those which we formerly made upon the same subject, (See Vol. V. p. 338, et seq.;) and also upon the value of the phenomena of instinct, as indications of design in the universe.

We have but a few words to say in conclusion on the two other books at the head of this article. If our readers should look into Dr. Bushnan's Treatise, with an expectation of finding the *philosophy* of the subject which it professes to unfold, we fear they will be disappointed. It contains much that is clever, but not much that is original. Often as the author has professedly quoted from the writings of his preceptor, Dr. Fletcher, we think that he has hardly sufficiently acknowledged his obligations to them; for the great bulk of his book seems to us elaborated from certain parts of Dr. F.'s Physiology, in which his peculiar opinions are set forth, with a very grievous mutilation of those arguments, by which that very clever writer often contrived to make the worse appear the better reason. If any of our readers wish to find a justification of our assertion, let them compare the corresponding chapters in the two works on Irritability and its Seat in Plants and Animals. The most original part of the book is that in which he undertakes to demolish Lord Brougham's arguments for the Immateriality of the Soul. On the distinctive attributes of instinct and reason, he expresses himself pretty clearly, and locates the former in the spinal cord, and the latter in the brain; in which we think he is not far from the truth. But he represents instinctive actions as always depending on sensation, which is incorrect, in the extended sense at least in which he uses the term instinct. He is a strenuous advocate for the concession of reasoning powers to the lower animals; and quotes Darwin's wasp, with a still fuller account of both flights, and of the mental operations which passed in its cephalic ganglia, than that which we have already noticed. There are many acute remarks in the book; but we would rather say that its author was clever, than that it was good.

Mr. Swainson's Treatise contains a very good compilation of the materials furnished by several authors, in regard to the habits and instincts of animals, arranged under their respective heads; and it is, therefore, a very useful work to those who stand in need of such information. Of the value of his general observations, however, we cannot say much. The following will serve as a specimen. "If we once allow the least degree of reason to the brute creation, we must concede a portion of it altogether incompatible with their situation."

ART. V.

1. *A Narrative of the Treatment experienced by a Gentleman, during a state of Mental Derangement; designed to explain the Causes and the Nature of Insanity, and to expose the injudicious conduct pursued towards many unfortunate sufferers under that calamity.—London*, 1838. 8vo, pp. 278.
2. *A Narrative of the Treatment experienced by a Gentleman, during a state of Mental Derangement; designed to explain the Causes and the Nature of Insanity, and to expose the injudicious conduct pursued towards many unfortunate sufferers under that calamity.* By JOHN PERCEVAL, Esq.—*London*, 1840. 8vo, pp. 430.
3. *Aphorisms on the Treatment and Management of the Insane; with Considerations on Public and Private Lunatic Asylums, pointing out the errors in the present system.* By J. G. MILLINGEN, M.D., Surgeon to the Forces; late Medical Superintendent of the County of Middlesex Pauper Lunatic Asylum at Hanwell, &c. &c.—*London*, 1840. 8vo, pp. 202.
4. *Bemerkungen über den Umgang mit Geistkrankheiten, für diejenigen die mit ihnen in Berührung kommen.* Von J. F. LEHMANN, Inhaber einer Privat-Heil-Irrenanstalt auf dem Wedding bei Berlin.—*Berlin*, 1837. 8vo, pp. 51.

Remarks on the conduct to be observed towards Lunatics by those who are in charge of them. By J. F. LEHMANN, Proprietor of a Private Lunatic Asylum at Wedding, near Berlin.—*Berlin*, 1837.

5. *Du Danger des Rigueurs Corporelles dans le Traitement de la Folie.* Par le Docteur BLANCHE, de Montmartre, Médecin des Hôpitaux de Paris (Section des Aliénés).—*Paris*, 1839. 8vo, pp. 63.

Of the Danger of Corporal Severities in the Treatment of Insanity. By Doctor BLANCHE, of Montmartre, Physician to the Hospitals of Paris (Section of Lunatics).—*Paris*, 1839.

6. *The Fifty-fifth Report of the Visiting Justices of the County Lunatic Asylum, at Hanwell; and Second Report of the Resident Physician* (Dr. CONOLLY).—*London*, 1840. 8vo, pp. 128.

IT is not often, we imagine, that opportunities present themselves of obtaining such detail of the thoughts, feelings, and propensities of a lunatic, or of an individual who has laboured under insanity as can be relied upon. Disinclination to look back upon so unhappy a period often prevents all allusion to it. Some remaining feebleness or irritability of mind also often renders it impossible to receive such statements as are made, without much abatement. The impressions of madness are sometimes fleeting, like those of dreams. The instances, above all, are few in which individuals of known vigour of mind have sustained an attack of insanity, and recovered so far as to refer to it with composure, as they would to any other kind of calamity; or to review the visitation philosophically, as a psychological phenomenon at once curious and terrible. No greatness of intellect enables its possessor to contemplate

its diminution with resignation. Other evils may be borne, but this is the compendium of all possible evils; and to have suffered it once, if only for a time, produces too painful a conviction of the insecurity of mental power for any mind to reflect upon without dread.

The celebrated Robert Hall is described by one of his biographers, after his first attack of insanity, which was but of six weeks' duration, as pale and emaciated, and his spirits broken by "severity and harshness," and as discovering to his friend, "with a heart full of injuries, all the secrets of his prison-house, with his feelings and usage at the time." We may learn something of these feelings, when we find such a man complaining that they "took away his watch" in the asylum to which he had been sent; but Robert Hall's own words have a claim to quotation:

"'Sir, they took my watch, and confined me in a place which overlooked the ward in which were a number of pauper lunatics practising all manner of ludicrous antics. Sir, this sight was enough to make me ten times worse; they were as mad as March hares; I was at times quite insensible. I don't believe Dr. ——— was aware how I was treated by a lazy keeper. Do you know, sir, to save himself a little time and trouble (being winter), the fellow came at five o'clock and fastened me down upon my bed, where I could not stir either hand or foot, till about eight o'clock the next morning. During this time I had many lucid intervals; he had no business to leave me, sir, so long, but it was to enable him to go away sooner. You cannot conceive the horror of my situation, when I found myself perfectly sensible.......Now, sir, I hope, if ever I am taken ill again, your will use all your influence to prevent my being sent there a second time. It is a very mistaken notion that severity is requisite. *Mild treatment, with proper restraint and kindness*, is all that is necessary in such cases. There is nothing so beneficial as private confinement, with proper medical attendance, to prevent abuses by unfeeling keepers.' Then, with great energy, he repeated the request, 'I must beg, sir, you will do all in your power to prevent such treatment in case of a relapse.'" (Green's Reminiscences of the Rev. Robert Hall.)

From such a man, these are affecting words; and those we are about to quote are even more so. Being complimented on a sermon he preached soon after his recovery, his observation was:

"'O do not say anything about it, sir; I shall never be the preacher I was. I find I have lost the principal faculty that distinguished my preaching, which was imagination; you know that was my forte, sir; all my imagination has been overstretched. You, with the rest of my friends, tell me that I was only seven weeks in confinement, and the date of the year corresponds, so that I am bound to believe you, but they have appeared to me like seven years. My mind was so excited, and my imagination so lively and active, that more ideas passed through my mind during those seven weeks than in any seven years of my life. Whatever I had obtained from reading or reflection was present to me; I had all my ideas at my fingers' ends, and could bring them to bear upon any subject.'" (Ibid.)

That Robert Hall's painful representation of his treatment in his first attack was correct is rendered probable by the very different account he gave of the mode in which he was treated in a second attack of his lamentable malady. His slightest observations derive importance from his character, from the known powers of his understanding, and from the circumstance that so accomplished a friend as Sir James Mackintosh ventured not only to correspond with him on the subject of his insanity,

but addressed to him, in reflections drawn from the greatness of the sufferer's intellectual character, the noblest topics of consolation ever applicable in such circumstances, and which were never before so eloquently applied.

The volumes placed first in the list at the head of this article, although the production of an individual of powers very inferior to Hall, are still not only very singular compositions, but far from being uninstructive. We must, however, chiefly restrict our observations to the first volume: the second, which followed it after a considerable interval, has more of repetition and useless detail, unmingled with the consistent and touching reflections which occur in the former. The chief value of the narrative and that which strongly recommends it to all who undertake to manage lunatics, is that it explains the imaginary reasons for many of the actions of the insane, otherwise not to be divined; and that it records the impression made on the mind, even in its disturbed state, by the treatment administered. The last we consider to be a point of the extreme value, and one which has never yet received adequate attention.

We shall not dwell on the innumerable details of which this melancholy story is composed; yet it is impossible to pass over the author's preliminary history, and not to lament that a young gentleman of good connexions, of cultivated mind, of tender conscience, and of an excitable imagination, should have been left to wander unguarded among the most ignorant and besotted male and female bigots that the kingdom was ever infested with. To mingle with these people, he left his college (Oxford); and his mind, already a little off its balance, was soon overthrown by the wildness, ignorance, and superstition to which it was recklessly exposed. At Port Glasgow he was privileged to hear a "manifestation of tongues," by which his unsettled brain was so agitated that he discoursed on religious matters at night in the traveller's room at the inn, over whiskey-toddy, with a "kind of frolicsome squire or laird," who felt or pretended to feel a sudden contrition; upon which the author of the book *sung* words of comfort to him, in an inspired way, and the squire, "awed into compunction," rose up and kissed him.

After this, his progress was rapid. When a clergyman was preparing to preach, and "the rest of the congregation were at peace," he was led, he says, to open his mouth, "and sing a part of a psalm." The clergyman descended from the pulpit, and tranquillized him; but his chief adviser touching this essay appears to have been a lady, who caused him to see his error by the clear method of showing that "we are not to speak altogether, but to command the spirits." Removal from such society to a lunatic house would have been a change very much for the better. But it was yet long before he was placed under proper protection or salutary guidance. Going to Dublin in this zealous and heated frame of mind, on the principle, one would think on which Hamlet was to be sent to England, his worst delusions were strengthened by the religious excesses of those with whom he associated. In a state of such excitement that he was continually attempting "utterances and singing;" conceived a red handkerchief given to him was blood, and a token of ill; and even wished to put his hand into the fire, persuaded that he might draw it out unhurt; he was still, apparently, left to his own management; and thus, by too common an error, the precious period of

incipient or of active disease was neglected. In such cases, the first neglect ever ushers in a long train of hurtful modes of subsequent treatment. The period of prevention being past, the poor lunatic is next pretty sure to be exposed to a prolonged punishment, for such it is, for evils which vigilant care might probably have prevented.

All the horrors of insanity soon came upon the unfortunate subject of the book. He awoke in the night with dreadful impressions; he heard a voice charging him with "disobedience to the faith, in taking the medicine overnight," by which he had rendered his salvation extremely difficult, by its effect upon his "spirits and humours." He was told that he could only be saved by being changed into a spiritual body, by a great fight, to take place in his mortal body between satan and Jesus. A spirit came upon him and guided his actions; he was placed in a fatiguing attitude, resting on his feet, his knees "drawn up and on his head," and made to swing his body from side to side without ceasing. Voices, and sounds as of clanking iron, and the breathings of great forge bellows, and the force of flames, were heard within and without him. After a long struggle, he considered his salvation to be completed; and that he was to proceed through the world "as an angel," and "in an extraordinary way, and by singing." At this time, however, a military friend, who appears to have seen that all this was less reasonable than it might appear to the zealots by whom the poor patient had been surrounded, interfered, and placed him under medical care.

The lamentable workings of a perverted piety were now most painfully endured. When he found his going out prevented by a stout man-servant on the landing, placed there by a physician, he declared himself a prophet; seized one of the man's arms, and desired it to wither; and, the effect not following, did not still mistrust "the doctrines," but thought himself mocked by the Almighty for his disobedience of them; and for not having waited for the spirit to guide his action when the word was spoken; and because he had seized the man's arm with the wrong hand. At one time he was desired by "voices" to sing, at another to pray, and by some maladroitness he misplaced every injunction, and as surely received, from "the voices," the most cutting and insulting reproaches. At last, in one of his mental conflicts, "hunted in every direction," his patience, he relates, gave way, and he "mentally cursed the Holy Trinity." He adds, "a cutting sense of my ingratitude, and deep grief, followed with mute despair." These passages are calculated to excite the sincerest pity. So much conscientious feeling, mingled with such wild disorder of the reason, called for the most judicious interposition; for the most delicate and temperate interference; for all that kindness could dictate, regulated by all that calm good sense could approve. It does not appear to us that it was the author's good fortune to meet with this kind of treatment. We know with what reserve the statement of an unhappy man whose reason has once been fully dethroned must be received concerning the circumstances which took place in the dark period of his intellect; but there are circumstances related in this work which bear the stamp of truth upon them, so as not to be mistaken, and which not only show to what degrading discipline he was subjected, (for that, being common, is of no account,) but how keenly, even in his abject

mental state, he suffered from useless or, as we deem, from ignorant and hurtful inflictions, from coarse and improper hands.

Little do those know who look with contempt or ridicule on the antics of the lunatic, what deep and serious meanings are sometimes attached to them, in his disordered mind. He is seen to limit his walk with undeviating scrupulosity to two or three flag-stones, or he takes up his position day by day in one corner, or he raps his forehead smartly with his palm some scores of times, and with unheard-of grimaces. For all this he may be scolded, or jostled about, or beaten; but this sagacious treatment has no influence on his prepossessions: it does but add to distraction a sense of burning insult. The lunatic becomes ferocious, and he is put into restraint; his voice becomes elevated, he raves, he despairs. All this seems in the ordinary course of things; and he dies as much a riddle as he lived.

Listen to our unfortunate author. The *voices* informed him that his conduct was owing to a spirit of mockery and blasphemy having possession of him, and that he must *redeem himself* and rid himself of these foul spirits. There was certainly something inconvenient in the mode in which he expected to effect this exorcism.

> "The way in which I was tempted to do this was by throwing myself on the top of my head backwards, and, so resting on the top of my head and on my feet alone, to turn from one side to the other until I had broken my neck. I suppose by this time I was already in a state of feverish delirium, but my good sense and prudence still refused to undertake this strange action."

Who can fail to see that there was still an opportunity of corroborating that sense and prudence, and of moderating that feverish delirium? But by the side of this accomplished afflicted man there was but a stout man-servant, whose weapons were those usually brought to bear on the insane, mere brute force and bodily restraint. "Had he not been there," says the patient, "I might by that time have been sound asleep." Yes, poor sufferer! or if, instead of the stout servant, there had been some kind physician or some Sister of Charity by thy side, or even some one of thine own rich and well-sheltered, and not unkind family, thy feverish brow might have been cooled, thy faltering reason steadied, and thy neck-breaking summersets driven out of thy perturbed head. The man-servant adopted the only alternative in his power: he procured another stout man, and, together, they forced the poor neck-breaker into a strait-waistcoat, and tied his legs to the bed-posts, and then all was quite comfortable. "I had no design," says the patient, looking back on these conflicts," to destroy myself; but I have often conjectured since, that God in his mercy may have meditated my self-destruction to save me from the horrors he foresaw preparing for me: they were great and intolerable, shocking in themselves, more shocking in my abandonment. I awoke from them as from the grave, to be cut off from all my tenderest ties." Whatever may be thought of these sentiments, there can be but one opinion of the treatment, of which the bare recollection suggests them.

Bound and helpless in his bed, the patient lay, as usual, much disinclined to speak; those about him little dreaming wherefore to them he uttered not a word. But the patient knew wherefore; and those who look and talk heedlessly at the bedsides of lunatics, disabled from ex-

pressive attitudes by strait-waistcoats, may not uselessly remember his reasons. "I was unable to explain myself," he says," "because Dr. —— was reputed to be an Unitarian, and therefore I conceived it impossible to make him credit the supernatural voices and agency under which I acted. His companion seemed so stupid, and so like a man of the world of a common and vague stamp of mind, that I thought it perhaps still more hopeless to address him." Two things may be gathered from these hasty remarks of the poor man in bonds; first, that the insane may yet require and be consoled by a manifestation of reasonable sympathy with their sorrows; and secondly, that men of the world, of a common and vague stamp of mind, are by no means qualified for this line of practice. "They remained," he adds, "about five or ten minutes on the left hand side of my bed, and then went away." He says he afterwards learned that the *second* doctor (he of the vague aspect) was a celebrated lunatic doctor, to whom alone he ascribes the being confined to his bed "in nearly one position for several days together, tied hand and foot, in a strait-waistcoat, and in a small and close room." His first physician, he thinks, would have reasoned and acted differently. "He would have said, 'whatever harm may be in him, or may arrive to him from his complaint, it cannot be greater than what will certainly happen if he be confined.'" We entirely agree with the lunatic; and we read with shuddering the natural detail of the results of a neglect of such reasonable considerations.

"My need of wholesome exercise and occupation was denied. My idleness of body and mind left me at the mercy of my delusions; my confined position increased or caused a state of fever, which brought on delirium, and they kept drenching my body to take away the evil which their system was continually exciting, and which ultimately triumphed completely over me. My want of exercise produced a deadly torpor in the moral functions of my mind, combined with the ruin of my spirits by their diet and medicines."

Now, although the diet and the medicines might be innocent of ruining the spirits of the patient, what can be said in defence of that denial of exercise of body and mind, that confined position, and that unnatural drenching thus made necessary? Where is the man who could lie thus constrained, heated, disregarded, and drenched for days and nights, without "torpor in the moral functions of the mind, or without absolute frenzy?" Physicians think too little of these things; but out of the mouths of the insane wisdom cries to them, and they would do well to hear it and profit by it. Poor fellow! his simple language has an irresistible force in it:

"Since boyhood I had never been confined to my bed for more than two or three days, nor to my room for so much as a week together, and, on an average, had never had less daily than three hours active exercise. Now, after a fortnight's confinement to my room, I was fastened on my bed, with the liberty of my arms and legs denied to me."

But perhaps there were reasons for this. By the patient's account there were two: One was, he confesses, that when set at liberty he strove to renew his summersets; a triumphant reason in the eyes of many lunatic doctors, perhaps, but not in ours. The other reason was, *to get water, for which he longed;* and in which he says, "I think I succeeded once." He had another reason, also; namely, that he wished to look out of the

window, thinking all his family were there to receive him. Under thi enlightened treatment he came, not unnaturally, to think himself "the one only being to be eternally damned, alone, in multiplied bodies, and in infinite solitude and darkness, and torments." We can scarcely wonde at this; doomed to be fastened down, kept hot, and debarred from cool ing drinks, in fever and in madness, night and day.

Yet all this time he was not fortunate enough to be insensible. Th mind was working, but unaided; struggling, but without a friend. Le any physician in his senses read the following passage, and ask himsel whether the patient's delusions might not have been in some degree re lieved by allowing him to look out of the window, as he so much desired and by calmly and soothingly taking the opportunity, which all wh know much of lunatics know is of occasional but of most precious occur rence, of sapping his insane belief, and confirming his sane but waverin doubts. By this method, also, all the exhausting and irritating struggle and perhaps all its consequences, might have been prevented.

"For I had a species of doubts; but no one who has not been deranged ca understand how dreadfully true a lunatic's insane imagination appears to him how slight his sane doubts. But I was not permitted to reach the window, an I was tied down again in bed; then my usual delusion came upon me, that was gifted with the power of an elephant to break my bands; and when I trie and found how futile were my efforts, I was told I did not choose to use th strength I had, from cowardice, or ingratitude, or laziness. On one occasion I remember, after my brother had come to attend me, a spirit came to me whilst was lying on my back, fatigued with my efforts to break the strait-waistcoat b forcing out my arms and elbows laterally, and said, '*Use thy strength, I will sho you how to do it.*' The spirit then guided my arms and my hands, and with m fingers *sought and scratched the seams of the waistcoat sleeves*, soon loosened them and I began tearing the seams asunder. The noise of the rending asunder, how ever, soon aroused my attendant; my strait-waistcoat was taken off, and m arms were crossed over my stomach in two heavy, hot, leather arm-pieces, whic were not taken off from me for good until I reached England. I feel thankfu now for their removal."

Think of these things, advocates of restraint! Ask yourselves whethe the evidence of those who have suffered these inflictions is not more to b depended upon, as to the effect of them, than the flippant sayings of me of the world, vague and common; and who, never missing their dail comforts, their dinner, their club, or their seat in the theatre, tell you as Dr. Millingen tells us in his Aphorisms, that the sovereignest thing o earth is a strait-waistcoat for an inward bruise; or, at least, that attempt to do without restraint are "speculative and peculative," and lead to "useless waste and dilapidation of property." They forget, good eas men, that this destruction might be prevented by efficient keepers i sufficient numbers. But *this*, also, is expensive; ruinous in privat houses, and not to be thought of for paupers. It is very true, but it also expensive to keep lunatics alive at all; and if cheapness is to b the criterion of excellence then let them all die, relieve the rich familie and relieve the much oppressed parishes, and put an end to the fond spe culation and peculation of treating them humanely.

There is something amusing, but not uncommon, in the history of re cent attempts to mitigate the treatment of lunatics by abolishing thes personal restraints. First, it is sneered at as a thing impossible; the

s reflected upon as not only visionary but dishonest—not merely spe-ative, but, by ingenious alliteration, peculative too; then its perfect icticability is granted, but its humanity gravely denied. At length it laimed as a virtue long practised, and in each asylum the strait-waist-it is disavowed, and the muff sacrificed to the public opinion; these appear, dive to the basement-story, not without inconsistent aspersions st on those who have forced them to this exercise of virtue. In the an time we trust, not too confidently, but still trust, that the abolition cruel and useless methods of personal coercion makes a steady and iet progress; reflecting on none save by contrast, and challenging ne save by example.

Those who have long floated, by the help of day and night restraints, a sea of madhouse troubles, are likely to sink when such bladders are thdrawn. A troublesome patient brings them to their wit's end! To continue common bonds and manacles calls for vigilance, for fore-ought, and for all the ingenuity of preventive treatment, assisted by ery practical means of diverting the mind from its haunting delusions. ese things require reflection; and, as reflection is more troublesome an the imposition of restraint, those unused to reflection will accord no ile of favour to them; yet the very victims of their indolence see what ight be done. Even the author of the work before us, on his restless lusive pillow, manacled hand and foot, and in a whole fortnight only ice, and that by a rare accident and happy sally, obtaining water to ink; even he, in a situation not favorable to tranquil meditation, could agine, and, being recovered, can recollect what might have been better an all this. Again we quote his own simple words:

"It may be asked, what course I would have had pursued towards me, seeing ere was such evident danger in leaving me at liberty? I answer, that my con-ict ought to have been tried in every situation compatible with my state; that ought to have been dressed, if I would not dress myself; that I should have en invited to walk up and down my room, if not quietly, in the same confine-ent as in bed; that, whilst implements that might do me hurt were re-oved, pens, pencils, books, &c. should have been supplied to me; that I ould have been placed in a hackney-coach, and driven for air and exercise wards the sea-shore, and round the outskirts of Dublin. Few can imagine e sense of thirst and eager desire for freshness of air, which the recollection f that time still excites in me. I do not recollect water having been presented me; if it was I systematically refused it, like everything else, and it was not rced on me like the medicine and broth. If I recollect correctly I got some ater after my brother's arrival, and he also brought me once some grapes, a w of which I ate in spite of my false conscience, and God knows how refresh-ig they were."

The injudicious language addressed to lunatics, even by those who ave the care of them, has often struck us with surprise. It shocks us o see a poor demented biped standing before his doctor, whilst the said loctor enumerates the said biped's faults, errors, weaknesses, and pecca-lilloes, with marginal remarks on the faults of his father, the errors of his nother, the weaknesses of his grandmother, and the peccadilloes of all his ther relatives. We have repeatedly seen those who have once attempted aicide thus reminded of the attempt, and their memories refreshed as to ll the particulars, and even shameful crimes spoken of as having been aid to the lunatic's charge, until we have observed all the blood in the

supposed insensible biped mounting up into his conscious face, and have expected, and almost hoped, that he would cut his reviler's story short by knocking him down. Smaller offences against the lunatic are perpetually committed; such as stirring him up on his mad subject; asking him questions, the replies to which are likely to manifest his delusions; staring at him as at a wild animal, or laughing in his face. The folly of all this is very great, but it renders a visit to a lunatic asylum amusing to people of small benevolence. Such a visit, by exhibiting the composure of weak minds under mild parental care, excluding all hurtful excitement, ought to gratify the noblest feelings of humanity, rather than to divert idle visitors, who come to see the asylum to-day because they visited the wild Indians yesterday. The subject of the narrative which now occupies us remarks of a relative, that he replied to him, when he spake according to his delusions, in an "ill-judged tone of levity, and as if speaking to a child, ridiculing the idea;" by which, he says, "his hopes of being comprehended were blighted." He reflected, he says, that his relatives should have remembered that it could be no light matter which had so changed him; and he is of opinion that if he had been reasoned with, the possibility of his inspirations admitted, but their probability questioned, he should have been rescued from his "dreadful situation and saved from ruin."

After a time, however, he found himself at liberty to get up and dress, and to help himself to breakfast. He was taken down stairs, put into a hackney-coach, driven to the quay, and put on board a Bristol packet, with his relative and the stout man-servant. Having a fancy for walking about in the packet (not very unnatural after a fortnight's attachment to the bedposts), he was made to go to bed again; "which, God knows," he says, "I had had enough of." He soon became again the sport of the wildest delusions; many of which are detailed, and some of which are exactly like long and vivid dreams. One of his unfortunate fancies was, that it was necessary for him to devote himself to death to save the ship; and, in such difficult circumstances, this notion was evidently incompatible with his being at liberty. On arriving at Bristol another doctor was introduced to him, who, as usual, ordered him *to bed.* "I would have given my hand to remain up; my bed was a scene of horrors to me. However, I made no reply, and to bed I went." Now, this is just the case to support the views of those who advocate the comfort of being strapped down in bed. What would you do? say they. Would you, instead of confining the patient by a gentle strap, have two strong men to sit by him and frighten him with their looks? We would do neither; neither have two stout servants instead of one, nor yet adopt the asserted milder scheme of strapping down the patient to his bed. What, then, *would* you do? We should *let him sit up.* Upon what possible ground of reason would you, *we* would ask, force a poor creature into his bed who would "give his hand" to remain up? Where is the humanity you boast of in thus confining him, by strap and chain, to a "scene of horrors." Such treatment is opposed to the first principles of the curative art in relation to irritated minds. It is foolish, and hurtful, and cruel, but then it is convenient and it is economical. If the patient sits up, somebody must sit up to watch him: an additional servant would be required. If you fasten him tightly in bed, the whole

house may be at ease about him. He may toss and groan, indeed, and suffer fever and thirst and many horrors, but he "can't get out;" so the small and inefficient establishment will not be incommoded. Until the proprietors of private establishments have the courage so to arrange their houses as to enable them to adopt a proper classification of their patients, and to separate the violent from the moderately tranquil, and both from the tranquil and convalescent, these inconveniences must attach to them; until the time comes when the public will bear them no longer.

After the period to which all the preceding remarks apply, the author of the work was removed to a private lunatic asylum, where at first he appears to have been free from personal restraint. But he represents himself as being without occupation, amusement, or variety, &c. When he had read the paper, he had nothing to do but to look out of the window, and when he had looked out of the window till he was tired, he had nothing to do but to read the paper again. The *spirits* were of course soon busy with him; many of them, and of contrary dispositions. When at dinner or at tea, some spirits would order him to eat this and to leave that, and others would order him to eat that and to leave this. Livelier spirits still would order him to waltz round the table; and then to make other patients waltz, though rheumatic and averse to such exercise; and then he would consider himself ordered to wrestle with them. And now an end. Left with patients duller than himself, no book, no amusement, no walk allowed, no attendant present, the sameness produces ennui, ennui leads to restlessness, restlessness finds relief in dancing, and nobody being there to keep this innocent activity within bounds, an attempt is made to excite the rheumatic, the helpless and the unwatched; and this brings on the *regular* crisis, namely, the hasty entry of two keepers, interrupted in their whist perhaps, poor men, or over their afternoon beer, and all heated and affrighted. What do they do? Calm the dancer, and protect the rheumatic, and divert the troubled mind by amusement or by soothing words, or a walk in the fresh air? None of these things. All these modes of succour are neglected at last, just as the prevention of all that calls for them was neglected at first. But the dancer is thrown upon a sofa and forced into a strait-waistcoat. *Cœlum non animum*: he had crossed the channel, but was still in a region where strait-waistcoats flourished, ever ready in the hands of strong men. "*Thus,*" he says, "*commenced my second ruin.*" Up to this time, he says, he did not know that he was insane. He also complains that he was taken to the private lunatic house without any courtesy or respect, and turned into the house without the ceremony of an introduction. From these observations may be gathered the desirableness of making a lunatic early acquainted with the nature of his malady, in order to abate his self-confidence; and secondly, the importance of treating him with decent respect, adapted to his station in society when in health. It shocks us to see a grisly, ragged man, called up, as it would seem, from labouring work, that we may learn it is or *was* a man of literary fame, or Sir so and so, or a man of higher degree. The dress of such persons should be scrupulously attended to, their occupation regulated by their own taste, and their manners, if possible, kept from sinking into slovenly negligence,

by the most unvarying politeness towards them of those who have undertaken to repair their minds and restore them to society. A lunatic, whatever his rank, commonly requires twice as much attention as when he was sane. In a private lunatic house he is usually bereft of two thirds of the attention which he was accustomed to at home. It is quite visionary to talk of preserving this inattention, which is so cheap and convenient, without personal restraints. Tear up strait-waistcoats, take aching hands out of muffs, remove the nightly handcuff and the daily leg-lock, and there must be more servants, more attendants in every asylum, and of a different kind, not merely muscular, but with some intelligence; persons less prone to punish than active to prevent; less disposed to visit dancing with severity than to keep it within salutary bounds; governing, in short, less by the arms and the legs, and more by the brain. This will not be allowed without a struggle. It would be ruinous to the lunatic housekeeper; the present system only ruins the patients.

That the unfortunate author of this work was a most troublesome inmate we learn from his own pages; and that, considering his great restlessness and activity, he was for the most part treated with consideration and forbearance. Yet the description he gives of his being fastened in a sort of restraint-chair every day, with no amusement but to rub and flatten his nose against the wall; and his account of the manner which he was forced into the bath, and occasionally corrected for his strange fancies, sufficiently show that the treatment was extremely defective. Medicine might be given, and proper food, but cheerful recreative exercise seems to have been much neglected, and the closest and most miserable form of complicated restraint, (sitting in a restraint-chair in a strait-waistcoat, and the feet fastened to the floor,) resorted to for a period which no circumstances could render necessary, or even justify. He seems also to have worn the strait-waistcoat in bed, and to have had his hands strapped to the sides of the bedstead, and his feet fastened to the bottom of it. This is in fact, or was, an ordinary method of restraint in old institutions; very convenient to keepers and nurses, but often utterly destructive of the patient. "Fastened thus, lying on my back, I passed my wretched sleepless nights for nearly, if not quite, nine months!" We do not hesitate to state our belief that there never was a case which required this treatment; never a case in which it would not be mischievous. "I had not exercise enough during the day to procure sleep, but I lay exhausted, wearied, agonied, terrified in my spirits, hungering after rest, but unable to procure it." Most true, and terrible, and instructive commentary! No doubt the humane head of the establishment would have put an end to this if he had not been misinformed; but when he was asked by the patient to be allowed to sleep unfastened, the keeper took up his voice, and preventing any words of mercy, "said in an off-hand, impertinent manner, 'O! no, sir, there's no trusting him,' &c. &c., and I remained silent from resentment." This is the usual course of things. First, the head of the house has no very distinct idea what extent of restraint is nightly employed; it is very difficult to ascertain it if nurses and keepers are cunning and cruel; and when a petition *is* put up for liberty, he is then

told that he *may* liberate the patient if he chooses, of course; but in that case the house will be upset, or the nurse or keeper must *leave;* and although this leaving is commonly at some subsequent period found to be for the common advantage of patients and of master, the sense of this blessing is not yet awakened, and the idea of losing a vigilant guardian is full of terror. Thus is the lowest tyranny long confirmed; lunatics, like dead men, tell no tales; or if they do mingle with their fanciful complaints some miserable truths, they attract no attention, and occasion the severest keeper no discomposure.

The besetting evil of lunatic asylums is, that every lunatic patient is of necessity made to associate with many other lunatics. If occasionally useful, we cannot but think this practice must often be injurious. The author of the narrative complains much of having been put into a room with eleven others, "certain of them occasionally raving, stamping, bawling, violent madmen." He says that the mental agony, the distress, and the actions of these men, their quarrels with one another, and with the servants, and the rude and cruel manners of the servants occasioned him the greatest distress. Amongst these he was confined hand and foot, *for six months.* He was often left with two or three of them for hours. Indeed, if our object was to make out a strong case against lunatic asylums, we might quote a hundred different passages, of the truth of which we entertain no doubt, and illustrative of the errors which creep, by negligence, into houses, affording in most respects superior accommodations. It is not so much these errors, cruel as some of them are, as the effects of them upon the lunatic's mind, which we wish to point out. These effects show in the most convincing manner how unsalutary, to say the least of them, these yet too common errors are. Here is a book, evidently written before the author's mind had quite regained its health, and yet in which are depicted the most poignant feelings of shame and mortification, occasioned by his finding himself deserted by his family, severed from his friends, deprived of any confidential ear, or of any spiritual comfort and consolation, and exposed, without scruple, question, or explanation, to the most abject treatment in rough and servile hands. We agree with him that a man of his rank, acquirements, and connexions, should have been more considerately managed; and we remark, with pain, that the want of accustomed respects, and of gentlemanly care and attention, added in no trifling degree to the discomforts of his state of trial. He has also recorded many of the ordinary feelings incidental to lunatics of every class; and we think words like the following must fall impressively on the ears of all who have insane persons under their care:

"They who have not been confined in a lunatic asylum, cannot conceive the dreadful and cruel suspense that delay, and not only neglect but the refusal of every-day civilities, together with inattention to just and obvious complaints, occasion. They do not know our wants and fears, because they do not know the danger we are in. They may judge our danger, however, from what these men do; and from what they have done, they may judge what they dare to do; being encompassed, even more than a king, with a hollow impunity, and clothed in the deepest hypocrisy. They who have not endured this confinement do not know how the very suspicion of being a lunatic, coupled with being cut off from all pecuniary resources, shuts the minds of others against sympathy, impedes the proffer of assistance and the exercise of protection,

and aught but the show of pity. Neither how it embarrasses the suppliant in his applications for redress, awakens anxiety, excites mistrust, and closes the door of his hopes, whilst he finds himself left defenceless to the sarcasm and persecutions of those he is accusing. .. .. How much more fearful is such a trial for one who knows that he cannot plead innocence of lunacy; one who, in mind and bodily health, is weak, and thereby more exposed than another to follow a wrong course; exposed to suffer even from treatment which men in sound health might almost laugh at, still more from that which he dreads from having experienced it, and against which he is exasperated; and also, still more liable than the other to lose that gift, lately lost, so dear now, being newly restored to him, the gift of a sound mind and convalescent health; perishing again from want of wholesome communion, shattered by assault, or insidiously undermined."

To us there is a solemn eloquence in these passages, which it would disgrace us to be insensible to. We are by many things convinced that an important era in the management of insanity is at hand. Pinel struck off the chains from all the lunatics in the Bicêtre, in the period of the French Revolution; but the example was well-nigh lost. Tuke, with no less moral courage, established an institution at York, on principles of the purest humanity, and opposed to every prejudice then prevalent and powerful. Beams of accidental light broke in on Bethlem; and riveted fetters fell off,—were even struck off by woman's hands, to be imposed no more. The occupation of lunatics in various works mentioned, and partly put in practice, by Pinel, adopted in Germany, and forcibly recommended by Tuke, was tried to a great extent at Wakefield, and subsequently at Hanwell by the late Sir William Ellis. A great diminution of restraints took place at Nottingham and in other provincial asylums; and their complete disuse was tried with success at Lincoln; an experiment too important not to need the test of a larger institution, which test has been for fifteen months afforded at Hanwell, one of the largest and perhaps, taking it altogether, the finest establishments for the insane in the world. This is an extraordinary change. That it should prove difficult, attempted as it is in buildings constructed solely with a view to the old physical treatment, and filled with officers and attendants to whom the substitution of what may be called the mental in place of the physical treatment is a novelty, full of obvious inconveniences, and promising only remote and refined advantage, is not a matter of surprise. That it should ever be interrupted, suspended, delayed for another century would scarcely surprise us. At Lincoln, without any reasonable cause yet communicated to the public, restraints are said to have been resumed by one set of officers after a long discontinuance by others. At Northampton, they have been wholly laid aside. Whatever accident occurs will of course be at first ascribed to the want of straps and chains. Not only every broken window, and every torn blanket, every bruised face, every blow made in sudden passion, and every attempt to climb a wall and see the cheerful world beyond, will be ascribed to the want of manacles, and aggravate and justify the woe of those who weep over the memory of restraints dear to the tyrant heart; but any case of suicide will be set down to it although suicidal patients were scarcely ever in restraint, and if it should happen that some benevolent and fearless matron or doctor should be killed outright, we may yet behold the triumphant resurrection of all the

implements of horrible coercion. Resurrection, do we say? There are still in many *English* Lunatic Asylums miserable objects in cruel gyves and restraints which have been worn *for years;* and in some cases so long that no one survives in the asylum to tell the first occasion of them. These things, we acknowledge, exceed belief, but *we know them to be true.* In the mean time, every hour that passes, but much more every week and every month, supports the humane experiment going on in Middlesex; and we trust that the great Creator whose sun rises and sets on Hanwell will permit it still for many years to rise and set on a house undisturbed by any accident which may be converted into an apology for a revival of cruelty.

We should be extremely sorry to do injustice to Dr. Millingen, whom we take to be a lively author, not much given to serious reflection; and whose work, consisting partly of paradoxes and partly of aphorisms, seems to have been composed for the sake, among other things, of carrying certain satirical foot-notes, and an oddly-conceived preface, scarcely serious, we imagine, or more than a burlesque, by a witty writer of the grand style. But even trivial and jesting objections to the removal of old corruptions are ever welcome to those who wish to preserve them. For this reason we shall here introduce Dr. Millingen's authority in favour of those restraints which we presume he practised when lately at the head of the largest lunatic asylum in Britain. This is his grave and, we are to conclude, well-considered aphorism :

"257. Except in cases of violent mania, restraint is rarely necessary, unless it be to prevent the mischievous idiot and the maniac from destroying property, when gentle restraint is required to prevent them from constantly tearing their clothes and bedding, or breaking the window-panes, or anything they can lay hold of. It may, however, be occasionally employed as a punishment, the dread of which keeps many lunatics in order."

To this sagacious aphorism is appended a recent note, the offspring of circumstances not clearly foreseen, we imagine, when the aphorism was elaborated. This is the note :

"Nothing can be more absurd, speculative, or peculative than the attempt of theoretic visionaries, or candidates for popular praise, to do away with all restraint. Desirable as such a management might be, it can never prevail without much danger to personal security, and a useless waste and dilapidation of property."

The personal character of several portions of Dr. Millingen's work would make it mere affectation in us to deny that we consider these passages to have been modified by his own aggrieved feelings. His preface alludes to "labours and sufferings;" he seems to expect the most virulent hostility in various quarters; he vents much wrath on justices of the peace; and in his attacks on matrons of asylums, he seems almost to forget not only what is due to those who undertake those truly heroic as well as philanthropic duties, but what is due from a gallant man to all women—matrons, wives, maids, or widows. Can we wonder, then, that this 257th aphorism is full of contradictions, or that the note to it is also full of contradictions, or even that aphorism 257 is contradicted by an aphorism in the next page, namely, aphorism 260. Aphorism 257

says that, except in cases of violent mania, restraint is rarely necessary; except also in the mischievous idiot and maniac in whom it is *constantly* necessary. The note says, attempts to do away with all restraint are *absurd*, speculative, and *peculative*, but also *desirable;* that they are the attempts of theoretic visionaries, and of candidates for popular praise, leading to much danger to personal security and a useless waste of property, but *desirable*. To complete the measure of contradictions, this great 257th aphorism sanctions the use of restraint as a punishment, "the dread of which keeps many lunatics in order;" yet in the very face of this, on the opposite page, written by the same hand, stands aphorism 260, which propounds with no less authority that "the influence of *fear* will be found to *aggravate* lunacy instead of keeping the patient under proper discipline. The utmost kindness will be found far preferable," &c.

These are but the struggles of constitutional good nature, unaided by steady reflection or earnestness of purpose, with prejudices fomented by vexatious circumstances. It is but right to say that Dr. Millingen objects to strait-waistcoats; but, alas! by what seems a constitutional inconsistency, he lauds sleeves of canvass, by which the arms, he says, "are kept loose alongside of the body;" and he magnifies the leather muffs, "which only," he assures us, "confine the hands;" and are consequently useful with those who, he tells us, "are always tearing or destroying their clothes or bedding;" and who, of course, are always to be in muffs. But of all devices the restraint-chair is his favorite: "the patients have the free use of their arms and hands, and are *only* prevented from roving about, and committing mischief." Who would think that this elegant contrivance was a large deal watch-box, pierced as a close-stool, in and upon which the patient was placed in the morning to remain there till evening, secured in the chair by straps passed through holes in the back of the chair and locked! Rows of these were once to be seen in most asylums. Thirty-five of these were insufficient for Dr. Millingen's enlarged views at Hanwell, and forty-one were at length at his command, in wards where now not one is to be seen. They now form a floor for the carpenter's shop; two or three being preserved as curiosities. The forty-one cost between two and three hundred pounds, which might have been expended with more wholesome effect in wages to a few additional attendants. The expense of these disgusting chairs, now avoided, may at all events form a set-off against that dilapidation of blankets which Dr. Millingen so sincerely appears to lament. And as Dr. Millingen is a gentleman of some pretensions to literature, although hitherto in paths rather curious than useful, we presume to recommend to him a certain Report of the Commissioners for Inquiry concerning Charities, in which at page 350, he will see that in an ancient institution, conducted on the principles of which he so much approves, and containing nothing speculative or peculative, "*the destruction of the blankets is exceedingly great:*" so that something more than strait-waistcoats and muffs, and chairs of restraint, is required for the preservation of bedding.

But one word more justice requires from us. As Dr. Millingen lends the whole weight of his authority, derived from a year's residence at Hanwell, to maintaining a system of personal restraint, it is right to look into the effects of his government when there, in public and uncontra-

dicted documents. After time had been afforded to test the efficacy of Dr. Millingen's restraint system, the Magistrates' Report states (49th Report), that "a relaxed state of discipline and disorder were found to prevail to such an extent as must seriously have injured the character and efficiency of the establishment, had they been allowed to continue." In less than a year afterward, the restraint-system having been wholly discontinued, the magistrates report (52d Report) that "better order and more continued decorum have been maintained; and although occasional outbursts of violence are, from the capricious nature of the disease, unavoidable, they have become rarer, and the whole establishment has less of the characteristic features of a lunatic asylum." To this testimony no reasonable exception can be taken. The present resident physician may be a partisan, or he may look for some credit for his system; he may be vain or prejudiced, or he may conceal the disadvantages of his darling plan. We put him quite out of the question. But we have here the deliberate opinion of the magistrates, who are exposed to obloquy if anything goes wrong; and who, when windows are broken or blankets are torn, have to perform the unpopular task of raising money from the county to repair the damages.

Notwithstanding all this, we are convinced that there is a fund of kindness in Dr. Millingen's disposition: but as his government of Hanwell indicated how little kindness can effect if not regulated by reflection and by systematic habits, so his book shows how little it can oppose the pettiness of personal feeling. There are many passages, especially in the notes to the Aphorisms, the only object of which seems to be to wound some one or other whom he does not love. If quite purged from these defacements, many of the aphorisms might be read with pleasure, and some with advantage. As it is, the work is scarcely worthy of Dr. Millingen's reputation. The oddest fancy haunts Dr. Millingen, namely, that a proposal, made by him, as he says, but, with his usual inconsistency, also ascribed to Dr. Browne, of Dumfries, *that all lunatic establishments should be placed under the control of government*, will subject him to martyrdom; the truth all the time being that his proposition is neither his nor Dr. Browne's; but was made, and printed, and published, and read by all readers, full eleven years ago. Deducting these and other defects, there are many useful observations in the Aphorisms; certainly not all, or many of them, original, but all or most of them worthy of remembrance. With these, however, are intermixed some singular remarks of the author himself. In aphorism 76, it is observed that the complication of paralysis with insanity appears to be much more frequent on the continent than in England, "especially in France;" and this is partly ascribed by Dr. Millingen "to the practice of the French, who carry burthens on their backs and loins instead of their shoulders and head." We are inclined to question the correctness of the statement altogether; particularly as we see that Dr. Millingen states, that out of 1000 patients at Hanwell, only twelve were paralytic; and yet find in a Report of the Hanwell Asylum, made a few months after Dr. Millingen left that institution, that out of 805 cases twenty-three were complicated with paralysis. We are even of opinion that the instances of slight paralysis are in greater proportion; and that the insanity is, in many cases,

merely the consequence of a paralyzed condition of part of the brain. The phenomena of these cases are sometimes not very striking; the lesion seems slight, the impairment of mind inconsiderable; but the disease is incurable, and the mind is irretrievably ruined.

In noticing the moral causes of insanity, Dr. Millingen arranges them in the following order: 1, Pride; 2, Fear; 3, Fright; 4, Ambition; 5, Loss of property; 6, Domestic cares. And he immediately and characteristically adds: "Of these, domestic cares are the most active;" so that this class should have been the first instead of the last on the list. The Fifty-first Hanwell Report states that the causes are, "in a great number of instances, referrible to poverty, and the general struggle of the poorer and middle classes for existence."

We are staggered (perhaps in consequence of our ignorance) by aphorism 193: "Whatever may be the cause of the phenomenon, when demonomaniac women are plunged into a bath, and invariably rise to the surface of the water, the case may be considered of an obstinate nature." We should be glad to know on what authority this phenomenon of obstinate resistance to drowning rests. Is it to be found anywhere except in the annals of witchcraft, or in that curious department of literature in which we humbly acknowledge ourselves to be immeasurably Dr. Millingen's inferiors?

The notices of suicide in the 205th aphorism are very interesting:

> "The propensity to commit suicide is more generally observed between the ages of twenty and thirty, and men are more liable to its fatal influence than women, in the proportion of three to one. Men have recourse to the halter, the pistol, or the knife; women to drowning, hanging, starvation, and poison. Most of these unfortunates make use of the ready means afforded to them by their trade: thus a barber will cut his throat with his razor; a surgeon will open an artery with a scalpel; a shoemaker with his leather-cutter; a soldier will use his musket or his pistol, a sportsman his fowling-piece." (p. 205.)

Without vouching for the absolute correctness of all these particulars, we may gather this useful knowledge from their general tenor, that the comparative immunity of public institutions from these distressing accidents is in a great measure to be ascribed to the absence of opportunities suggestive of horrible temptation. The importance, as a preventive of suicide, of promoting a cheerful and hopeful character throughout a lunatic house, will scarcely, we suppose, be disputed; but such a character can never be maintained in houses where restraints are frequently resorted to; and where, consequently, some are punished, and more are frightened and agitated every day.

With the exception of passages relative to restraint, on which we have already animadverted, the section on treatment contains very interesting and even valuable matter. The importance of classification, of employment, of amusement, of kind conduct, of judicious conversation, and the desirableness of avoiding excitement, irritation, or any kind of irregularity, are extremely well enforced. Dr. Millingen entertains perhaps an excessive jealousy of *clerical* interference in an asylum; and states that out of 1000 patients at Hanwell, he had only *four* "who were fit to receive religious consolation in sickness and on the bed of death." Such an

observation scarcely seems consistent with the fact, that between two and three hundred lunatics attend the chapel at the asylum at Hanwell every Sunday; yet we believe it to be nearly correct; attendance at chapel is the restoration of an old habit, agreeable to the patient. Private conversation with a minister is not one of their habits, except in the emergencies of sickness; and to propose it, startles and often offends them in other circumstances. We agree with him in thinking that mistaken zeal may do much harm, and that great judgment is required in the sermons preached to lunatics; but are not at all disposed to think permission to partake of the sacrament, "a wild absurdity of bigots;" or that "any patient fit to participate in this service is fit to be discharged."

In the observations on the physical treatment and on the diet of lunatics, will be found many remarks deserving of attention. Dr. Millingen agrees with most of the authorities on the treatment of insanity in considering general bloodletting, seldom admissible. Of local bleeding he thinks highly. When there is "much excitement and increased action of the heart and arteries," *aconite* in fractional doses will, he says, produce a state of calmness more rapidly than detraction of blood. No particulars are given, and there is nothing very lucid in a note to this aphorism, stating, "the endermic inspersion should always be aided by counter-irritation in the intestinal tube;" a sentence as little translated, and as barefacedly "taken from the French," as "a day room *giving* to the garden," and an opening "*practised* in every cell door," and other peculiarities in Dr. Millingen's style. If the endermic *inspersion* refers to the aconite, what do the fractional *doses* mean? And what may uneducated persons like ourselves understand by counter-irritation *in* the intestinal tube, to antagonise these inspersions or these fractional administrations?

The aphorism p. 290, sets forth that "great excitement frequently arises from the want of sleep;" a sentence which we should rather read backwards, saying, "the want of sleep arises frequently from great excitement;" and in this case Dr. Millingen recommends the salt of morphia "according to the endermic method," after slight vesication. Cold applications to the head are advised when there is considerable determination to the brain, and slight pressure upon the carotid arteries. A *yoke* is suggested for this purpose, which we should very much like to see. Its application, it is candidly acknowledged, would be incommodious. Nevertheless, Dr. Millingen assures us that "arterial pressure is a powerful agent in *brainular* excitement, which in insanity, is mostly of a transient nature." Emetics are recommended in cases of melancholia, connected with dyspepsia; and small doses of the tartrate of antimony are mentioned as most efficient in mania. When speaking of purgatives, Dr. Millingen says that croton oil should be prescribed with reserve, as he has "frequently observed increased cerebral excitement with stupor after its administration." In dyspeptic and melancholy cases the compound rhubarb pill, given repeatedly and in small doses, is mentioned as "most effectual;" and castor oil stigmatized as injurious, and sometimes productive of fatal diarrhœa.

We have anxiously read the aphorisms concerning opium, narcotics,

and sedatives; being convinced of their efficacy in some cases, of their inutility in others, and of the difficulty of saying, without trial, whether they will do good or harm. Dr. Millingen says opium is injurious in cases of cerebral congestion and great vascular action, "as it will often occasion phrenitis;" and that it may be given with safety when there is "*no* great excitement," &c. This is much too vague. In cases of no great excitement it is not required; and the truth is, that in some cases of great excitement it is strikingly useful. Dr. Millingen prefers the endermic method of applying it in sleepless patients. When given internally, he prefers Battley's solution. His experience of hyoscyamus, belladonna, and conium, is in favour of belladonna. He certainly underrates the hyoscyamus, but it requires to be given in a full dose; two drachms of the tincture will in many cases be found very useful. We partake of Dr. Millingen's unfavorable opinion of digitalis. Its bad effects are soon produced, and benefit seldom attends them. The remarks on counter-irritants are very brief; and the recommendation of the actual cautery in cases of demonomania,—an iron of a globular form, at a white heat, applied to the mastoid process, however ingenious, is not likely often to be practised.

Dr. Millingen's work is concluded by a section on the construction and distribution of public asylums, pervaded by one great fault, that it contemplates the perpetuation of restraints, the discontinuance of which will necessarily suggest many improvements on the old plans of asylums. It is only when an attempt is made to diminish or abolish restraints, that the physician finds the arrangements of the whole house have been modified by habitual dependence on the means without the aid of which he is endeavouring to regulate it. Dr. Millingen justly objects to long galleries, as opposed to classification, particularly when directly communicating with one another: and recommends detached buildings. The inconveniences of detached buildings, are at the same time obvious; but in proportion to the size of a gallery or ward, the chances of disturbance and confusion must always increase. The massive bedsteads praised by Dr. Millingen are useless, at least if made of iron. Massive as they are, they can be moved by a patient, and their coldness is extreme. Wooden beds fastened to the floor, answer every purpose for the refractory; and light iron bedsteads are well suited to other patients. The following axioms are deduced from Hanwell under Dr. Millingen's system:

> .... "On each side of the bed should be slides for passing straps and buckles to keep down the bedding, and *enforce quiet*. To the bedsteads of epileptic patients should be fixed a strap and buckle, to secure one of their hands during the night—a precaution highly necessary, as these unfortunates will *not unfrequently* turn upon their faces and be smothered. Epileptic patients are themselves so well aware of the necessity of this precautionary measure, that they will *often* request the keeper to confine one of their hands at bedtime."

This is merely a revival of Sir William Ellis's notion. At all events, at Hanwell this "highly-necessary" precaution has been discontinued entirely; and of one hundred of these "unfortunates" who used to be strapped to the bed every night in the year, we do not learn that any one has yet petitioned for a renewal of a custom, which neither permitted them to lie comfortably on the right side nor on the left.

M. Lehmann's little work is written in an animated strain. He is not a medical man, and the severe notions of discipline with which he sets out prepare the reader to find him the advocate of terrors; for he censures Dr. Bräunlich for asserting that lunatic patients are not responsible, and therefore ought not to be punished. M. Lehmann thinks the same may be said of a child; and, assuming that punishment is excellent for children (a very disputable notion), he argues that, therefore, it is good for lunatics. Poor as such an argument is, we believe that no better one can be adduced for the practice. In both instances (in that of the helpless lunatic and in that of the defenceless child), a humaner view would take in the possible good effects of prevention. But punishment, like revenge, is sweet; and will not yet be abandoned either in the madhouse, or in the schoolroom, where it prepares subjects for the madhouse, until after many struggles.

Nevertheless, the author elsewhere speaks as a humane man. He takes, indeed, one would sometimes suppose, elevated views of his duty, and eloquently enforces the truths that "love produces love, and esteem, esteem;" that the superintendent is placed in his position for the patients, and the patients are not placed there for him; that these poor creatures are still the image of God, and deserving of sympathy; and, finally, "are unfortunate and not guilty." "The first essential in a superintendent or a keeper," he says, "is to endeavour to acquire the confidence of the patients;" an opinion in which we cordially concur, but the value of which has been surely overlooked by those whose first actions towards the patients are those of violence. He justly points out the importance of studying the character of each patient, in order to effect a useful classification; but, unless patients are treated with the greatest consideration and delicacy on admission, we do not believe that those who have the charge of them ever become acquainted with their real characters. There are bodily maladies which are the joint product of bodily disease and medicine, and we suspect that certain phenomena ascribed to madness have been the joint result of disease and treatment.

The observations of M. Lehmann on the qualifications of the attendants, who are always with the patients, are generally judicious. He thinks it less desirable to give a flat contradiction to a patient's erroneous notions than to attend a little to them, so that the patient may not think himself neglected. Sometimes he has competed with them in their own line; as with a tailor in poetic compositions: it appears that the tailor, although he did not think so highly of M. Lehmann's performance as of his own, was gratified to think that he did not disdain the muses. The necessity of perfect good sense, good temper, and all the other good qualities which, if possible, the guardians of lunatics should possess, is expressively conveyed by M. Lehmann in the maxim, that they should be *soul-sound;* for, although it may not be true that he "who drives fat oxen should himself be fat," it is most true that he who undertakes to regulate the minds of others should be especially careful of his own. He very properly reprobates the carelessness with which abandoned or dissolute characters, fit for nothing else, are thought fit to be intrusted as the keepers of insane patients.

There is surely a providential inconsistency in those who advocate severities. We have seen some edifying examples of this in Dr. Millingen's Aphorisms; and M. Lehmann's pages present others. After finding fault with Dr. Bräunlich for advocating the immunity of lunatics from punishment, he himself asserts that they are unfortunate and not guilty; and yet, immediately afterward, we find him insisting on the importance of "exciting *fear* in the patients, and from the very first." We read the title of this section with incredulity. We thought there might be some advantage in allaying fear, and from the very first. But it is not so. These poor creatures, still God's image, unfortunate and not guilty, are to be improved in their understandings by frightening them out of their wits.

With astonishment and with pain do we read, that, although "love begets love," yet love has its bounds; that kindness is unwholesome for the lunatic; that Jean Paul says, "the rod helps God;" and that Lichtenberg says, "with mad people blows do often more good than all other measures;" aye, and with this epigrammatic reason too, that "through them the soul is forced to knit itself once more to that world from which the cudgels come." Truly, if we admit that the patients are still God's image, we cannot extend that similitude to these physicians. The *antagonizing* principle seems to animate them, and their doctrines are only fit for the inquisition in this world, and for the place of torment in the next. If anything could deepen our disgust on reading such advice to superintendents, it would be that the "English" are praised for carrying it into effect. Too justly, we fear, has Esquirol observed, that the Germans and the English have exhibited the greatest zeal in devising ingenious and cruel bodily restraints.

It is astonishing how much the feelings, when thus perverted, interfere with directness of vision. M. Lehmann seems to have heard how remarkably insane persons are sometimes tranquillized on their introduction to to a lunatic asylum. M. Esquirol observes, that some never exhibit any madness afterwards. The frequent temporary tranquillity induced by the change is dwelt upon in the recent reports of Hanwell. In these instances it has been supposed that kindness, an appearance of consideration and regard, a cheerful building, refreshing gardens, and various other circumstances, may contribute to the end observed. But M. Lehmann, with the fatal obliquity of vision to which we have alluded, attributes all to change, and change alone; and seems to think it is of very little consequence what the change is, provided it is for the worse. He again refers with much facility, it appears, to authorities; quotes Horn with satisfaction, as saying "that a lunatic asylum must be so arranged that a sane person would go mad there," and even speaks with some approbation of Reil's fanciful saying, that "the reception of a lunatic in his asylum should be amid the thunder of cannon; that he should be introduced by night, over a drawbridge, be laid hold of by Moors, thrust into a subterranean dungeon, and put into a bath with eels and other beasts." Upon this M. Lehmann observes, "If this does not bring our patient to attend to what is going on about him, both by eye and ear, I know not what will." We confess that we quote these expressions with a degree of horror. Such doctrines and practice, springing

from cold hearts and wild imaginations, at once declare the total unfitness of such men to be intrusted with the care of lunatics. Take this single and simple test of them: If a man has horses that he values, or dogs that he regards, would he intrust them to the care of these *cudgel-players*? Certainly not. He would say, "these are cruel fellows; they will ill-treat my horses and dogs, and ruin their tempers." Can it then be too much lamented, that men disqualified by the brutality of their minds from being the keepers of brutes, should be the guardians of human beings bereft of their protecting reason?

On a former occasion we alluded to M. Leuret's plan for subduing all mental delusions by the *douche*. Did a man proclaim himself an emperor, straightway he was put under the douche-pipe. After a few minutes an opportunity was given to him, by the cessation of the stream, to renounce his imperial claim. If, breathless but unconquered, he was still an emperor, down came the column again and again until he fairly abdicated. It is against this notable discovery that Dr. Blanche directs his pamphlet. The insertion of M. Leuret's Memoir in the volumes of the Royal Academy of Medicine has given it, Dr. Blanche thinks, an importance which entitles it to notice. Twenty years' experience has convinced Dr. Blanche, that, in comparison with the results of a mild and patient treatment of insanity, all violent means are vain. He acknowledges that even Pinel and Esquirol have, in certain circumstances, advocated severity; and he is willing to give to M. Leuret all the advantage of his argument, that if the means be successful we should practise them with firmness; just as the surgeon performs a severe operation to save life. But here lies the whole question. The point to be determined is, whether M. Leuret, ambitious of fame, has published an academical dissertation on a new and cruel method of acting upon an old and cruel principle, or has really found out a method of cure for the most obstinate of maladies. Dr. Blanche quotes from the works of Pinel and Esquirol passages sufficiently expressive of their abhorrence of all cruel methods of control; and he appeals to the practice of M. Pariset of the Salpêtrière, and of M. Desportes in the same institution, and of M. Ferrus at the Bicêtre, as supporting the same views. He then examines the *four* cases on which M. Leuret bases his new doctrine of moral treatment. We agree with Dr. Blanche, that the cases prove rather the effects of a strong impression made on the mind by contradiction, mockery, and contempt, alternated by elaborate kindness, than by the douche. The treatment is certainly not the more humane on that account.

Any document relating to insanity, bearing the signatures of Esquirol and Pariset, is calculated to arrest attention. At the end of Dr. Blanche's work, we find a formal report upon it with these names, the production of M. Pariset, who makes a very ingenious defence for M. Leuret, acquitting him of Dr. Blanche's two principal charges: the affectation of novelty in a method of intimidation, and the general application of the practice as a kind of doctrine. This disposed of, M. Pariset agrees wholly with Dr. Blanche, and shows, with his usual force, the limited range of the means of intimidation in the variety of cases brought under the physician's notice in a lunatic asylum. In an eloquent page of conclusion,

M. Pariset shows that there is "a precept and a maxim" which are to be kept in view in the treatment of insanity. The precept is, to favour the renovation of the organization, by attending to the excretions and to nourishment. The maxim is, to establish with the patients the only authority worthy to be gained; "the only authority to which they yield;" an authority "founded on confidence and respect," and "only to be obtained by justice and goodness." Of the justice of those who have the care of them, he observes that insane people never lose a discerning sense; and goodness, he adds, is itself only justice, and should be conspicuous in every word and action of which the lunatic is the object. Thus alone, he says, will the hearts of the insane be touched; their reason, thus alone, will become docile, and their will controlled. Rigorous conduct, on the other hand, in the physician will degenerate into barbarity in those he employs, and excite in the patients feelings of disgust, of aversion, and a desire to be revenged. These wise and benevolent observations are worthy of M. Pariset, and of him whom he affectionately styles his "master," M. Esquirol.

The Second Report of the Resident Physician at Hanwell, accompanying the Fifty-fifth Report of the Visiting Magistrates of that asylum, has just reached us. It embraces many particulars, and should be read entire. The First Part of it contains numerous tables, showing every circumstance interesting to the statistical enquirer respecting the patients admitted, discharged, cured, or otherwise, and dying in the asylum; and to some of these are annexed several remarks, particularly with reference to those discharged, and to the fatal cases. Part Second of the Report is devoted to an exposition of the general system of management pursued at Hanwell; and commences with the gratifying statement that the use of the strait-waistcoat, the muff, and every kind of strap and chain, and restraint-chair, was discontinued on the 21st of September, 1839; and has not since been resumed. Dr. Conolly briefly traces the progress of improvement in the treatment of lunatics; and shows that in this attempt he has only given a little further extension to the principles avowed in every page of Pinel and Esquirol, both of whom continually regret that every measure of violence becomes a bar to gaining the confidence of the patient. This confidence, Dr. C. maintains, is the key-stone of all moral treatment; an opinion in which he is certainly supported by every respectable authority; and he makes many observations calculated to show that the imposition of restraint always militated strongly against the acquisition of the patient's confidence; and, although regarded as a punishment and a mortification, failed in any degree to promote amendment. The details given concerning the gradual amelioration of conduct in some very old cases, which were considered so violent as to be hopeless, since restraint was wholly discontinued, are extremely interesting; and the reasons alleged for not resorting to restraints in the recent cases are to us quite convincing.

But Dr. Conolly expresses great anxiety that those who determine to try to dispense with bodily restraints, should understand that to take off waistcoats and muffs, and to unfasten leg-locks and hand-straps, is only the commencement of a system which, to be complete, must secure the

t and constant co-operation of the whole household, and compre-
areful medical treatment, and a never-wearied patience and phi-
py. A great portion of the second part of the Report is devoted
ain what are termed the substitutes for restraint; and which, as
be expected, do not consist so much of methods for which any
f novelty is set up, as of the more systematic application of me-
lready stamped with the approbation of all medical men who have
perienced in the treatment of insanity. Among these, seclusion
patients is largely dwelt upon, as one of the most important, al-
like restraint itself very liable to be abused.

observed that seclusion is compatible with all the remedial means
in each case; and that whatever objections apply to it, apply
ouble force to restraint, which either left the patient able to hurt
he himself being defenceless, or consigned him to a seclusion em-
d by straps and chains. There is, indeed, a wonderful incon-
y in the arguments of those who object to the non-restraint
. They object to seclusion, because restraint is not conjoined
. They declare that restraint was never abused, was seldom in-
ractised anywhere to a great extent; yet they are furious at its
inuance. They at the same time profess to defend restraints, and
nd a charge against Hanwell that restraint is secretly practised
a charge so utterly ridiculous as well as false, as applied to an
seldom free from visitors for one day in the year, that we do not
that the resident physician should have resolved to take no part in
roversy for which few persons can be prepared by experience;
ve contented himself with referring to the general state of the
itself as the fullest answer to all misrepresentations, and one to
no exception can be taken.

advantageous effects of the shower-bath are spoken of as esta-
by the experience of them in numerous cases. The douche,
t being of greater, or of so much benefit, is thought to be objec-
le as more distressing to the patient. Several remarks are made
erent medicines recommended in cases of mania; the plan of em-
nt, exercise, and amusement followed at Hanwell is explained;
veral directions are given for the selection and duties of the male
male attendants. The great points enforced are the exercise of
nt patience and kindness towards the patients; the abandonment
orce or threats, or authoritative language towards them, except in
and rare instances; and the union of the whole household, offi-
ttendants, and servants, in the great design of regulating every
f their conduct and manner with a view to the comfort and cure
lunatic inmates of the establishment. Some of these remarks,
e portion of the Report which treats of the difficulties to be en-
red in an asylum in which the attempt is made to abolish restraints,
too clearly at the difficulties encountered at Hanwell from officers
ttendants accustomed to the daily spectacle of cruel methods of
n. Feeble and uncertain support appears to have been given to
ysician by some of the subordinate officers; and the evils arising
heir negligence have occasionally, it seems, been coined into
s against the system. These unpleasant occurrences are very

slightly touched upon in the Report; but the newspapers have cont ffering allusions to them in reporting a recent discussion of the m trates at Clerkenwell. By the same means we learn that eve chaplain is a friend to fetters and manacles; and consequently gi aid to the physician. In fact, we believe that the physician has rec consistent support from one officer alone—the matron, who seems a woman of rare endowments, daily exemplifying by her conduct the most courageous self-devotion in the cell of the poor lunatic, in circumstances of difficulty and danger, as compatible with all the unf gentleness required by her peculiar office. Supported by such an effi assistant, and steadily countenanced by the visiting magistrates, have shown a great degree of moral courage throughout this experi the physician has been enabled to apply the non-restraint syste [illegible] patients, without the occurrence of a single accident that rest would have prevented; and, as a kind of answer to many general vague objections to his plan, which many may look for who do not to bewilder themselves with controversy, we think we may point t following statements of the cures and of the mortality under the systems, as at least showing that the non-restraint system is not favourable to the well-doing of the patients than were the strait-waist the leather-muff, the leg-locks, the hand-straps, the restraint-chairs, the bed-chains.

PER CENTAGE OF DEATHS AND CURES.

Year.	Average Number of Patients.	Number of Cures.	Number of Deaths.	Per centage of Cures.	Per centag of Deaths.
1831 from May 16	295	20	29	6·77 or 10·2 per ann.	9·83 or 14 per a
1832	427	64	99	14·75	23·18
1833	627	69	77	10·98	14·33
1834	564	48	58	8·51	10·28
1835	580	28	71	4·82	12·24
1836	611	37	65	6·05	10·63
1837	608	27	48	4·44	7·89
1838	662	33	89	4·98	13·44
1839	804	88	78	11·19	9·7
1840 to Sept. 30	846	45	50	5·31 or 6·64 per ann.	5·91 or 7 per

ART. VI.

ements of Chemistry, including the application of the Science in Arts. By THOMAS GRAHAM, F.R.S. L. & E., Professor of Chetry in the London University, &c. &c. Parts I.—V.; compreding *Inorganic Chemistry.—London*, 1837-40. 8vo.

emistry of Organic Bodies: Vegetables. By THOMAS THOMSON, ., F.R.S. L. & E., Professor of Chemistry in the University of sgow, &c. &c.—*London*, 1838. 8vo, pp. 1076.

Introduction to the Study of Chemical Philosophy; being a paratory View of the Forces which concur to the production of mical Phenomena. By J. FRED. DANIELL, F.R.S., Professor of mistry in King's College, London, &c. &c.—*London*, 1839. 8vo, 565.

pplement to the Introduction to the Atomic Theory; compreding a Sketch of certain Opinions and Discoveries bearing upon general principles of Chemical Philosophy which have been ught into notice since the publication of that Work. Prefaced by e Remarks on the projected Reforms in Academical Education. CHARLES DAUBENY, M.D., F.R.S., &c. &c.; Professor of Chetry and of Botany in the University of Oxford.—*London*, 1840. , pp. 62.

have for some time been intending to offer our readers a brief il account of the recent progress and present state of chemical phiy, chiefly for the benefit of those who, like ourselves, feel it imle to devote more time to this engrossing subject than is necessary pace with its generalizations; extending, as these rapidly are, in ehensiveness, and increasing in certainty. Dr. Daubeny's pamphlet rly corresponds in character with the review we had intended to the first three works on our list, that we might almost have satisrselves with directing the attention of our readers to its contents. vident, however, that the learned author destined it chiefly for a circulation; and we are sure that we shall not displease him, we confer a benefit on those who would like to receive his concise a state of still greater compression, in transferring to our pages s extracts from his very luminous summary.

the present article it will be our main object to set before our s, in the narrowest possible compass, the chief advances in the oles of chemistry, to which the labours of recent years have given on. We shall suppose them to possess a general acquaintance e laws of definite proportions and the atomic theory, as propounded lton; since these are embodied in every systematic work of modern n this science, and have been made the foundation of all public ction upon it for some years past. When Dalton had succeeded in ing the assent of the scientific world to his generalizations, it was ed, and not without reason, that the science of chemistry had d a degree of precision, which might place it on a level with those y regarded as the exact sciences. It appeared possible to embrace the compass of a few very simple propositions all the laws which

regulate chemical combination; and when, as in the case of organic substances, the phenomena were such as could not be directly deduced from the principles of this theory, it was then thought reasonable to refer such anomalies to the operation of the living principle, that mysterious agent, which, by imparting new properties and new affinities to the inert mass, removed it, as it were, beyond the domain which came under the exclusive jurisdiction of chemical laws. We shall express, in Dr Daubeny's language, a small number of propositions, which at that time appear to have formed the creed, expressed or understood, of chemists in general, and on which our subsequent remarks will be based.

"1. Matter is composed of a number of particles incapable of further division (or of atoms), bound together by the force of cohesive attraction; and compound bodies are produced by the union of ultimate particles of different kinds of matter possessing an affinity one for the other.

"2. There exists, therefore, a variety of elementary bodies, each of which is distinguished by the possession of certain fixed and unalienable properties chemical as well as physical; so that the business of the analytical chemist is limited to the determination of the nature of the several elements which enter into the composition of the substance he examines, and of the definite proportion they bear to each other.

"3. The difference between one compound body and another can be referred only to two causes, namely, either to their being formed of different elements, or of the same elements in different proportions; so that all bodies possessing the same chemical composition must be regarded as identical both as to *form* and *nature*—in physical as well as in chemical properties; whilst those which disagree, either in the nature or in the proportion of their component parts, must be expected to differ likewise one from the other in both the above particulars.

"4. Decomposition is caused by the approach of a foreign body, possessing a stronger affinity for one of the ingredients of a compound than that which binds the latter mutually together; so that, in every such case, the decomposing agent may be expected to enter into union with one or other of the elements of the combination which it destroys.

"5. The water, which is found attached to many bodies, is essential to their crystallization, but does not materially affect their intrinsic properties; a salt perfectly divested of water being in no respect to be regarded as chemically different from one which contains it.

"6. Organic bodies have their particles held together partly by what is called the *living principle*—a principle distinct from chemical affinity, and superadded to it; so that the laws which operate upon inorganic matter are, in many instances, suspended by its influence. Hence it is idle to expect that any of the various chemical compounds which result from processes taking place in the living body can be produced by artificial means; they being themselves resolved into their component elements, or into simpler forms of combination, so soon as the sustaining force, the principle of life, is abstracted." (pp. 2, 3.)

Most of the above propositions may be regarded as taken for granted, i[f] not embodied in the atomic theory, as propounded by Dalton. This theory was erected, as our readers will recollect, on the law of definite proportions, which must be considered as his real discovery, and which was pronounced by Sir John Herschel to be, after the laws of mechanics, the most important which the study of nature had disclosed at that period standing foremost, as it does, among the discoveries of the present age for the universality of its applications, and the importance of its practical results. It will be found, we believe, that *the essential part* of Dalton['s] propositions is applicable to all the new discoveries announced in ch[emistry]

mistry since its promulgation; so that each succeeding year has added something to the completeness of the evidence upon which it is based, as well as to the extent of the field which its jurisdiction embraces. This distinction it may claim equally with the Newtonian system; to which, in the scale of physical truths, it may not improperly be compared. It cannot be denied, however, that since the period of the reception of the Daltonian *theory*, erected upon those propositions, many new views, involving some fundamental points of theory, have obtained currency amongst chemists; which, though perhaps by no means irreconcilable with that hypothesis, cannot be said to be comprehended under its principles, and which run counter to many of the propositions which have been just quoted as having received a tacit, if not a distinct recognition as appertaining to it. We shall now proceed to notice the more important among these novelties; adverting, in the first instance, to those which simply extend the application of the law of definite proportions, and afterwards discussing those of which the relation to it is less evident.

The two great classes of combinations known as *acids* and *alkalies*, or alkaline *bases*, were long considered the only ones by which *neutral salts* could be formed; all these alkaline bases (with the exception of ammonia), as well as most of the acids, being formed by the union of oxygen with one of the elementary bodies. Thus, the salt known as carbonate of potass is composed of carbonic acid—a compound of carbon and oxygen, and of potass—a compound of potassium and oxygen. The researches of Berzelius and his followers have made it evident that *sulphur* is to be regarded in the same light, and that the *sulphurets* of the different elementary substances have relations, as *acid* and *base*, exactly parallel to those of the oxides. Thus, the bisulphuret of carbon, or the sesquisulphuret of arsenic, forms a *salt* with sulphuret of potassium; which, by the substitution of oxygen for sulphur in *each* ingredient, would form carbonate or arsenite of potass. Similar compounds are formed by the union of the chlorides, iodides, &c. with each other. These have been known as *double* chlorides or iodides; but it is now certain that they are to be regarded as *salts*—one of the ingredients acting as the acid or electro-negative substance, the other as the electro-positive substance or base.

Moreover, it has been shown that two oxides, iodides, or chlorides of the same metal may combine with each other in the relation of acid and base, in accordance with the relative proportions of oxygen, &c. which they contain. Thus, chromic acid, formed of 1 chromium and 3 oxygen, will unite with and be saturated by the sesquioxide of chromium, formed of 1 chromium and $1\frac{1}{2}$ oxygen. Further, two *salts* may unite together in a similar relation, one acting as the electro-positive and the other as the electro-negative ingredient. Thus, sulphate of potass will act as a base to sulphate of alumina (in which the proportion of oxygen is greater), and will form with it a double salt, familiarly known as alum. It is evident that, by the discovery of these facts, the application of the law of definite proportions has received an immense extension.

But this is not all. The study of the compounds of *cyanogen* has led to the discovery that this, though itself a compound body (formed by the union of 2 carbon with 1 nitrogen), acts like a simple or elementary body in all its relations, forming *cyanides*, which are parallel in character

to oxides, chlorides, and iodides, and which may stand to each other in the relation of acid and base. Cyanogen, then, is to be regarded as a *compound radical*; that is, as a simple body in its mode of combination, though compounded in its composition. To use the language of the atomic theory, 2 atoms of carbon and 1 of nitrogen together form a single atom or combining equivalent. Now this discovery has opened the way to a series of the most unexpected results, and may be regarded as having afforded the key by which the dark chamber of organic chemistry has been entered and explored. It will be presently seen that such compound radicals are so universally present in organic combinations, that the appellation lately given to this department of the science—the chemistry of compound radicals—would be extremely appropriate, if such combinations were not also to be met with elsewhere. But, as will be presently seen, it is not improbable that the same system prevails in the inorganic world to a much greater extent than has been generally supposed; and that, in fact, our ideas, simple as they appeared, of the union of acids and bases will have to undergo an entire revolution.

In what has been hitherto said, the old phraseology has been employed, because our object has been to indicate the analogy between the compounds of sulphur and cyanogen and those of oxygen. If the long-established view of their composition is changed in regard to one, it will have to be for all alike. We shall now pass on to some of the cases in which it seems pretty well ascertained that compound radicals, of much more complex composition than cyanogen, play the same simple part. The cyanuret of iron will not only combine with the cyanurets of potassium, &c. but also with that of hydrogen—hydrocyanic acid. Now, of such a compound, two views may be taken. It may either be regarded as a salt, like the former, or it may be considered that the whole of the cyanogen in the compound unites with the iron, forming a new radical, which unites with the hydrogen as a simple substance. On the one view, the composition will be thus represented (we avoid the use of symbols for the benefit of our older readers): 2 (hydrogen + cyanogen) serving as the base to (iron + cyanogen). On the other it would be (2 hydrogen) united with (iron + 3 cyanogen). The latter is the opinion of most chemists at the present time; and the existence of a compound radical, ferro-cyanogen, serving as an elementary body, although actually consisting of three, is generally recognized. There is little doubt that cyanogen and sulphur, cyanogen and carbon, and other similar compounds, act as radicals or elementary substances in their combinations. It becomes difficult, then, to assign the limit to this kind of action; and the complex constitution of a body need no longer be regarded as essential by altering its chemical relations.

It will be observed that cyanogen may be regarded as the connecting link between organic and inorganic substances; for, though so simple in its composition, it cannot be produced by the artificial union of its elements, and can only be obtained from substances which have been elaborated by the living system. We must, therefore, consider cyanogen as an organic compound, although so simple in its relations. The facility with which these may be investigated is a great aid to the organic chemist; for, whilst most organic compounds are disposed to separate

into their *ultimate* constituents, when submitted to any analytic process, those of cyanogen are much more easily resolved into their *proximate* constituents, by the reunion of which the compound may be readily reproduced. Now it is becoming more and more apparent, that it is only required by the organic chemist that he should *know how* to analyze the substances of which he wishes to ascertain the composition, so as to prevent them from separating at once into their ultimate constituents—to make the *theory* of organic chemistry as simple as that of inorganic; and, with certain data (as, for instance, cyanogen, the hydrocarburets, and other very simple compounds, which cannot be obtained except from organic substances), the *practice* also. Not that the chemist can ever expect to manufacture *organized* tissues, possessed of vital endowments: these are produced by the operation of a living system upon *organizable products.* But we do not see any reason why these last should not be brought within his reach, and be as readily produced in his laboratory as they are in that of the plant or animal. We shall now briefly indicate what has been already done in this interesting line of investigation.

A compound radical has lately been discovered by Liebig, having the same elements as cyanogen, but in different proportions. To this the name *mellon* has been given, and it is composed of six carbon and four nitrogen. In the large number of proportionals which go to form one combining equivalent of this body, it bears a strong resemblance to organic compounds, of which this is usually a characteristic. Mellon acts as a compound radical in precisely the same manner as cyanogen; and its compounds are of parallel character.

A most important series of compound radicals are the hydrocarburets, or compounds of hydrogen and carbon. These serve as the bases in a great variety of organic compounds, which may be artificially made from them; but the chemist is still unable to form these radicals by the union of their elements, and, in his ulterior operations, is obliged to start from these as postulates or data, obtaining them from various sources, in all of which vitality has at some period had concern. Thus, a large number of different hydrocarburets may be obtained by the destructive distillation of coal, which, there is now no doubt, had its origin in the decomposition of resinous wood. It is a curious fact, which may be observed in passing, that in several of these hydrocarburets the relative proportions of the elements are the same, whilst the form of the body is different, its combining equivalent also varying. Thus, in *olefiant gas,* two proportionals of carbon and two of hydrogen are condensed into one volume. In *etherine,* a liquid obtained by Faraday from oil-gas that had been condensed to render it portable, four volumes of each ingredient are condensed into one. An oily liquid, described by Dumas under the name of *cetene,* contains sixteen volumes of each. And he also describes a compound (not yet obtained in a separate form), to which he gives the name of *methylene,* in which he calculates that one volume only of each occupies the same compass.

For our present purpose, however, it will be better to confine ourselves to those hydro-carburets which have been ascertained to serve as compound radicals in well-known organic combinations, that were formerly regarded as resolvable only into their ultimate constituents. The volatile oil of turpentine, in a state of great purity, consists of eight propor-

tionals of hydrogen and ten of carbon, together forming a compound radical, to which the name *camphene* has been given. This unites with oxygen in two proportions, forming *camphor* with one, and camphoric acid with two. The volatile oil of lemons consists essentially, and almost solely, of a corresponding substance, termed *citrene*, which contains the same ingredients, in the same proportion, but in only half the absolute quantity for each combining equivalent. From the researches of Dumas it appears that most of the volatile oils containing oxygen, as those of anise, peppermint, &c., owe this ingredient to the camphor they contain, which is dissolved in variable proportion in the liquid carburets of hydrogen that essentially form them. Here is another instance in which *ultimate* analysis would have completely failed to indicate the mode in which the elements are combined.

A still more interesting compound radical was discovered, a few years since, by Liebig and Wöhler; a most unexpected relation being disclosed by the knowledge of it, between that highly poisonous substance—the oil of bitter almonds, and the inert body benzoic acid. The base of both these, termed *benzule*, consists of fourteen carbon, five hydrogen, and two oxygen. This, in union with one additional equivalent of hydrogen, forms the oil of bitter almonds; whilst, with one of oxygen, it forms benzoic acid. Hence, by merely substituting an atom of oxygen for one of hydrogen, the former compound is converted into the latter; and, accordingly, the essential oil, if exposed for some time to air, spontaneously undergoes this change, and is metamorphosed, from one of the most virulent of poisons, into a mild innocuous acid, an ingredient in many animal as well as vegetable excretions. Lowig has traced a similar class of compounds, derived from the oil of the spiræa ulmaria.

The knowledge of these facts is very important, not only in regard to the general principles of the science, but also in reference to the opinion we may form of the relations between alcohol, ether, and olefiant gas. In the time of Lavoisier, when the task of analysis was limited to the determination of the ultimate elements of a body, and of the proportions they bore one another, nothing more was attempted than to set down the amount per cent. of oxygen, hydrogen, and carbon, present in the several bodies of this class which were then known. But it has since been discovered, that a very simple and beautiful relation subsists between alcohol and olefiant gas; and that its composition may be represented by one atom of the gas and one of water. Now sulphuric ether, which is derived from alcohol by distilling it with sulphuric acid, has exactly the same constituents, but in different proportion, the amount of water being only half that contained in alcohol. Hence Dumas regarded olefiant gas as serving as the base to both these bodies, standing in the same relation to water as alkali does to an acid, and consequently readily entering into combination with it. Thus, one atom of water with two atoms of olefiant gas would constitute ether; whilst two atoms of each form alcohol. It is contended by Liebig, however, that it is more conformable to analogy to regard ether as the oxide of an unknown base, containing one atom more of hydrogen than exists in olefiant gas; and alcohol as a compound of this oxide with one atom of water. It is no argument against such a view, that this base has not been obtained in a separate form; for the same is the case with *benzule*, yet it may be trans-

ferred, like cyanogen, from one compound to another. It has been, we think, successfully shown by Liebig, that no water *as such* exists in ether; but that this substance is to be regarded as an oxide, being neutralized by acids, and forming with them a class of compounds analogous to the salts. It would appear, from a recent memoir, that Dumas has in a great degree adopted the doctrines of his opponent, to the abandonment of his own.

We have judged it advisable to abandon for a time the order of the propositions which we adopted from Dr. Daubeny's pamphlet; in order that we might bring before our readers, somewhat more fully than he has done, a view of the direction in which our knowledge of organic chemistry is now being improved and extended. It will be convenient for us now to accompany him in his remarks upon the sixth proposition, which affirms that "organic bodies have their particles held together, partly by what is called the living principle, so that the laws which operate upon inorganic matter are in many instances suspended by its influence." "This," observes Dr. Daubeny, "seems, to say the least, questionable, and the corollary founded upon it;" as to the impossibility of producing by artificial means, any of those compounds which result from the principles of life, is contradicted by fact.

"We know already of at least two, if not of more, distinct substances, proceeding ordinarily from living processes, which can be formed at pleasure by art. Such, for instance, is formic acid, which is excreted by ants, but may also be procured very readily by chemical means. Such, too, is urea; which Wöhler has succeeded in producing by the action of ammonia upon cyanogen. The crystals formed are identical with those of urea; whilst the composition is the same as that of the salt termed cyanate of ammonia, the properties of which are very different. These two, therefore, are isomeric bodies, and one of them, though it derives its existence from the ordinary processes of life, is capable of being produced without the intervention of the living principle at all.

"There is little doubt that the progress of research will bring to our knowledge many similar cases; and that it will eventually appear that all the other secretions or excretions of animals and vegetables are only so far dependent upon life, inasmuch as, in consequence of the favorable temperature which it sustains, the constant circulation of the fluids it occasions, and their exposure to external agents in vessels of different shapes and dimensions, a mechanical separation of the ingredients of the blood is effected in some instances, and a chemical change produced in its constitution by catalytic action in others." (p. 58.)

If the vital principle, Dr. Daubeny afterwards justly remarks, operates directly upon the elements of the living structure, imparting to them new affinities or suspending old ones, there would seem to be no good reason why the decomposition of the organic structure should not necessarily and immediately ensue upon the abstraction of the principle of vitality; whereas it is well known that these changes only take place when favoured by a suitable temperature, and promoted by the agency of external causes.

"The laws which regulate the decomposition of organic matters bear a sufficiently close resemblance to those that prevail amongst mineral bodies. The more complicated their structure may be, the more readily are they found to be resolvable into their component parts; for the greater the number of proximate principles which an organic substance may contain, the more the chances are multiplied, that something will interfere to disturb the balance of the sustaining affinities. The putrefaction, then, of vegetable and animal matters appears to

be produced, not by any sudden cessation of those affinities which had previously bound their respective elements together, but by the predominance over them of other natural forces, which we may without much difficulty conceive to have been controlled under the circumstances in which the living body is placed; nor does there seem any sufficient reason for calling in the intervention of an occult principle to explain that, to the solution of which, by known causes, every fresh advance in chemical knowledge seems to bring us into closer approximation." (pp. 58-9.)

The following concluding remarks strike us as peculiarly apposite:

"Some, indeed, like the late Mr. Abernethy, seem to flatter themselves that they are making head against materialism, by assigning to the direct agency of a vital fluid the processes of the animal economy. But I have always felt that those who, in imitation of him, would assign to that immortal principle of our nature, which manifests itself in the operations of thought and intellect, any concerns in the functions of the perishable body, more direct and immediate than that which it may exert through the medium of the nervous system; so far from establishing on a surer foundation the doctrine of the soul's immortality, are, in fact, degrading that *divinæ particula auræ* to a level with electricity, chemical affinity, and other influences, which affect equally inanimate as well as animate matter...When we suppose that any real explanation is afforded of the phenomena, by ascribing them to the operation of the vital principle, or to any vital affinities, which is merely a less simple mode of expressing the fact, we are indulging in one of those delusive attempts to substitute words for ideas, which have so much tended to retard the progress of physiological science." (pp. 35-6.)

With these remarks we need scarcely say that we entirely concur; and we rejoice to think that such sound views are making their way amongst intelligent men. Dr. Daubeny's clear and forcible expression of them may do something, we hope, to open the eyes of those who think it necessary for the interests of religion to excite the *odium theologicum* against any one who impugns the doctrines, of which nothing but the blindness of prejudice can prevent their distinguishing the fallacy.*

We shall now follow Dr. Daubeny through his remarks (sometimes abridging and sometimes expanding them) upon the first five of the propositions above stated; having already disposed of the sixth. The *first* question we shall notice is that of the infinite divisibility of matter, which remains (Dr. Daubeny conceives) much on the same footing as that upon which it has rested for several years past. Passing by a mathematical argument, founded on the known limits to the atmosphere surrounding the sun and planets, we shall notice a statement quoted from Mr. Whewell's Bridgewater Treatise, which the researches of Ehrenberg show to require great modification. "The smallest microscropical objects which can be supposed to be organic are points or gelatinous globules, or threads, in which no distinct organs, interior or exterior, can be discovered. These, it is clear, cannot be considered as indicating an indefinite progression of animal life in a descending scale of minuteness. We can, mathematically speaking, conceive one of these animals as perfect and complicated in its structure as an elephant or an eagle, but we do not find it so in nature. It appears, on the contrary, in these objects as if we were, at a certain point of magnitude, reaching the boundaries of the animal world. We need not here consider the

* See Edinb. Med. and Surg. Journal, Jan. 1840.

hypotheses or opinions to which these ambiguous objects have given rise; but, without any theory, they tend to show that the subordination of organic life is finite on the side of the little as well as of the great." Now, we are not going to enter upon a discussion on the validity of Ehrenberg's statements respecting the complexity of organization in polygastric animalcules, but we think that it will be interesting to our readers if we present them with some of his recent estimates of the degree of minuteness of the particles of which the microscope can take cognizance.

It may be observed, *in limine*, that every improvement in the powers of the microscope has brought into view whole groups of living beings, whose existence was not even suspected, and has laid open a complex structure in others which were previously considered to be of the simplest possible character. We have no doubt that the hints thrown out by Ehrenberg, in his Essay on Organic Molecules and Atoms,* written about five years since, have been subsequently realized. Of this paper we shall now give a brief summary:

"I could plainly distinguish (says Ehrenberg), with a microscope magnifying 800 times, *monads* which had been filled with colouring nutritive substances, and which possessed voluntary motions, but the entire and greatest diameter of whose body only amounted to $\frac{1}{1500}$ or $\frac{1}{2000}$ of a Parisian line. To this smallest animal form yet discovered I have given the name of *monastermo*. I could perceive in the largest individuals as many as six, and in the smallest as many as four internal sacs, coloured blue by indigo, which at times did not occupy quite half of the dimensions of the animal. Such a sac, therefore, of the *monastermo* must be at the most $\frac{1}{12000}$ of a line in diameter. At the upper part of this animal is seen, as in all the monads, a powerful movement of particles still smaller than themselves when these have approached them; and it is therefore probable that it has a fringe of from ten to twenty cilia at the mouth-aperture, like that which can be distinguished in the larger monads. Further, even if we do not suppose the single colouring particles with which the stomachs are gradually filled to be very numerous, we must admit, from the roundness of the sacs, that there must be at least three particles in each. This affords us proof of the existence of material colouring particles of red and blue, moving freely in the water, which measure $\frac{1}{36000}$ part of a line, or $\frac{1}{432000}$ part of an inch in diameter. If we found our computation on the size of the smaller monads these particles would not exceed $\frac{1}{576000}$ of an inch in diameter. If the cilia really exist, they must probably have a less diameter than $\frac{1}{38100}$ of a line, or they would be themselves visible with the highest powers of the microscope. [Ehrenberg subsequently states it as the result of his experiments on the limits of microscopic vision, that squares of $\frac{1}{48000}$ of a line each way can be distinguished by a careful observer with a clear magnifying power of 1,000.] Further, in the larger infusoria of similar structure it is seen that, when the coloured globules contained in two digestive sacs appear to touch one another, they are separated by a membranous partition, bearing but a small proportion to the diameter of the sac: say, one-twentieth at most. This would amount to $\frac{1}{320000}$ of a line, or $\frac{1}{3840000}$ of an inch in monads $\frac{1}{30000}$ of a line in diameter. The proportion which the granules constituting the ova of larger animalcules bear to the size of the parents is generally from $\frac{1}{40}$ to $\frac{1}{80}$ of their diameter. We may then consider the diameter of young monads, just come from the egg, to be almost $\frac{1}{80000}$ of a line, and perhaps much less. These would have digestive sacs, whose diameter in the same proportion would be $\frac{1}{480000}$ of a line; and the thickness of their walls would not exceed $\frac{1}{9600000}$ of a line, or $\frac{1}{115200000}$ of an inch. It is by no means certain that the *monastermo* is the smallest species existing; for, under favorable circumstances,

* Taylor's Scientific Memoirs, vol. i. p. 568, et seq.

wandering shadows of much smaller monads have been observed, which son future improvements in the microscope may enable us to study with the san precision."

These calculations are made upon fair data, and are not to be re garded as conjectural.

"They plainly demonstrate," says Ehrenberg, "an unfathomablene of organic life in the direction of the smallest conceivable space; and the word *infinity* be too much for what we know at present, let the woi *unfathomableness*, which I have purposely employed, avert from me tl reproach of exaggeration, and establish the points of view under whic the physical, chemical, and physiological enquiries of our days, shoul they be rendered fruitful by new powers, will have to take, and what d viations they have to avoid." This, we think, is the correct mode viewing the subject. We do not see how the *infinite* divisibility of ma ter can be ever proved, for all man's operations are finite. But that n human means will ever meet with a limit, we are fully persuaded.* O this, as on many other subjects, we deem Mr. Whewell's conclusions e tremely hasty. We shall now return to the purely chemical view of tl subject, for which we shall have recourse to Dr. Daubeny:

"Although the abstract doctrine with respect to the existence of ultima atoms seems still to maintain that ascendancy over the opposite opinion, whi it acquired from the period that Dalton first established the laws of definite pr portions, it must be admitted at the same time that combinations amongst bodi may be more readily explained by imagining them to take place between certa definite groups of these atoms, than by assuming, as the father of the atom theory preferred to do, that they resulted from the union of simple atoms of ea ingredient. Thus much, at least, appears clear, namely, that when a body is r solved into the state of gas, so that its several parts exert a mutual repulsion, tl parts so repelling each other are not single atoms, but groups of them." (p. 14

This supposition is necessary to account for the fact that, in some i stances, when two gases unite to form a third, the total bulk is not dim nished, as it would be if the atoms, between which that repulsio operates which keeps them in the state of gas, were the same with tho which chemically unite; and which, therefore, are diminished in numb to one half. It also affords a satisfactory explanation of the want correspondence between the combining weights and combining volum of certain gases, as well as of the simple numerical relation always sul sisting between these. Dumas has hence proposed to designate that d scription of molecular groups, which constitute the extreme division which matter is susceptible by heat, by the term *physical atoms;* an those simpler groups, into which their affinities for other bodies ofte subdivide them, *chemical atoms*. From the experiments of MM. Dulon and Petit on the relation between the specific heat of various bodies an their atomic weights, it is inferred by M. Dumas, that the chemic atoms, or smallest combining proportionals, are not themselves th ultimate particles of the body, but contain these in a number varyin with each element.

* Muncke (Handbuch der Naturlehre, Heidelberg, 1829,) demonstrates that a grain indigo may be divided into thirty-eight *billions* of visible parts: and when we consid that a human being could not count a single billion, working night and day, in a short period than *thirty thousand years*, we may form a faint conception of the number particles in a grain of matter.

The following may be regarded, in Dr. Daubeny's opinion, as expressing the present state of knowledge on this subject:

"On the mathematical question which stands on the threshold of the subject, it cannot be expected that much new light should have been thrown. We are still, therefore, equally at liberty to embrace the theoretical doctrine of the capacity for infinite division necessarily inherent in matter, or the more metaphysical and refined hypothesis of Boscovich, who deduces the primary qualities of all natural bodies from the existence of a number of ultimate points, destitute of all properties save that of mutual attraction and repulsion, operating at certain distances, and obedient to certain laws.

"But the present stage of experimental knowledge, whilst it leaves us at liberty to speculate as before with respect to the inherent capacity of division, which may belong to the smallest conceivable portion of matter, as well as to the largest, affords at the same time grounds for the belief, that the Author of Nature has placed somewhere in the scale of minuteness a point beyond which no natural force can carry division. Now a body, of whatever dimensions we may assume it to be, which is held together by a force superior to any which can ever be brought to divide it, we denominate an *atom*.

"The relative weight of these atoms may probably be indicated by the specific heats assigned to the several substances.

"With these atoms, however, chemistry, strictly speaking, has no concern; but it is with groups or assemblages of them, held together by a certain cohesive force, which is proof against every other sort of attraction, that this science is conversant. These assemblages of atoms (the *chemical atoms* of M. Dumas), uniting with each other in various proportions, produce combinations according to the law of definite proportions, and are mutually displaced by the operation of chemical affinities.

"Lastly, by converting a body into gas or vapour, we separate it into other groups of particles (the *physical atoms* of M. Dumas), consisting of one or more of those between which chemical union takes place." (pp. 17-20.)

The first part of the *second* proposition—which implies that the chemical properties of each body, so long as it remains uncombined, are, like its physical ones, essential and constant—requires great modification in consequence of the important discoveries of Faraday and others, as to the relation between *electrical attraction* and *chemical affinity*. Whether or not we regard these powers as coincident, it is unquestionable that the latter is greatly influenced by the former, so that a body is often affected very differently by the same reagent, according to the electrical condition that has been induced in it. Some very curious facts bearing upon this subject have been recently brought to light by Professor Schœnbein of Basle. These, from their general tenor, might lead us at first sight to believe that the affinity of iron for nitric acid, instead of being inherent in the metal under all circumstances, was superadded to it by certain extraneous influences, its existence being dependent upon the relations of the metal at the time to electricity, or on some other causes equally obscure.

"1. If an iron rod be raised at one extremity only to a red heat, it will not be acted on by nitric acid of the sp. gr. of 1·35. 2. This immunity from the action of the acid may even be imparted to a second rod if brought into connexion with the first. Thus, if the heated wire be made to touch a second, and both be plunged into the same acid, neither one will be acted on. The same immunity is obtained if an iron wire, plunged into nitric acid, is simply made to touch a wire of platina. 3. The same wire, if made the positive electrode of the

galvanic battery, is not acted upon by the acid, though it transmits the galvan current, and consequently decomposes the water present; whilst, on the oth hand, it is vehemently attacked by the same acid when in connexion with t negative electrode. 4. The same immunity is conferred on the metal, by in mersing it for a few moments in acid, after which the action entirely cease and it may then be sometimes renewed by various mechanical methods, as rubbing it with a copper wire, with glass, or in other ways." (p.21-2.)

Modern discovery, then, compels us to recognize a distinction betwe the physical properties of matter and their chemical ones, inasmuch the former are always essential and inherent, the latter sometimes i duced by extraneous agencies. But there is nothing to contradict t belief, that the conditions, on which depend the capacity of being affect in this manner, are in themselves as permanent, and subject to laws fixed and definite, as those which seem more directly to belong to th constitution and nature. But, if the chemical properties of matter a affected by changes in their electrical condition, it is not unreasonable suppose that a different arrangement of the particles of the same speci of matter may alone be sufficient to bring about a change in its chemic constitution, by altering its relations to electricity. Hence we are pr pared, on theoretical grounds, for receiving the new doctrine which h so much changed the face of chemical science, and on which we ha already expatiated, namely, that many substances which are known be of a compound nature, are nevertheless in some sense to be regard in the light of elements.

The doctrine of *compound radicals* has lately been extended, as have already hinted, to inorganic chemistry. These views were, we b lieve, first propounded by Liebeg; and he is supported in them by Pr fessor Graham, who may justly rank, we think, as the highest Briti authority on philosophical chemistry. Formerly we were in the habit assuming that four or five combinations might take place between o elementary body and another; thus, that sulphur with one atom of ox gen formed hyposulphurous acid; with two, sulphurous acid; and wi three, sulphuric acid. But, according to these chemists, the hyposu phurous acid unites as a compound radical with an additional atom oxygen to form sulphurous acid; and sulphurous acid, in like mann acts as a compound radical, forming sulphuric acid with another atom oxygen. Again, when sulphuric acid unites with soda, it is believed th the acid takes the oxygen of the alkali to form a fourth compound, which the name *sulphatoxygen* has been given, which unites as a com pound radical (performing the part of an elementary substance) with t metallic sodium.

Nor is this the only change which it is proposed to introduce into t mode of considering these acids. According to the old notions, it w necessary to suppose that some of these acids derived their acidity fro oxygen, others from hydrogen, others from chlorine, iodine, &c. order to remove this anomaly, it was originally suggested by Davy, th what we call the oxygen acids are, in fact, combinations of hydrog with a compound base containing a certain number of atoms of oxyge Thus hydrous sulphuric acid is usually represented as sulphuric acid water. Whereas it is possible to consider it in the light of *sulphatoxyg* and hydrogen; hydrogen being the acidifying principle here, as in t

case of muriatic acid. This view has been fully developed by Professor Graham in his Elements (p. 160); and, more recently still, Liebig has given it the weight of his name and authority. It is supported by the fact. that the power of saturation which an acid possesses is dependent upon the amount of hydrogen present in it; and, moreover, that every known substance which exhibits acid properties contains hydrogen, either by itself or in the form of water. For, when destitute of this principle, as is the case with the anhydrous sulphuric, phosphoric acids, &c., it exerts no chemical reaction whatsoever. Still we *know* nothing of the existence of such a compound radical as *sulphatoxygen;* and the chemist who assumes it may be charged with having fallen into the same error with his predecessors, who imagined the hypothetical principle phlogiston.

It will be evident, however, to any one who takes the trouble to examine the subject for himself, that the doctrine of *compound radicals*—that is, of combinations of two or more elements, which, in their chemical relations with other bodies, correspond with simple substances—enables us to bring organic compounds under the same general laws as those of the mineral world; and that the extension of it to inorganic che mistry will, in the end, do much to simplify the principles of the science.

"We perceive, then," concludes Dr. Daubeny, in reference to the second proposition, "that it cannot be adopted without considerable modifications; for neither, on the one hand, do those substances which we still hold to be elementary manifest relations to others at all times the same in degree or intensity, nor, on the other, are those which constitute the basis of organic substances, and which stand on the same footing with respect to the rest as the elements of mineral bodies do to the salts and acids to which they give rise, necessarily themselves simple, or composed of one kind of matter. They resemble, indeed, rather the elements represented by Ovid and other writers of antiquity, the bases of other bodies, but yet capable themselves of conversion from one shape into another." (p. 35.)

The *third* proposition, which lays down that compound bodies, formed by the same elements in the same proportions, must have the same properties, and that a difference in the ingredients must produce a difference in the properties of the compound, must also undergo considerable modification, in consequence of the discoveries of recent times. It has been found, for example, that the same ingredients, united together in precisely the same proportions, will often give rise to several compounds as distinct in their nature as those whose component parts themselves vary. Such bodies are now distinguished by the term *isomeric;* denoting that they consist of equal parts or proportions of the same elements. In some instances, the distinction between them appears to arise from the different degrees of condensation which the ingredients have undergone; as in the case of the hydrocarburets already alluded to. In another class, the difference between two isomeric bodies may be traced to a difference in the processes by which they have originated. But there are many to which no such explanations apply. In all these instances there can be no doubt a different arrangement of the component particles has taken place in the two resulting bodies; and this accounts for the fact of their assuming different crystalline forms, and having different relations to light, electricity, and other physical agents, and also for their combining proportions not corresponding. But why the same ingredients

should affect one rather than the other arrangement is a question sti veiled in obscurity.

Nor is the assumption of different properties by bodies which, in thei constitution, appear to agree, the only point on which recent discoverie clash with the established principles of the chemistry of a former era The same substance will sometimes assume two or more crystalline forms without any alteration of its qualities supervening. Bodies which thu present themselves under two different primary forms are called *dimor phous ;* and the number of these already known is so great, that it ma perhaps be doubted whether there be a body in nature which constantl occurs in the same physical condition under all circumstances. If w regard *isomerism* as resulting from a different arrangement of the *che mical* atoms, we may perhaps attribute *dimorphism* to a variety in th arrangement of the *physical* atoms ; or, to use an analogy employed i a corresponding case by Lucretius, the first class of bodies may be com pared to different words formed by the transposition of the same letters whilst, in the second class, the arrangement of the syllables only is re versed, the letters in each syllable remaining the same. As to the caus of the variety of crystalline form assumed by the same body, we are stil much in the dark. The researches of Mitscherlich, however, have shed some little light on the question. He has shown that heat, in man cases, tends to modify the crystallization of a body, by causing it t expand unequally in different directions, enlarging the acute angles, an thus imparting to it a tendency towards the cube. Hence the same sub stance, if made to crystallize at a high temperature, may assume a diffe rent form from that which it would take at a lower one; and this i actually found to be the case with carbonate of lime, which, when depo sited from a solution in the cold, assumes the rhomboidal form of cal cerous spar, but when from water of a higher temperature become arragonite.

If it be true that a mere variation in the arrangement of the same par ticles is alone sufficient to produce an entirely different material, we ar naturally led to speculate as to the possibility that the substances which inasmuch as they have hitherto resisted our powers of decomposition, w regard as simple, may themselves be isomeric bodies, or be produced b a different arrangement of the same elementary matters. Thus have th beautiful discoveries of Mitscherlich and others brought us back to th very speculations which engrossed the alchemists of old ; so that it ha happened in this case, as in that of the atomic theory itself, that moder science has incidentally lent support to views which had been originall brought forward on grounds altogether different, and by a class of per sons whose habits and principles of reasoning were as opposite as pos sible from their own.

We are rather surprised that Dr. Daubeny has not referred to the pheno mena which may be regarded as the converse of these two, namely, *iso morphism.* This term is employed to express the fact, that, in many in stances, one element of a combination may be withdrawn and replace by another without any change in the external form of the crystal, or i its general physical properties. The bodies which may thus be substi tuted for one another form groups, which are said to be *isomorphous* One of the most remarkable of these contains the acids formed by th

union with arsenic and phosphorus with oxygen, which have altogether a most remarkable correspondence—arsenious acid having a strong resemblance in all its chemical relations to phosphorous, and arsenic to phosphoric. Another group contains the sulphuric, selenic, chromic, and manganic acids, which may be substituted for one another without the form of the crystal being changed. In another group are included the sesquioxides of aluminium, iron, chromium, and manganese. Several other groups might be enumerated, did space permit. It is, of course, necessary that the combining proportion of the substituted element should be the same as that of the one withdrawn. The variety which may be produced in the constitution of alum affords an interesting example of these properties. The alumina may be replaced by the sesquioxides of iron, chromium, or manganese: ammonia may be substituted for the potash, and selenic, chromic, or manganic acids for the sulphuric; so that the composition of this salt may be partly or entirely changed, without its external form being affected. Isomorphous substances have often very close points of resemblance, independent of external form. Thus the vapours of arsenic and phosphorus have nearly the same odour: many of their precipitates correspond in colour; they both form gaseous compounds with hydrogen; they differ from nearly all other bodies in their mode of combining with oxygen, but agree with one another; and their salts are disposed to combine with the same quantity of water of crystallization. A similar analogy exists between sulphur and selenium, baryta and strontia, lime and magnesia, cobalt and nickel.

It is evident that isomorphism affords additional weight to the speculations just now hazarded in reference to the compound nature of many substances now regarded as elementary. They can *only* be at present regarded, however, as speculations; but we think the day is not very far distant when their truth or falsity will be determined.

The *fourth* proposition—in which it was stated that "decomposition is caused by the approach of a foreign substance, possessing a stronger affinity for one of the ingredients of a compound than that which binds them mutually together, so that in every such case the decomposing body may be expected to enter into union with one or other of the elements of the compound it destroys"—cannot, in the present state of our knowledge, be received without considerable limitation, although it might formerly be regarded as an obvious consequence of the recognized principles of chemical combination. It has been found that there are substances capable of causing decompositions, and of bringing about new unions between certain elementary and compound bodies, merely by virtue of their *presence*, and without themselves combining with either constituent. The numerous cases in which this is assumed to take place have led to the invention of a particular term, *catalysis* (dissolution), to indicate the property in question; thus marking the distinction between what takes place here and that which occurs in a process of *analysis*, where a new substance being presented to an existing combination annuls it by uniting itself with one of the constituents. The following are instances of the kind.

Platinum, especially when in that state of minute division in which it is known as spongy platinum, has the power of occasioning the union

of various inflammable gases with oxygen, at a temperature at which they would otherwise remain together without combining. The peroxide of hydrogen, or oxygenized water, is resolved into water and oxygen, (the latter passing off with effervescence), by the contact of various substances, such as the alkalies, metallic oxides, as well as metals, which do not combine with the oxygen liberated. In like manner, the nitro-sulphuric acid of Pelouze is decomposed, by spongy platinum, by oxide of silver, by metallic silver, by powdered charcoal, and by other substances, which are not acted upon by any of the constituents of the compound which they decompose.

These cases of catalytic action are taken from the inorganic kingdom; but they occur much more commonly in the processes of organic chemistry. Of the vegetable principles upon which it is distinctly exhibited, one of the best instances, perhaps, is starch; which undergoes the changes into sugar, gum, and the substance called by the French chemists *dextrine*, under the operation of agents which do not enter into union either with it, or with the ultimate principles of which it consists. This change is effected in the living vegetable by the operation of a secretion termed *diastase*; but the chemist can imitate it by various other substances. The action of sulphuric acid upon alcohol, in producing ether, appears to be of the same nature. It has commonly been attributed to the affinity of the acid for water, which causes it to abstract from the alcohol the proportional which it contains, and so to convert it into ether; but it has been objected that potass, chloride of sodium, quicklime, and other substances which possess an equal affinity for this liquid fail of converting alcohol into ether; and Mitscherlich has shown that, if alcohol be poured upon sulphuric acid at a temperature higher than that of boiling water, a proportion of water and ether, exactly equivalent to the alcohol employed, will distil over; so that the sulphuric acid, having united with neither of the constituents, must be regarded as having acted catalytically. This new principle furnishes us with a clue to the formation of those many products, which result from the selfsame fluid in the living animal and vegetable. In this way we may comprehend how from the sap alone, sugar should be produced in one part of the plant, camphor in another, an essential oil in a third, caoutchouc or milky juices in a fourth; and how, from the blood of the animal, the various secretions and organizable products should be elaborated, which are characteristic of the different parts.

The proposition under consideration will also, in another sense, be found to embrace only a portion of the truths which modern chemistry has revealed to us; since it overlooks the very remarkable influence which cohesive attraction exerts upon chemical combinations. This is remarkably exemplified in the solubility of recently precipitated silica in water, as compared to its entire insolubility when in a state of aggregation; and the knowledge of this fact affords us the only clue we possess to the knowledge of the mode in which this is introduced into the juices of plants, and other positions from which its nature would seem to exclude it. But, according to Dr. Daubeny, something different from what is commonly known as cohesive attraction must be here understood.

"There is another mode (he remarks) in which the attraction between like bodies appears to operate, bearing, as it would seem, somewhat of the same re-

to the cohesive attraction which binds together their particles, as the at- n between opposite masses of matter of dissimilar natures, when in op- electrical conditions, bears to the elective attraction which exists between imate atoms of each. This has frequently been applied by geologists and to explain certain phenomena that have presented themselves to their ation; but has not, so far as I am aware, received the attention it deserves hemists. It is well known that, if an intimate mixture of finely com- d sand and clay is left together, as in the common operations of pottery, ca will, after a certain time, be found collected in clusters. Dr. Faraday own that the same principle causes camphor, if suffered to evaporate in a essel, to arrange itself round certain nuclei on the surface, instead of equally disseminated throughout. The same principle has been called in ain the nodules or concretions of one kind of stone, as of flint, so fre- y found to occur in rocks of another description. The adhesion of smooth of glass or of metal, when brought into close contact, and the property of o adhere to solid substances, which gives rise to the phenomenon of ca- attraction, and to the hygrometric power belonging to certain earths, may unted for in the same manner; as may also the capacity, which most substances possess, of absorbing, and even condensing within their pores, es with which they happen to be surrounded. The curious manner in gases appear in some cases to adhere to metallic surfaces, without actu- ombining with them, seems another fact in illustration of the same le.

conceive that the circumstances by which this singular kind of attraction lated deserve a more full investigation, and that it is highly proper to nish it by a separate name from the cohesive attraction subsisting be- the particles of matter. Perhaps the term *adhesive* attraction, which has ready proposed for it, would be sufficiently characteristic. It has been med, indeed, whether this species of attraction is essentially different hat which we denominate cohesive; and it may safely be conceded that mary cause of both will most probably turn out to be the same. But so, , may cohesive attraction itself, according to the ingenious generalization soti, be included under the same law as that of gravitation; and the al affinities of bodies be regarded as the consequences of their physical ties, as Persoz has attempted to demonstrate.

one of these hypotheses however, even if they were fully established, and completely developed than they can claim to be, ought to prevent us anging under distinct heads, and treating as separate branches of science, several phenomena, regarding the attraction which operates between of matter distinct from that which takes place between their particles, trical attraction is from chemical affinity; or as the repulsive energy l by heat upon masses of matter is from the same force which imparts tic condition to their particles." (pp. 51-4.)

only one of the propositions first stated that now remains for us sider, is that one which set forth "that the water attached to many , though essential to their crystallization, does not in other re- affect their properties." This has been greatly modified by recent ches, especially those of Professor Graham; which have shown ater contributes, not merely (as was supposed) to the particular trical form which a mineral has a tendency to assume, but like- o the chemical relations it possesses towards other substances. sor Graham establishes, in the first place, that acids, such as the ioric, combine with water in certain definite proportions, just as o with bases; that one atom of water is in most cases necessary r existence; but that they disengage this atom when they unite equivalent quantity of any other base. Thus water acts as an acid

with bases, as in the case of hydrate of potass, lime, &c.; and as a base in the instance of acids, as the liquid sulphuric and nitric acids testify.

We may, therefore, distinguish several states in which water exists as the constituent of a compound. 1st. As *water of crystallization;* that is, essential to the crystalline form which the substance assumes. 2d. As an *acid*, combined with alkalies, earths, &c. 3d. As a *base*, being essential to the existence of acids, with which it is combined. To these Professor Graham has since added a fourth condition, in which it may occur; forming what in his last Memoir (1837) he has called *constitutional water*. In this case, we have a certain proportion of water united to a given salt, but capable of being driven off from it by another, in consequence of the stronger attraction which the latter exerts. Thus, the sulphates of magnesia, zinc, &c. contain besides their water of crystallization, a proportion of constitutional water, which may be replaced by sulphate of potass. This constitutional water is expelled with more difficulty than water of crystallization. Thus, of the seven equivalents of water which crystallized sulphate of zinc contains, six may be easily driven off, but one remains in combination at 410° and all inferior temperatures; and it is this one which is replaced by sulphate of potassa, so as to form what has been hitherto termed a double salt, without any change in external form. It may, perhaps, be questioned whether this constitutional water is not really acting as a base or acid; the salt with which it is united serving as a compound radical. The curious analogy between the effects of heat on plaster of Paris and the arsenic and phosphoric acids would seem to support this view. Plaster of Paris appears to consist of a sulphate of lime and two equivalents of constitutional water. At a temperature not exceeding 270°, this salt loses its water, but retains the power of recombining with it, or *setting;* but if heated above 300°, it becomes properly sulphate of lime, and loses the tendency to recombine with water. Now, according to the researches of Professor Graham, phosphoric acid cannot be obtained except in combination with *three* equivalents of water, which is here termed *basic*, and which may be wholly or partly replaced by other salifiable bases. Thus, there are three phosphates of soda, known under the names of the *alkaline*, *neutral*, and *acid* phosphates; in which *one*, *two*, or *three* of the atoms of water have been replaced respectively by the soda. But on exposing phosphoric acid to heat, one atom of water is driven off; and an acid remains, termed the paraphosphoric, having the same proportions of phosphorus and oxygen as exist in phosphoric acid, but combined with only two equivalents of water, and capable of only receiving *two* proportionals of a base in their stead. And if this acid be exposed to a still higher temperature, another atom of water is driven off, and the product is metaphosphoric acid, which is only capable of uniting with one proportional of a salifiable base. Arsenic and telluric acids have a tendency to form similar combinations.

This principle, that the acid possesses an affinity for just so many atoms of a base as the number of atoms of water with which it is already combined, is probably by no means limited to the cases just mentioned; and it has been ingeniously brought, by Liebig, under the general fact, that no acid has any tendency to combine with a base, except when it already contains water, or at least hydrogen, in the following manner.

hosphoric acid owes its acid properties to the presence of a certain roportion of water; and without it, like dry tartaric acid, has no tenency to combine with bases. Accordingly, when one atom of water is stracted from phosphoric acid, the paraphosphoric acid which remains ay be regarded as an admixture of one proportional of anhydrous nd therefore inactive) phosphoric acid, with two which still retain their mbining power. And when two atoms are abstracted, so as to form etaphosphoric acid, we may view this as really composed of two prortionals of anhydrous, and one proportional of active, phosphoric id.

The important part which, in these cases, water seems to perform, in parting to the compound into which it enters as an ingredient its most tive properties, and the impossibility of pointing out a single acid stitute at once of water and of its constituent hydrogen, are the rcumstances that principally lend a foundation to the bold theory ready noticed, which supposes all the acids to consist of a compound dical, united with hydrogen as an acidifying principle. The arguments favour of this view are stated with much clearness by Professor raham, (Elements, p. 389, et seq.;) but they need not be here dwelt n; as the several theories suggested on these points are admitted by e best authorities to be so nearly balanced, that new discoveries may t any moment give the preponderance to one over the other.

The only other important modification in the hitherto-recognized priniples of chemical philosophy that we shall now notice is that which as been termed by Dumas (by whom it has been proposed) the *law of bstitutions*. We have not been able to find any single statement of it hich gives to our minds a sufficiently clear view of its nature, as disinguished from the general law of definite proportions on the one hand, nd the doctrine of isomorphism on the other. It is founded upon the dea that the several elementary bodies may be divided into groups, the embers of which may severally replace or be substituted for each ther, in any compound into which one of them enters, without producing any essential change in its general chemical relations. This principle was first applied to the action of chlorine on organic compounds. When chlorine acts upon these bodies, for every atom of hydrogen that is abstracted in the form of hydrochloric acid, an atom of chlorine is left in its place. The same doctrine has been successfully applied to the action of oxygen and other agents upon the same bodies. Thus, in the oxidation of alcohol during the acetous fermentation, hydrogen is withdrawn in the form of water, by combining with oxygen, and at the same time the hydrogen is replaced by an exactly equivalent quantity of oxygen. By the pursuit of this principle, chemists have been led to the discovery of many new and important combinations, and their operations have been much simplified, so that they are now able to perform in a short time what was formerly the labour of years. This results from the power which is obtained by it, of predicting (in cases entirely unknown) what will be the results of particular decompositions. It seems to be one of those subordinate laws, " limiting the generality of the law of definite proportions in particular cases, diminishing the number of combinations abstractedly possible, and restraining the indiscriminate mixture of elements," which we formerly spoke of (Vol. V. p. 323,) as remaining to be discovered. One of the examples adduced

by M. Dumas will perhaps set this point in a clearer light. If acetic acid be subjected to the influence of chlorine, a new compound will be formed, in which the oxygen of the original acid has been replaced by chlorine. This is called chloroacetic acid; and as it fully retains its power of saturation with a base, it must be regarded as belonging to the same *type* with acetic acid. Now if chloroacetic acid be boiled with an alkali, it is destroyed, and is changed into carbonic acid and chloroform (a compound of six chlorine, two hydrogen, and four carbon). If acetic acid is treated in a similar manner, it may be surmised that it would resolve itself into carbonic acid, and a carburet of hydrogen; but *which* of the several hydrocarburets nearly similar in composition would be produced by it could not be predicted without the aid of this law. The knowledge that the two compounds in question are of the same chemical type, however, would enable the chemist to assert that out of at least four possible products, that one would be evolved which corresponds most nearly in composition with chloroform, namely, the light carburet, or marsh gas; this consists of eight hydrogen and four carbon, and is changed by the action of chlorine into chloroform (six proportionals of chlorine being substituted for the same number of equivalents of hydrogen), thus bearing the same relation to it as acetic to chloroacetic acid. This is what is found experimentally to be the case. Those of our readers who wish to have M. Dumas's own account of his discovery may consult with advantage his recent Memoir upon the Law of Substitutions, and the Theory of Chemical Types, (Comptes Rendus, Feb. 5, 1840; and Philosophical Magazine, May, June, and July, 1840.)

We have already recorded our opinion that the place at present held by chemistry in the curriculum of medical study is much too high; and we doubt not that it may have occurred to many of our readers, during their perusal of the foregoing pages, that, however interesting in themselves, the statements there put forth had no very direct bearing upon the objects to which the life of the practitioner is devoted. His therapeutic employment of ether is not affected by the dispute, whether it is to be regarded as an oxide of ethule or a hydrate of olefiant gas; and he can give a dose of sulphate of magnesia just as well whether he believe it to consist of sulphuric acid and magnesia, or of sulphatoxygen and magnesium. The practical applications of chemistry will be, for the most part at least, the same, whatever theory we may adopt of the abstract nature of the changes themselves. To the medical student (as we formerly observed) no more acquaintance with chemistry is essential than that which will prevent him from mixing incompatibles in his prescriptions, and will enable him to conduct, with tolerable skill, the search for the commonest poisons. To endeavour to cram him with more is, in our view, only to prevent the acquisition of, or to drive out if already acquired, knowledge of a much more useful description. We are ready to admit that the pharmaceutist ought to be further instructed; yet even here, a very moderate knowledge of chemistry will ordinarily be sufficient for those who have no ambition of exploring new fields of research, but are content to go on in the beaten path. He who devotes himself to the business of discovery in *any* branch of science must undergo a special preparation for it; and it can scarcely be fair that such preparation shall be obligatory upon every one, however limited the sphere of operation which he contemplates.

These remarks are intended peculiarly to apply, however, to the present state of chemistry. As the practitioner of medicine and the pharmaceutist have gradually been separated, chemistry, like botany, evidently belongs to the latter; so far, at least, as these sciences are united to the supply of therapeutic agents. And the peculiar complexity which necessarily attends the present transition state of chemical philosophy, and the unsettled condition of what have been for long regarded as its most stable foundations, furnish other reasons why the medical student should be perplexed with it as little as possible. We look to a future, however, in which the discoveries now in progress regarding the real nature of organic compounds shall be brought to bear effectually upon therapeutics; and shall not only furnish us with many new and important medicinal agents, but give valuable assistance in the discovery of principles, far more certain than any we at present possess, which shall be the guide in their application. But the prosecution of the researches by which this shall be accomplished is not to be looked for from the medical practitioner. With him it will rest to bring into application the results, as they are respectively attained; but organic chemistry is a science which, to pursue with any prospect of success, requires the devotement of the whole time and attention. As we have formerly stated, we consider it an important object in general education, to communicate as comprehensive a knowledge as possible of the *principles* of science; since, by this preparation, the subsequent pursuit of any one of them will be greatly facilitated. And we are by no means disposed to exempt chemistry, even though its present condition appears so unsettled. The general expressions of its phenomena may be communicated, without the theoretical explanations of them; in the former all agree, whilst on the latter almost every teacher would give a different opinion. There is, in fact, no more essential connexion between the theory and the phenomena to which it applies, than between the question as to the abstract nature of light and the laws of optics. The latter may be taught, as every one knows, without in the least degree involving the former.

Now it is in this manner, we think, that chemistry ought to be taught at present. Those general laws should be stated which are but comprehensive expressions of facts, which remain unchanged by any modification of theory, and which serve as the guide in predicting the effects of the operations to be performed; but very little further would we go. A student who learns a particular theoretical explanation from a teacher whose authority and talents he respects, is very apt to retain it in spite of clear evidence to the contrary, even after his master himself may have abandoned it; and such a tendency must check that spirit of enquiry, which is one of the most valuable results the student can obtain from this kind of training, as well as produce positive injury by habituating the mind to the retention of what has been proved to be erroneous. There is, perhaps, more difficulty in teaching chemistry on this plan than most other branches of science, since the nomenclature is so much involved in its theory; but we are so far conservative in our tendencies, that we would rather have a public teacher hold by the old as long as he can (provided he make it clearly understood that it may not improbably have to undergo modification), than be carried about by every wind of doctrine, and be always grasping at novelty. The difficulty is no small one; especially when the teacher is himself labouring to advance the theoretical depart-

[illegible]

[illegible] Researches in [illegible] chemical [illegible] and [illegible] at [illegible] the great progress of [illegible] extend our views— [illegible] to my [illegible] mode of carrying his designs into [illegible] that these [illegible] not be simply [illegible] a preliminary [illegible] embodying an [illegible] "Thus, I was gradually led to include [illegible] preparatory view of the forces which may be said to [illegible] of chemical phenomena, as it is absolutely necessary for the student to master, who aspires to anything beyond the [illegible] operations of practical chemistry." In accordance with this plan the author, after some general remarks of great excellence on inductive philosophy, considers what is meant by *matter* and *force*; the [illegible]; the laws of gravitation, with specific gravities; elasticity, elastic fluids and sound; homogeneous attraction and repulsion; heterogeneous adhesion; and crystallization. He then proceeds to the laws of heat and light, and thence to electricity and magnetism. Thus prepared, he enters upon what are usually termed chemical phenomena; and, after a general view of the nature of chemical affinity and the law of definite proportions, he illustrates it by a sketch of the properties of

metallic elements and their most important compounds. From asses to the more complex subject of concurring forces, and ition in the phenomena produced by the influence of these; and inder of the work is devoted to an exposition of the connexion electrical and chemical phenomena, as developed by the re- of Faraday. We have seldom examined a work which has more satisfaction, both in its design and execution, or which nore strongly recommend as calculated to afford sound know- its subject in a clear and attractive form.

ulky Vegetable Chemistry of Dr. Thomson professes to lay be- British chemical public a pretty full view of the present state of artment of the science. To us, however, the mode in which has been accomplished is far from being satisfactory, and we iink the work worthy of the distinguished author's reputation. ins an immense mass of materials, but they are ill digested. of analyses, good, bad, and indifferent, are brought together, any proper directions to the student as to their respective value; who is not previously acquainted with the name and character of iist whose results are set forth, will be quite in the dark as to the ity of their accuracy. To the advanced student the work will have lue, as comprising a vast amount of materials which he would e have had to seek in many scattered sources: it is not suited eginner.

Art. VII.

'ydrocephalus, or Water in the Head, an Inflammatory Disease, urable equally and by the same means with other diseases of In- ation. By David D. Davis, m.d., m.r.s.l.; Professor of tric Medicine in University College, and one of the Physicians iversity College Hospital.—*London*, 1840. 8vo.

we first glanced at the title of Dr. Davis's work, we suspected iad chosen a subject too fully investigated by preceding authors much room for fresh discoveries,—at the present time. Whether rovement in the mode of treating the disease had occurred to r, was a question which we hoped to have the opportunity of g in the affirmative. An attentive and impartial examination of me has shown us that our suspicion was correct; for we have discover in it the smallest addition to our pathological knowledge. we award higher praise to the treatment advocated, which can be considered novel, even in its details. The literary character ork is even still more defective, and can hardly be regarded by the ient judgment as creditable either to the author or the celebrated which he belongs. No doubt the extremely faulty style in which eal of the work is written, must be attributed to mere carelessness art of the author, as it is impossible that the manifold blunders ch it abounds, can be the result of ignorance. And even on mption we cannot acquit the writer of blame of no light kind, pecially in his relation as a teacher of medicine. If his avocations

prevented him from paying that attention to the composition and editing of his book which every book ought to receive, he might surely have obtained the assistance of some of his literary friends of greater leisure than himself; and we venture to affirm that he could have found many pupils in his own college, the revision of it by any one of whom would have saved his reputation from the literary discredit which, in the judgment of persons ignorant of his real powers, must attach to it after the present publication.

There is another unfortunate character of composition observable in several parts of Dr. Davis's book, however, for which the most lenient interpretation must hold him exclusively responsible, as it is the result of evident intention on the part of the author: this is the fashioning his style to meet the comprehension and views of the "general" reader. Our pages bear ample testimony of our desire that the public should be instructed in everything relative to the preservation of health and the prevention of disease; and we conceive that the medical philosopher can aspire to no higher or more dignified office than that of such an instructor. But we must ever deprecate the attempt in works on scientific or practical subjects, designed for the profession, to write down to the standard of non-professional readers. Whether intended or not, the effect of this is—when it has any effect, although we suspect the results are generally of a negative kind—not so much to instruct such persons in matters which they can understand, and which may be useful to them, as to turn their thoughts towards the doctor who writes so incomprehensibly-learnedly, and who, doubtless, can heal so incomparably-cleverly.

If our limits will allow, we may, before concluding, extract a few passages from Dr. Davis's book in illustration of our meaning, and in justification of our censure. For the present we turn to the more agreeable task of examining the strictly professional character of the publication.

The object of the author, as stated in his preface, is to prove that acute hydrocephalus is an iuflammatory disease of the vessels and investing membranes of the brain, and that it calls for the same remedies as other inflammations. The accuracy of this doctrine is inferred from the disorganization of the blood common to this and to all inflammatory disorders, and evinced by the following morbid appearances: 1, opacities and fibrinous adhesions of the contiguous membranes of the brain; 2, fibrinous coatings, linings, and deposits on the surface of the organ, and within its cavities; 3, serous effusions, both on the cerebral surface and between the convolutions, and also between the membranes; 4, occasional purulent formations, with disorder of tissue, as of the temporal bone, including the organ of hearing; 5, the presence of a peculiar fluid in the ventricles, infiltrated from the blood by the inflamed cerebral tissues, and which Berzelius regards as serum of the blood, diluted with seven times its volume of water.

The various theories of acute hydrocephalus are historically sketched in the Introduction, and this we deem the best part of Dr. Davis's work; although even this is marked by some striking defects. The disease was first described as a distinct affection by Mr. Pasley, of Glasgow, who records a case of it in the third volume of the Edinburgh Medical Essays. In 1768 it was the subject of a monograph by Dr. Robert Whytt, Professor of Medicine at Edinburgh, and also gave occasion to several papers

by Drs. Fothergill and Watson in the fourth volume of the Medical Observations and Enquiries. In the sixth volume of the same work is a case of the disease cured by mercury, published by Dr. Dobson, of Liverpool, in 1775. Up to that period the current theory was, that hydrocephalus resulted from effusion into the ventricles, consequent on a laxity of the exhalents, or on a watery state of the blood; in short, that it was an acute idiopathic dropsy. In opposition to this theory, Dr. C. Quin published an inaugural dissertation in 1779, in which he maintained that hydrocephalus is inflammatory, and consists in plethora of the vascular tissue of the brain, resulting in the effusion of an aqueous fluid, and demanding an antiphlogistic treatment. This opinion was expressed by Dr. Withering in his account of digitalis, published in 1785; and also by Dr. Rush, of Philadelphia, in an essay published in 1793; by Dr. Patterson, of Londonderry, in a series of letters to Dr. Quin, printed at Dublin in the following year; and by Dr. Garnet, in a short essay contained in the fifth volume of the London Medical and Physical Journal for 1801. The doctrine was combated, though not the treatment, in the essay of Dr. J. Cheyne in 1818. In 1814 Dr. Carmichael Smyth endeavoured to revive Whytt's theory of effusion from debility and relaxation of vessels, and expressed a conviction that "neither the brain itself nor its membranes in any case of genuine hydrocephalus that ever yet has been examined, showed any appearance of inflammation, or of the usual effects of this having taken place." Professor Monro, of Edinburgh, still holds the opinion of Smyth. The next writer on hydrocephalus was Dr. Yeats, who addressed, in 1815, a letter to Dr. Wall, Clinical Professor at Oxford, urging attention to the premonitory symptoms, which he says are easily controlled; but if neglected, lay the foundation for the disease. Some time after appeared the highly practical and valuable work of Dr. Gölis, of Vienna, translated by the late Dr. Gooch. Of this work Dr. Davis tells us he intends, in the ensuing pages, to point out the superiority to other treatises, and to supply any practical deficiencies.

We expected that some notice would have been taken of the labours of some of those French pathologists who support the same side of the question as our author; for instance, Martinet, Parent-Duchatelet, and Lallemand. The omission is inexcusable in this age of pathological research. Some mention, too, should have been made of that curious discovery of Magendie and Guillot—the hygrometric property of the brain. The first of these physiologists pointed out that, to show the cerebro-spinal ventricular fluid, the animal must be examined immediately after death, otherwise the fluid is absorbed, and disappears in the midst of the nervous substance. Guillot has shown, first, that during life, and in health, the ventricles of the brain are full of serosity; that the quantity of this fluid decreases in proportion to the interval between death and the dissection; that the fluid so disappearing is to be found in the cerebral substance; that the brain is hygrometric, so that a piece of the cerebrum of a dog just killed, and plunged into water or serum, absorbs its own weight of these fluids; and that differences exist in different animals, and in the same animals, according to age and the interval that has occurred between its death and the period of examination. A new light is cast on the subject of cerebral effusion by this discovery, and henc for-

ward it will be necessary, not merely to estimate the quantity of fluid found in the ventricles or cells of the subarachnoid cellular tissue, but likewise of that contained in the substance of the brain itself. If this organ is soft, and yields on pressure a large quantity of fluid, we are warranted in concluding that the effusion is dropsical. This is seldom seen, however, in acute hydrocephalus; and, when it is, it must be regarded as accidental.

Within the last fifteen or twenty years a doctrine has sprung up in the French school, which attributes acute hydrocephalus to deposition of tubercular granulations on the under surface of the cerebral layer of the arachnoid membrane; in other words, maintaining the strumous nature of the disease. MM. Guersent, Gherard, Rufz, Piet, Bequerel, and Dr. Hennis Greene, have zealously supported this view, and have accumulated a vast mass of evidence in its support.

Dr. Davis passes over the distinction of external and internal hydrocephalus, and the several forms of each, and restricts himself to that form of the latter in which the effused fluid is contained in the ventricles of the brain. He divides this into three stages: 1, the formative (or predisposing); 2, the inflammatory; and 3, that of effusion, which is commonly fatal.

The first or formative stage of hydrocephalus is more diffusely described than is compatible with clearness. We do not think it necessary to give even an abstract of the description, which varies in no important respect from what is found in every work of character on the subject. We were struck with the omission of one symptom, namely, alternate evening febrile exacerbations and remissions, constituting "Infantile Remittent Fever," which is certainly a highly important premonitory symptom of the disease.

Minutely as the second or inflammatory stage is described, yet no mention is made of the contraction of the pupil at the commencement, which marks the highly irritable retina; nor yet of the change from contraction to dilatation, so uniformly observed when effusion is taking place. Now this state of the pupil is as important a sign as the slow, unequal, and intermitting, "and easily quickened pulse, which indicates effusion ...." "*An eruption*" on the shoulders and nape of the neck is said by Dr. Davis to be symptomatic of this and the third stage of the disease; but of its specific character nothing is said.

The symptoms of the third stage are accurately related, allowance being made for obscurity and other vices of style, which in this part of the book seem to have attained their highest pitch. The general description of symptoms would have been more complete if some notice had been taken of their chief varieties, which are sometimes a puzzle to the medical attendant. Thus, the premonitory stage may be absent, and the attack be *suddenly* ushered in by strong convulsions. Sometimes the pulse is never slow, or never presents that change from slowness to extreme frequency, which is so characteristic of the last stage. Occasionally there is neither permanent contraction, nor dilatation of the pupil, but an irregularity in the contraction of the iris; so that when a candle is brought near it will dilate instead of contracting, and when the candle is withdrawn, it will contract instead of dilating. Again, children have been seen to retain their consciousness to the last.

The chapter on the diagnosis of acute hydrocephalus, under the head of athognomonic symptoms, is a mere abbreviation of the previous one on ymptoms. It offers no clue to the more exact discrimination of this om the other maladies for which it may be mistaken as softening, rachnitis of the base of the brain, and of the ventricles (Martinet and 'arent-Duchatelet), inflammation and ulceration of the ileum and of e other small intestines, and intestinal worms. The subject of diagnosis in point of fact absolutely overlooked: a striking proof of the negli-ent and hasty construction of this work, notwithstanding the self-com-lacency with which the author speaks of "the above *accurate and arefully drawn* picture," alluding to his diagnostic sketch.

In treating of the specially predisposing causes "to" (of) acute hydro-ephalus, great stress is very justly laid on the irritation of teething, hough there is nothing new in this to justify the phrase, "it may be here dvantageously *intimated* to the reader that the irritation from teething s in nine out of ten cases, the occasional cause of acute hydrocephalus luring the three first years of life." Another frequent predisposing ause is the too common substitution of artificial for the natural food, luring the early period of infancy, a practice justly condemned by the uthor. A curious example of the predisposing power of mental emotions of the mother during the close of pregnancy, is extracted from the work of Dr. Gölis, who relates that in 1809 most of the children born after the ombardment of Vienna, were seized with convulsions, twenty or thirty lays after birth, and died. Traces of inflammation were found within he cranium, and in the ventricles effusions of lymph and serum. The ther predisposing causes enumerated, do not call for any observations.

Dr. Davis devotes a large portion of his volume to the examination of he proximate cause of hydrocephalus. He tells us that collections of serous fluid in the cerebral cavities were seen and are recorded by noso-logists prior to the last century, but were attributed by none to inflam-matory action. A dissection of this kind was given in 1676 by Borelli, in his Histories and Medico-Physiological Observations (obs. 38); an-other by Morgagni, in his great work De Sedibus et Causis Morborum, (ep. 1, act. 2;) and others again in the 6th, 7th, and 8th ep. of the 1st book. We are warranted in concluding from the silence of writers at this period, that the inflammatory origin of these effusions was unknown, and indeed until the appearance of Dr. Quin's dissertation in 1779, upwards of a century after, no new light was thrown on the subject. This author is undoubtedly the parent of the inflammatory theory, though from his father he learnt the distinction of hydrocephalus into acute and chronic. The inflammatory nature of the acute variety, he inferred from observing that when the headach was severest, vascular excitement was strongest, and that the superficial vessels of the head evinced a plethora, further shown by repeated epistaxis before the attacks. The vessels of the cerebrum and cerebellum too were found by him much injected; and opacities, thickening, patches of coagulable lymph, and serous effusions in the ventricles, were observed in several post-mortem examinations. It is singular that neither Dr. Quin nor any of his contemporaries adopted a line of treatment in accordance with this doctrine. Indeed, it is only of late years, that the profession has treated hydrocephalus on the antiphlo-gistic plan; the contrary doctrine inculcated by Dr. Carmichael Smyth

having had great weight in retarding the progress of the new theory. The works of Drs. Underwood, John Clarke, and Abercrombie, corroborate the modern view, yet Dr. Davis, never naming the two latter authors, questions if it is generally received, and says, "but unfortunately, this pathology has been more of a tacitly admitted principle, which, however, has not been unfrequently denied, than a doctrine of an *operative living faith*," (p. 211.) A series of cases with dissections from the admirable work of Gölis, are introduced to show that an inflammatory state of the blood-vessels of the encephalon is the true proximate cause of hydrocephalus; and these are succeeded by several other dissections, published by Dr. Cheyne shortly before the date of Gölis's work.

Here Dr. Davis closes his argument for the inflammatory nature of acute hydrocephalus; and though we agree in much that he has said, and admit the weight of the concurrent testimony he has adduced, yet we cannot award him the praise of having shed any new light on the pathology of hydrocephalus. Neither in his arguments nor in his pathological researches, do we discover any of that cogency or originality which we were led to expect.

Treatment. The most important of the remedies of hydrocephalus is, in Dr. Davis's opinion, *bloodletting;* which may be employed as a prophylactic in the premonitory stage, when the only symptoms are cerebral vascular turgescence with pyrexia. The first bleeding, to be effectual, should be carried to complete fainting, the blood being drawn quickly from a large orifice. A repetition of the depletion will rarely be necessary, as the disease will generally have been arrested by this vigorous treatment. For a child below five years of age, Dr. Davis considers cupping behind the ears preferable to venesection, on account of the diminutiveness of the veins at the bend of the elbow, and to leeches, on account of the difficulty of computing the exact quantity of blood abstracted by them. The extent to which cupping may be carried is thus laid down: From an infant a year old, from 4 to 5 oz. of blood may be abstracted, and half an ounce or an ounce more on the second day of stupor, especially if the head is hot; from one two years old, from 5½ to 6 oz.; from one of from 3 to 5 years of age, from 5 to 10 oz.; from one of from 6 to 10 years of age, from 10 to 18 oz. But in all these cases, fainting should be produced. Next to cupping, the opening of the jugular vein is most recommended.—Agreeing generally with this plan of treatment in decidedly acute cases in children of ordinary powers, we must caution our younger readers against its indiscriminate employment. There are not a few cases, even of the inflammatory kind, in which it is only applicable in a most modified degree; and there are many cases of hydrocephalus in which *it* is altogether inadmissible.

As soon as the child has pretty well recovered from the faintness induced by the bleeding, an emetic should be given; for instance, ¼ gr. of tartar emetic with 5 grs. of ipecacuanha, and vomiting promoted by draughts of fluid at intervals. In this way the morbid excitability of the heart and arteries is powerfully subdued. The sickness and nausea may continue for several hours with ultimate advantage, a fact which induces Dr. Davis to add this piece of encouragement by way of caution, "Whilst on this subject, it may not be improper to caution *parents* against placing any confidence in certain alarming, and other-

ise uncalled for observations of *druggists' apprentices, and other young entlemen occupied in shops of pharmacy*, about the magnitude of doses f medicines intended for young children."

Next to bloodletting and emetics, the author places the reduction of emperature, by means of cold applications; for instance, "iced water, r other *solutions of water* impregnated with a variety of salts, vinegar, &c.," and contained in a Mackintosh's water-cushion, which should be hanged every half hour, as long as it appears agreeable to the child. n the incipient stage of the disease, if the naked hand is applied to the ncovered forehead or occiput, a marked excess of temperature will be oticed, and more accurately still, by a pocket thermometer. As long s the head continues preternaturally hot, so long will the infant, if concious, appear to hail the cushion, and so long ought it to be continued.

To blisters, the author does not assign a high place among the remedies f acute hydrocephalus; he believes them absolutely injurious when used s a first remedy, without previous ample bloodletting. He recommends that they should be applied over a large surface of the parietal egions, the forehead and occiput being constantly bathed with evaporating lotions.

Mercury he considers a valuable remedy in this disease; as a purgative he gives it in the form of calomel in doses of 2 or 3 grs., sometimes twice that quantity, with 6, 8, or 12 grs. of jalap, according to the patient's age, as soon as the sickness consequent on the emetic, shall have ceased. Where the bowels are obstinately costive, he adds occasionally $g^{tt.}\frac{1}{4}$ to $g^{tt.}\frac{1}{2}$, or even more, of croton oil. Calomel is no less valuable as a cholagogue, when the secretion of the liver is suspended, or in other words, its circulation much obstructed; which frequently predisposes to affections of the brain, by destroying the balance of the circulation, so that congestion of the cerebral vessels takes place. When exhibited with a view to promote the biliary secretion, Dr. Davis advises the dose to be from 1 to 3 grs. every three hours. Frequently a very active purgative will be required at the same time. A third object in employing calomel is to produce its specific effect, with the view of subduing inflammatory action. On this, however, the author tells us he does not much depend, except the system shall have been reduced by the lancet. At the same time he would administer calomel in cases where bleeding would be too late, or otherwise inadmissible. On the whole, he considers mercury inferior as a curative agent to any of the other active remedies enumerated, and this opinion derives confirmation from the results of Dr. Cheyne's treatment of twenty cases of hydrocephalus by bleeding and calomel. The number of recoveries was only nine.

Such is Dr. Davis's account of the treatment of acute hydrocephalus. A series of paragraphs, thirteen in number, succeeds, detailing the "circumstances indicative of danger of acute hydrocephalus, which should operate as a warning to *parents and guardians* to take the advice of their medical friends in good time." We cannot see the use of this superfluous recapitulation. If there are so many indications of liability to the disease, parents and guardians will be bewildered by the mere enumeration, even if they could be supposed competent to appreciate the indications and the succeeding cases which are meant as illustrations.

The work closes with the details of six cases, successfully treated by

the author at University College Hospital, on the rigorous plan alrea described. We are told that the records of the institution present simi results in the vast majority of cases, and are assured that no less succ may be expected from the treatment in private practice, so long as disorder is submitted early enough to the notice of the practitioner.

In thus closing our sketch of the author's treatment of hydrocephal we cannot help repeating that it has no claims either to novelty originality; scarcely a modern author or practitioner of reputation professes similar therapeutic principles. If the reader will but turn Dr. John Clarke's excellent Commentary on the Diseases of Childr he will read a confirmation of our remarks in the observations on bloc letting in inflammation of the brain, and in short, on the treatment the disease in general.

ART. VIII.

Cancer. By WALTER HAYLE WALSHE, M.D. (*Cyclopædia of Practi Surgery,* Parts VI. & VII.)—*London,* 1840. Royal 8vo, pp. 102

THE great importance of the subject of cancer, and the admirable a elaborate manner in which it is here treated, induce us to deviate fr our usual practice of not noticing individual articles in periodical wor With the exception of one or two intentional omissions, to be suppl under the proper alphabetical heads, Dr. Walshe's memoir, indeed, m be considered a complete monography. It is equal in size to many d tinct works, and surpasses in merit the majority that come into hands. It is divided into two parts: in the first of which cancer in ge ral is considered; and in the second, the disease as it occurs in those sues and organs in which it is likely to come under the notice of the pr tical surgeon. The first part is that to which our attention must be pı cipally confined. Turning to the end of it for an instant, we extract definition of cancer, which Dr. Walshe deduces from all that he has s before: "Cancer is a disease anatomically characterized by the prese of scirrhus, encephaloid, or colloid, originating in a general vitiation the economy, and possessing the properties of assimilation, of reprodu tion, and of destroying life by a peculiar cachexia." (p. 650.)

Assuming *cancer* or *carcinoma* as a genus, Dr. Walshe recognizes e cephaloid, scirrhus and colloid, as species. But, as most writers on t subject have hitherto been more keenly perceptive of the differences these products, he finds himself under the necessity of insisting on th analogies, as maintained by Young and Carswell, and Laennec, Ott Cruveilhier, and Müller, in order to justify his grouping them togeth and separating them from all other morbid formations. Accordingl he refers to the points in which they agree *anatomically*, *chemicall physiologically*, and *pathologically;* and then remarks that their title be united is quite as strong in respect of practical medicine and surge as in respect of scientific pathology; a consideration, it is justly observe of the highest importance.

In a table, which we subjoin, our author displays the method, accor ing to which, as apparently best in accordance with the present state knowledge, he treats of carcinoma in general.

Family.	Class.	Order.	Genus.	Species.	Varieties.	Synonyms of the Species.
Adventitious Formations.	Heterologous Formations.	Tissues.	Cancer, or Carcinoma.	Encephaloid	Common vascular sarcoma. Mammary sarcoma? } Abernethy. Solanoid. Recamier. Zang. Nephroid. *Idem.* Napiform. *Idem.* Carcinoma fasciculatum vel hyalinum. Müller. Fungus hæmatodes. Hey. Hæmatode cancer. Auct. Gall.	Spongy or ossivorous tumour. Ruysch. Palletta. Struma fungosa (testis). Callisen. Spongoid inflammation. Burns. Milt-like tumour. Munro. Medullary sarcoma. Abernethy. Cerebriform disease, or cancer. Laennec. Pulpy testicle. Baillie. Carcinus spongiosus. Good. Carcinoma spongiosum. Young. Fungoid disease. A. Cooper. Hodgkin. Medullary fungus. Maunoir. Chelius. Acute fungous tumour. C. Bell. Medullary cancer. Travers. Cephaloma. Hooper. Carswell. Carcinoma medullare. Müller. Soft cancer. Auct. Var.
				Scirrhus . .	Pancreatic sarcoma? Abernethy. Napiform. Chondroid. } Recamier. Lardaceous tissue. Auct. Gall. Carcinoma reticulare. Müller.	Carcinomatous sarcoma. Abernethy. Carcinoma scirrhosum. Young. Scirrhous cancer. Travers. Scirrhoma. Carswell. Carcinoma simplex vel fibrosum. Müller. Stone cancer. Auct. Var.
				Colloid . .	Pultaceous cancer. Pearly alveolar ditto. } Cruveilhier.	Areolar gelatiniform cancer. Cruveilhier. Carcinoma alveolare. Muller. Gum cancer. Hodgkin.

It will be seen by this table that carcinoma is distinguished as an adventitious heterologous tissue. The circumstance of being a tissue, susceptible of undergoing all the changes of increase and decay, is an important feature of carcinoma, and it is duly dwelt on by Dr. Walshe; but he remarks that Adams, Baron, Carmichael, and Cruveilhier go too far in their views regarding the independent vitality of carcinoma.

Before entering into an examination of the chapters on the Anatomy, Physiology, Pathology, and Therapeia of Carcinoma, we would have the reader accompany us to p. 647, where we find Dr. Walshe exhibiting tabularly the characteristics of the three species of carcinoma; thus

Encephaloid.	*Scirrhus.*	*Colloid.*
Resembles lobulated cerebral matter.	Resembles rind of bacon traversed by cellulo-fibrous septa.	Has the appearance of particles of jelly inlaid in a regular alveolar bed.
Is commonly opaque from its earliest formation.	Has a semitransparent glossiness.	The contained matter is strikingly transparent.
Is of dead white colour.	Has a clear whitish or bluish yellow tint.	Greenish yellow is its predominant hue.
Contains a multitude of minute vessels.	Is comparatively ill supplied with vessels.	(Its vessels have not been sufficiently examined as yet.)
Is less hard and dense than scirrhus.	Is exceedingly firm and dense.	The jelly-like *matter* is exceedingly soft; a colloid mass is, however, firm and resisting.
Is frequently found in the veins issuing from the diseased mass.	Has not been distinctly detected in this situation.	The pultaceous variety has been detected in the veins.
The predominant microscopical elements are globular, not always distinctly cellular, and caudate corpuscula.	The main microscopical constituents are juxtaposed nuclear cells; caudate corpuscula do not exist in it.	Is composed of cells in a state of *emboîtement*.
Occasionally attains an enormous bulk.	Rarely acquires larger dimensions than an orange.	Observes a mean in this respect.
Has been observed in almost every tissue of the body.	Its seat, as ascertained by observation, is somewhat more limited.	Has so far been seen in a limited number of parts only.
Very commonly coexists in several parts or organs of the same subject.	Is not unusually solitary.	Has rarely been met with in more than one organ.
Is remarkable for its occasional vast rapidity of growth.	Ordinarily grows slowly.	Grows with a medium degree of rapidity.
Is frequently the seat of interstitial hemorrhage and deposition of black or bistre-coloured matter.	Is comparatively rarely the seat of these changes.	
When softened into a pulp appears as a dead white or pink opaque matter of creamy consistence.	Resembles, when softened, a yellowish brown semitransparent gelatinous matter.	Undergoes no visible change of the kind.
Subcutaneous tumours are slow to contract adhesion with the skin.	Scirrhus thus situated usually becomes adherent.	
Ulcerated encephaloid is frequently the seat of hemorrhage, followed by rapid fungous development.	Scirrhous ulcers much less frequently give rise to hemorrhage, and fungous growths (provided they retain the scirrhous character) are now more slowly and less abundantly developed.	
The progress of the disease after ulceration is commonly very rapid.	There is not such a remarkable change in the rate of progress of the disease after ulceration has set in.	
Is the most common form [illegible] which secondary cancer	[illegible] much less common before	Has so far been obse

Great as the number and varieties of these distinctive characters of ephaloid, scirrhus, and colloid are, they are insufficient, Dr. Walshe arks, to counterbalance the weighty reasons afforded by the following s for uniting them into a genus, reasons which induce some eminent iologists to regard them as one and the same formation primarily: The different species are found coexisting in different organs in the e subject. 2. They are even met with in one and the same organ in e proximity. 3. After the ablation of a cancerous tumour, the oduced growth frequently belongs to a different species from the inal: thus, encephaloid follows scirrhus; scirrhus more rarely ence-oid (Müller); encephaloid appears in distant parts after the removal olloid. 4. In the hard state encephaloid and scirrhus are not to be nguished by their physical characters. 5. Structure possessing the earance of scirrhus may soften into true cerebriform pulp.

Ve now proceed to glance at the mode in which the anatomy, physi-gy, pathology, and therapeia of carcinoma are treated.

Under the head of Anatomy, Dr. Walshe considers the gross anato-al characters, the microscopical characters, the chemical characters, form, and the varieties, successively of encephaloid, scirrhus, and oid. All these points are very ably and fully discussed. A very d abridgment, illustrated by engravings, is given of the microscopical earches of Müller, to which we lately called the attention of our lers on the occasion of their coming before us in an English dress, by care of Dr. West. This part of Dr. Walshe's memoir, therefore, d not detain us, especially as our limited space will, we fear, be rcely sufficient for a very brief notice of the important practical ter with which the memoir abounds.

Colloid being the least generally known of the three species of carci-na, it may be interesting to the reader to have presented to him the owing account which Dr. Walshe gives of a specimen of the disease ecting the stomach recently examined by him; some of the charac-istics of colloid, in comparison with those of encephaloid and scirrhus, have already quoted; we would only further premise that the morbid duction in question was first distinguished, described, and named by ennec. A tolerably accurate description of it was also given in 1816 Otto; and an illustrative engraving in his "Beobachtungen" conveys, ugh in a rough manner, its characteristic features; the disease, which urred in the stomach, was however described by him as scirrhus of it organ. But to return to Dr. Walshe's case:

'The morbid matter occupied about one third of the posterior surface of the an, terminating at about half an inch from the pylorus, and formed an irre-ar globular elevation somewhat more than half an inch high (exclusive of coats of the stomach), two broad, and two and a half wide. Over by far the ater part of its surface, which was irregularly mammillated, and of a greenish low tint, the morbid matter was uncovered, and exhibited its characteristic olar and jelly-like aspect. But on the confines these characters were only n in some spots, where destruction of the investing mucous membrane had en place: this destruction seemed to consist in a simple wearing out from dual attenuation; there was no hardening or elevation of the borders of the ll perforations thus produced, nor had they the appearance of ordinary rs. Surrounding this mass, on its cardiac border, was an elevated flabby ation, more than half an inch high, and three lines thick in some places, the feel of a piece of lung condensed by simple pressure. On making a

section of this production, the colloid tumour and the coats of the stomach, the following were found to be their condition and mode of relation. The peritoneum was everywhere traceable, and in some parts considerably thickened the muscular tunic, of a pale yellow-green tint, varied from $\frac{1}{8}$th to $\frac{1}{4}$th of an inch in thickness; the major part of the colloid mass lay on the gastric surface of the cellulo-fibrous membrane, to which it adhered closely; in one or two points of its periphery the diseased structure penetrated, as it were, through this membrane and the muscular coat down to the peritoneum, along the inner surface of which it then spread for about half an inch. Where the mucous membrane existed entire over the colloid structure, it could be dissected from off the latter without injuring the cancerous cells; a fact showing that the morbid matter was here developed between the cellulo-fibrous and the mucous tunics. But the colloid alveoli also extended, gradually decreasing in closeness and number, into the elevated production already referred to: this seemed itself to be nothing more than a hypertrophous state of a fold of the mucous coat, accompanied with flabby induration; it had neither the structure nor the consistence of any form of indurated carcinoma, and its gastric surface was perfectly sound and apparently healthy. Hence it appears that the mucous membrane, previously rendered hypertrophous, may, as well as the cellulo-fibrous and mucous tunics, become the nidus of the diseased formation." (pp. 600-1.)

Under the head of Physiology are examined some of the phenomena influencing and attending the evolution of carcinoma; the circumstances of its origin, growth, and decay. The different species are, contrary to the plan adopted with their anatomy, considered together, "in order to avoid the repetitions which, on account of the similarity of their physiological processes, a separate examination of each would unavoidably entail."

Dr. Walshe subjects to a very searching criticism the principal views which have been broached in regard to the origin of carcinoma, by Pouteau, Abernethy, C. Wenzel, Broussais, Breschet, Ferrus, Andral, Hodgkin, Cruveilhier, Carswell, and he concludes as follows:

"From these investigations, however, an important fact derives: the microscopical elements of cancerous growths and their mode of propagation are identical with those not only of benignant tumours, but of the normal embryonic tissues. But we cannot agree with Müller in his inference, that this identity proves carcinoma not to be a heterologous formation. As it appears to us, such identity simply shows that the heterologous character, visible and palpable as it is, is produced not by the nature, but by the mode of combination and arrangement of the ultimate physical elements of the diseased growth. As well might it be contended that calomel and corrosive sublimate are not heterologous to each other, because they are both composed of atoms of chlorine and mercury." (p. 610-11.)

It may be well to remark, Dr. Walshe further says, that there is no real similarity between Müller's doctrine and the cystic theory of Dr. Hodgkin, as in the one case the phenomena are microscopical, and in the other putatively traceable with the unassisted eye.

Under the head of Growth, Dr. Walshe gives a short abstract of Schwann's theory regarding the development of the normal embryonic tissues, and Professor Müller's application of it to the explanation of the growth of carcinoma, and then makes the following comments:

"It will be observed that the origin of the cytoblasts is in this theory the unsolved problem; until this be decided, whatever light Müller's researches may have thrown on the mode of increase of these products, they have by no means cleared up the history of their origin. The only probable method of discovering the cause and mode of their primitive production, the physical change directly

preceding it, namely, a minute scrutiny of the state of the vessels leading to the molecular seat of the disease, has not been pursued by this observer." (p. 610.)

The following remarks on Melanosis are worthy of notice:

"Carcinoma occasionally presents a black tinge, either from abundant punctuation, infiltration, or deposition in masses of *melanic matter;* and an attempt has been made by some authors, led by this accidental impregnation into a belief that melanosis constitutes a separate malignant formation, to class it as a species of cancer. With the holders of this notion we cannot, for the following reasons, agree: 1. Pure melanic pigment, no matter in what abundance it be accumulated, produces no other effects than those dependent on the mechanical obstruction caused by the mass it constitutes. 2. When melanotic tumours produce the local and general symptoms of cancerous growths, they are found to be composed either of encephaloid or scirrhus (more especially the former), impregnated with black matter. 3. Neither the local nor general symptoms of carcinoma are modified in cases in which melanic matter is found to pervade it. 4. Melanic matter is incapable of forming a tissue, being an unorganized fluid into which vessels have never been observed to penetrate. It has been remarked, that when one cancerous growth is tinged with black matter, any others in the body are almost invariably found similarly discoloured: this fact, to which much importance has been attached, simply proves that there is a tendency in the system to the production of melanic matter. Again, Cruveilhier lays much stress on the circumstance that "melanic cancer" is rarely solitary; this is the necessary result of the fact, that black or bistre coloured impregnation much more frequently complicates encephaloid than the other species of carcinoma. The French pathologist has not thought of proving numerically that such multiplicity of growth is less frequent when the encephaloid is of normal than of abnormal colour." (p. 614.)

Under the head of pathology, the cause of the greater tendency of some organs than of others to cancerous deposition is enquired into, and the conclusion drawn is, that it is unknown. The anatomical course of the disease is next traced, then the condition of the solids and fluids of subjects dying with cancerous disease glanced at. The *etiology* is next very fully considered in all its bearings; and lastly, the duration, frequency, symptoms, diagnosis, and prognosis are each treated of in succession.

The disease may, in its material manifestation, be confined to a single organ from the outset to the fatal termination; or it originates in a single organ or tissue, whence it seems to spread, as from a centre, to a multitude of parts: the latter are said to be affected with *secondary* cancer. How does transmission of the disease to distant parts take place? This, answers Dr. W., seems only likely to be through the lympathic or venous system.

In regard to the doctrine of venous transmission, Dr. Walshe remarks that some apparently serious objections may be raised against it. He says, "If such, it may be urged, be the mechanism of the production of carcinoma in distant organs, why is such production not a constant phenomenon? why should cancerous disease ever terminate the existence of individuals without having manifested itself *materially* in more than a single spot? A satisfactory answer cannot, in the present state of science, be easily found. .... Another objection to this doctrine may appear in the multitude and large size of secondary formations; it is physically impossible, it may be alleged, that these should have been the produce of absorption from a mass, itself smaller than many among them, and which at no period of its growth suffered apparent diminution of bulk. But it is not meant in the remotest degree (and this is the answer to those who object to the origin of secondary carcinoma in absorption on the score of its consistence), that consecutive formations are composed of the actual

substance of the primary mass, though even this does not seem, from evidence of infinitely rare cases, to be impossible; the micrograph cancer shows that the translation and deposition of a few cells only the original nidus might lead to the development of the largest m each cell is in itself *the possible embryo of a tumour*. There is another fact, apparently subversive of the notion of venous transmiss the detritus and ichor of carcinoma have been injected into the vei animals, but no development of cancerous structure has resulted from experiment. Granted; but this simply proves—and it does so, perh as conclusively as any other adducible argument—the absolute ne sity of predisposition for the production of the disease; without this, eve material constituent manifests itself only as an irritative agent." (p. 6

After a further critical enquiry, distinguished by great acumen, the true mechanism of secondary cancerous development, Dr. Walshe s

"The reader has probably felt inclined to ask, where is the proof that, in cases of general infection adverted to, the growths which we have considere be developed consecutively to another or others, really possess such rela thereto? Is it not quite as likely that, in some cases at least, the presence heterologous growths in numerous organs may depend on their simultane development. Unquestionably such is the truth in a greater share of insta than the language of reporters of cases would lead us to suppose." (p. 622.

Etiology. This part is very ably treated of and illustrated by m statistical information. The two following tables we extract as illust tive of the influence which age exerts as a predisposing cause.

Table, showing the absolute mortality from Carcinoma in both sexes and at all ag

Age.	Males.	Females.	Both Sexes.
1 month			
2 months ...	...	1	1
3 and under 6	...	1	1
6 " 9			
9 " 12			
1 year ...	2	1	3
2 years ...	1	4	5
3 " ...	...	1	1
4 " ...	...	1	1
5 and under 10	3	2	5
10 " 15	1	4	5
15 " 20	3	5	8
20 " 25	4	2	6
25 " 30	1	13	14
30 " 35	6	23	29
35 " 40	15	43	58
40 " 45	19	77	96
45 " 50	23	98	121
50 " 55	34	130	164
55 " 60	35	120	155
60 " 65	44	110	154
65 " 70	45	88	133
70 " 75	35	69	104
75 " 80	30	49	79
80 " 85	16	28	44
85 " 90	1	8	9
90 " 95	2	1	3
95 and upwards ...	1	...	1
Totals.	321	879	1200

In regard to this table, Dr. Walshe says (and the remarks indicate the comprehensiveness and caution of the accomplished medical statist,)

"We learn the proportional number of individuals dying from carcinoma at each specified period of life; but were we to conclude that these numbers represent the relative tendency to the disease at different ages, we should fall into a very serious error. It appears, for example, that 250 females die of cancer between the ages of 50 and 60, while 118 perish between those of 70 and 80; but, if the total number of females living of the former age were double that of those alive of the latter, proportional mortality at both decennial periods of life would be as nearly as possible equal, instead of being in one of them double that of the other. In a word, in order to ascertain whether particular ages exercise any influence on the development of the disease, we must compare the absolute mortality with the total number living at each age. This we have been enabled to do from the population estimates of Mr. Rickman, and present the reader with the result in the following

Table, showing the proportion of Deaths from Cancer in 1,000 *living of each sex at the different Ages.*

Ages.	Males.	Females.	Mean.
Under 5	·006	·017	·012
5 and under 10	·007	·004	·006
10 „ 15	·002	·010	·006
15 „ 20	·009	·017	·013
20 „ 30	·010	·024	·017
30 „ 40	·058	·152	·105
40 „ 50	·140	·983	·561
50 „ 60	·290	1·066	·678
60 „ 70	·636	1·192	·919
70 „ 80	·935	1·421	1·178
80 „ 100	1·207	·973	1·089
All ages	·103	·245	·174

"A glance at this table," Dr. Walshe remarks, "will suffice to show the inaccuracy of the commonly-received opinion, that the tendency to cancer reaches its maximum between the 35th and 50th years. The mortality, on the contrary, goes on steadily increasing with each succeeding decade until the 80th year; after that period the results are, from causes which it is needless to enumerate, liable to slight error, and may therefore be omitted in the general survey." (p. 625.)

In farther illustration of this interesting subject, a diagram is given, reduced from the preceding table, *showing the relative mortality from cancer at different ages in both sexes*; for which, and many other discussions of surpassing interest, we must refer the reader to the original.

Therapeia. The cure of cancer! the philosopher's stone! the catholicon! Such things Dr. Walshe has carefully shut the doors of his imagination against, and encounters the subject with soberness and truth.

"The following propositions," says he, "appear to embrace such facts as are established respecting the curability of carcinoma, by art. 1. There is no existing evidence to show that carcinoma of an internal organ has ever been cured. 2. Cancer has never been removed by medicinal agents alone. 3. Morbid productions possessing the anatomical and pathological character of scirrhus have been removed with the knife, and no return of the disease been observed for a number of years, and occasionally during the entire course of a life, prolonged beyond the expectation of life in healthy individuals at the period of removal. 4. Although it cannot be doubted that many of the tumours referred to by M. Recamier were not really composed of that tissue, yet it seems

impossible to affirm this of all of them: it would follow then that carcinoma has in rare instances been cured by compression, aided by other external and internal remedies. 5. The probabilities of effecting a cure vary with the species of cancer. There are few authentic instances on record of permanent recovery after the ablation ef encephaloid; successful removal of scirrhus is of more frequent occurrence, and the tendency of colloid to reproduction, although Velpeau states it to be extreme, is probably lower than of the other varieties. 6. The earlier the morbid mass is removed, the stronger are the chances of ultimate recovery: Scarpa's three successful operations were performed within the first three months; of twenty-seven scirrhi removed by Flajani within the first month of their formation, two only returned. 7. Encysted cancer, provided the cyst be removed, is less liable to return than non-encysted. 8. The chances of success diminish out of all proportion with each repetition of the operation. 9. It has been stated that the removal of cancer in some parts is more prone to be attended with relapse than in others: mammary cancer is instanced as an example of the former, osseous of the latter. We are disposed to acquiesce in this notion, but it requires to be proved numerically; and the fact that under the term osteo-sarcoma have been included growths in bone of almost every description, fibrous, cartilaginous, &c., as well as the truly cancerous, renders it exceedingly probable, if not certain, that the supposed immunity of bone in this respect has been exaggerated. 10. It is indubitable that local, and probably distant, reproduction frequently depends on some of the diseased growth being left behind. .... The inference is that, were ablation perfect, recoveries would be more common." (p. 640.)

"Finally," says Dr. Walshe, in the penultimate paragraph of the first part of his memoir, "the following proposition directly flows from numerous facts stated in the preceding pages: a cancerous tumour, under all circumstances, even should it remain single and stationary for years, is but the local evidence of a general vitiation of the system. But, it may be enquired, is not this a contradiction? how is it possible, that if the organism at large be in a state of disease, a single spot shall alone manifest its presence? We confess our inability to explain away this difficulty; but neither the fact of its existence, nor its inexplicability shake the solidity of the opinion advocated. All pathologists who have carefully investigated the history of the tuberculous affection admit that local agencies, of whatever kind they may be, can never cause the production of a particle of tubercle, that constitutional derangement must lead the way. Now here we have precisely the same difficulty to contend with; a knot of tubercles may exist for years in the summit of a lung, and every other organ remain free to the last from the disease. Why this is so, we know not: these are the mysteries of pathology, and they are mysteries which it would, in the existing state of the science, be idle to attempt to penetrate." (p. 650.)

Our limited space forbids us accompanying Dr. Walshe in the second part of his memoir, of which indeed any abstract of ours would give but a very insufficient idea. We can recommend it to the practitioner as a most valuable source of reference. It is a cyclopædia of the accumulated experience of practitioners on cancer in various parts of the body. Were we to direct attention to any one part of the subject in particular, it would be to the article on cancer of the meninges.

It will be seen that the preceding notice of Dr. Walshe's memoir can scarcely be looked upon either as an analytical or critical review, because to have attempted making it so would have been supererogatory, as the memoir itself is not only a dissertation on the nature and cure of cancer, but a masterly analytical and critical review of the whole bearings of the subject. We have therefore principally confined ourselves to the quotation of such passages as appeared to us likely to interest our readers and to give them an idea of the execution of the whole original, the careful study of which we most cordially recommend.

Art. IX.

Schräg Verengte Becken nebst einem Anhange über die wicht-
n Fehler des weiblichen Beckens überhaupt. Von Dr. F. C.
gele, &c. &c.—*Mainz*, 1839. 4to, *mit sechzehn Tafeln.*
bliquely-contracted Pelvis, with an Appendix treating of the
cipal Defects of the Female Pelvis. By Dr. F. C. Naegele,
&c.—*Mayence*, 1839. 4to, pp. 124, 16 Plates.

a singular fact that the influence of the pelvis upon labour was the last points to which obstetricians directed their attention. ght justly have thought that so frequent and obvious a cause of t labour as deformity of the pelvis could not have escaped notice, fore the middle of the past century we find scarcely any mention subject.

ling Ould in 1742, and Smellie in 1751, were the first to investi- e mechanism of labour, which was soon afterwards still further ted by the valuable dissertation of Solayrés de Renhac, "De Viribus Maternis absoluto. Parisiis, 1771." The opinions there ed were rendered more generally known by Baudelocque, who ted into French the treatise of his late friend and master, and ed it in his own work, "De l'Art des Accouchemens." The dis- likewise which arose about the same time with reference to the on of synchondrotomy led to a juster estimate of the part sus- by the pelvis in natural as well as in difficult labour. Many erro- notions, however, with regard to the passage of the child through lvis still prevailed, and were such as gave rise to serious practical rs. A mischievously meddling practice, to which Osiander gave the n of his high authority, resulted from a belief that many positions child's head were unnatural, and that in such cases delivery could effected except by manual or instrumental interference. The of Professor Naegele have mainly contributed to remove these en ideas. He has too not merely improved and simplified the e of midwifery in his own country, but his valuable works, and lly his essay on "The Mechanism of Parturition," have obtained an European reputation.

book now before us is worthy of the high reputation of its author. s he informs us, the result of many years of careful observation, ough he has strictly followed the direction *nonum prematur in* , he would not even now have published it, but for some circum- related in the preface. He appears to have ransacked the mu- of Europe in search of whatever could elucidate the subject, and scarcely suppress a smile at finding a description of the pelvis of ptian mummy adduced in support of his opinions.

first part of the book, to which we shall entirely confine our notice, f "The obliquely-contracted pelvis, that is to say, a pelvis con- in the direction of one of its oblique diameters with complete sis of the sacro-iliac symphysis of one side, and imperfect for of the sacrum and os innominatum of that side."

y-four years ago, M. Naegele met with the first two specimens of formation of the pelvis. He was struck with their peculiar cha- the number of points distinguishing them from pelves deformed

by rickets or malacosteon, and with the fact that the persons from whoi they were taken had both died during their first labour. In 1813 h found another pelvis closely resembling the two former in every point bı one, and in 1825 he saw mention of one similarly malformed in the wor of Madame Lachapelle. In 1828 he accidentally discovered another pelv so like the former ones, that it might have been confounded with them and he now felt convinced that this peculiar deformity must be the re sult of certain fixed laws, and could not be a merely accidental pro duction. After a time he succeeded in gaining intelligence of oth similar pelves, and was thus enabled on Nov. 24, 1832, at the meetir of the *Gesellschaft fur Naturwissenschaft und Heilkunde*, to descri this as a new and peculiar variety of pelvis. A further account was giv of it by the author at the great *Naturforscher Versammlung* at Stuttg in September, 1834; and in the Heidelb. Klin. Annalen, (Bd. heft 3, s. 449.)*

The peculiar characteristics of this form of pelvis are as follows:

"1. Complete anchylosis of one sacro-iliac symphysis, or perfect contin of substance of the sacrum with one os innominatum.

"2. Atrophy or imperfect development of one lateral half of the sacrum and less width of the foramina sacralia anteriora of that side on which the an chylosis is situated.

"3. Less breadth of the os innominatum, and less width of the incisura ischiatica of the same side. That is to say, the distance of the anterior-superior spine of the ilium from the posterior superior spine, as also a line drawn along the brim of the pelvis from the spot where the sacro-iliac symphysis may be supposed to be, along the linea innominata and the pectinea ossis pubis, is shorter than on the corresponding bone of the opposite side. Further, that portion of the hinder part of the inner surface of the os innominatum of the anchylosed side, *superficies auricularis*, by which this bone is connected with the sacrum, is less high or does not reach so far down as on the other side, or as in the well-formed os innominatum.

"4. The sacrum appears driven towards the anchylosed side, towards which also its anterior surface appears more or less turned, while the symphysis pubis is forced towards the opposite side, and consequently is not directly but obliquely opposed to the promontory.

"5. The inner surface of the lateral wall, and the lateral half of the anterior wall of the anchylosed half of the pelvis, are flatter or less excavated than in a normal pelvis.

"6. The other half of the pelvis, namely, that on which the sacro-iliac synchondrosis exists, likewise differs from the normal condition. The side of the pelvis free from anchylosis does not merely share with the opposite side in the abnormal position or direction of the bones; but also in their unnatural form, for a line corresponding to the linea ileo-pectinea will be less curved in its posterior half, more in its anterior half, than natural."

* An abstract of this paper was given by Dr. Rigby in the seventh volume of th London Medical and Surgical Journal. The late Dr. Hamilton, in his Practical Obser vations, gives a drawing intended to represent a pelvis of this description, which he saw in the collection of Messrs. Pravaz and Guerin, and Dr. Ramsbotham, in his elegant Atla copies an indifferent representation of a similar pelvis from the work of M. Moreau Neither he nor Dr. Hamilton, however, seem to have noticed Naegele's paper as reporte in the London Journal, nor to be acquainted with Madame Lachapelle's description of pelvis of this kind in her Pratique des Accouchemens, tom. iii. p. 509.

The consequences which result from what has been already stated, are

"7. *a.* That the pelvis is contracted obliquely, or in a direction crossing a line drawn from the anchylosis to the opposite side, while in the opposite diameter it is not contracted, but in an extreme degree of obliquity is even longer than natural. This may be illustrated by comparing the form of the brim of the pelvis, to an oval figure placed obliquely; the transverse or small diameter of which would correspond to the contracted oblique diameter, and its long diameter to the opposite oblique diameter of the pelvis."

"*b.* That the distance between the promontory and the region above each acetabulum—distantia sacro-cotyloidea—as also the distance from the apex of the sacrum to the spinous process of either ischium, is less on the side where the anchylosis exists.

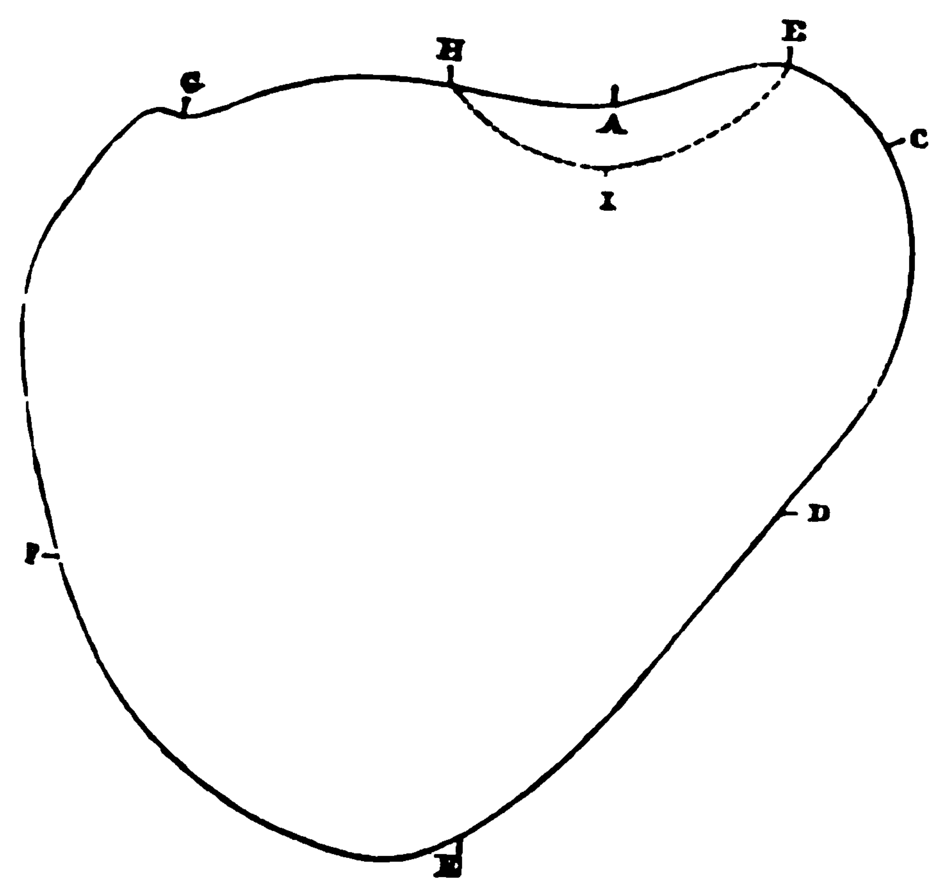
"*c.* That the distance between the tuber ischii of the anchylosed side and the spina posterior superior of the opposite ilium, as also the distance between the spinous process of the last lumbar vertebra and the anterior superior spine of the ilium of the anchylosed side, is less than the same measurements on the opposite side.

"*d.* That the distance of the under edge of the symphysis pubis, from the spina posterior superior of the ilium of the anchylosed side, is greater than the distance from the symphysis pubis to the same point on the opposite side.

"*e.* That the walls of the pelvic cavity converge somewhat at their lower part, and the arch of the pubes is more or less contracted, consequently approaches the form presented by a male pelvis.

"*f.* That the acetabulum of the flattened side is directed more forward than natural, while that of the opposite side is inclined almost directly outwards." (pp. 7-11.)"

A glance at the accompanying diagram will best explain the nature of the deformity.



The dotted line H I B marks the projection of the promontory.

I, The middle of the promontory.

E, The situation of the symphysis pubis.

G, The situation of the right sacro-iliac symphysis.

C, The point corresponding to where the left sacro-iliac symphysis ought to be.

D F, The situation of the acetabula.

The letters A B C D E F G H mark the circumference of a plane bounded by a line drawn along the upper edge of the symphysis pubis and the linea ilio-pectinea, and continued along the anterior surface of the sacrum.

Professor Naegele observes that all the pelves affected by this deformity bear the most exact resemblance to each other, and that the bones present no mark of rickets, malacosteon, or any other disease, but appear in every respect perfectly healthy. He suggests, as the most appropriate name, the term "*pelvis oblique ovata.*"

In § III. he proceeds to give a description of all pelves thus deformed, of which he has been able to procure any account: they are thirty-seven in number—two male and thirty-five female; and the history of all (where any history could be obtained) was, with one exception, the same, namely, that the persons were primiparæ, who died undelivered or in consequence of the severity of their labour. The fourteenth case forms the exception; the woman having given birth to six living children with no extraordinary difficulty, and having died at length from the effects of a cold caught four days after her last delivery.

Of all the cases, one only (No. 3) came under the notice of Professor Naegele during the person's lifetime. The particulars of her history are briefly these: A healthy girl, aged nineteen, was admitted into the hospital at Heidelberg at the end of her first pregnancy. She appeared to be well formed, but was thought to halt slightly in her walk. Measurement with Baudelocque's *compas d'épaisseur* gave an external conjugata of seven inches, and the promontory could not be reached even when two fingers were introduced into the vagina. Attention was drawn to the case by the circumstance that the head of the child, instead of being low down, as it usually is in first pregnancies, was situated very high up, and was exceedingly moveable. The liquor amnii escaped two days before any pains were felt, and it was not until the third day after labour had commenced that the head was sufficiently low down to be reached by the forceps. Great difficulty was experienced in extracting the child, which was dead and putrid. The placenta was retained by spasmodic contraction, and the occurrence of hemorrhage rendered its removal by the hand necessary. Five days afterwards the patient died of puerperal fever, and a post-mortem examination discovered the marks of inflammation of the uterine appendages. The substance of the uterus was healthy, and the only point it presented worthy of notice was that its right angle was situated higher than the left. There was no trace of the left sacro-iliac symphysis; the left distantia sacro-cotyloidea measured only one inch ten lines, while that on the right side was three inches six lines in length; and a line drawn from the centre of the promontory forwards intersected the left os pubis, just where its transverse and descending rami unite.

Other pelves are next described; which, though in most respects closely resembling those that form the subject of the previous section, are yet not exactly similar. They are of two kinds, either presenting deformity in shape without anchylosis of the sacro-iliac synchondrosis, or anchylosis of that joint without unsymmetrical form. Three specimens of the former and one of the latter are described by Professor Naegele, who suggests that they probably form the connecting link between the natural pelvis and that species of deformity of which his work treats.

In § VI. the origin of this form of pelvis is discussed; and M. Naegele still inclines to the theory which he adopted at the first, namely, that it is owing to congenital malformation. He assigns the following reasons for this opinion:

"1. The intimate and perfect blending of the sacrum with the ilium, as well

i internal structure as in outward appearance; so that, in many instances, he situation of the synchondrosis presents no sign of any previous separation.

"2. The incomplete development of one lateral half of the sacrum in its whole ngth, as well as the deficient breadth of the os innominatum on the same side, d especially the circumstance that that part where the blending of the sacrum d the os innominatum occurs is shorter longitudinally than the synchondrosis the opposite side, or in a well-formed pelvis.

"3. Synostoses and deformities are known to occur as original malformations other bones; and congenital synostosis is usually associated with deformity the bones, the result of arrest of development.

"4. The striking similarity between the different specimens of this deformity eaks for an identity in their cause.

"5. In no instance can it be proved that any morbid condition of the bones isted, or that any external injury had been inflicted on them, such as might ve rise to the deformity." (pp. 65-6.)

Although we cannot admit the force of all the reasons assigned by rofessor Naegele, yet we are fully disposed to allow the possibility of is form of pelvis being the result of original malformation. It is well nown that instances of want of symmetry in organs naturally symme-ical are by no means unfrequent.* Specimens of oblique skulls are to e found in almost every museum, and we have seen in the anatomical ollection at Bonn many specimens of oblique skulls and oblique pelves om the same individual. The existence of obliteration of the sutures nd consequent synostosis of the bones of the skull is likewise by no eans unusual;† and a case described by Dr. Tourtual, of Münster,‡ nd alluded to by Professor Naegele, exhibits a combination of synos-osis of all the bones of the cranium with great want of symmetry in its orm. The os hyoides is sometimes found unsymmetrical, and one half f the thyroid cartilage is occasionally seen to be larger than the other. Obliquity of the uterus, and even hernia of the ovary, from congenital hortness of one round ligament, sometimes occurs, and the two lateral halves of the uterus may differ much in size, while the orifice of the organ s situated far out of the mesial line of the cervix.§ It is also worthy of note, that in case No. 3, in which there seems to be the best evidence for the shape of the pelvis being congenital, the uterus was higher on the right than on the left side.

Original malformation, however, is not regarded by Professor Naegele merely as being one of several possible causes of obliquity of the pelvis, but he considers that the deformity is in all cases congenital. It is with much hesitation that we venture to dissent from so high an authority, but the evidence which he has adduced in support of his theory does not appear to us conclusive.

We apprehend that few persons would feel disposed to regard an anchylosis as congenital merely on account of the intimate blending of two bones, and partial absorption of bone is a very frequent attendant upon the process of anchylosis. An excellent illustration of this is pre-

* Meblis, De Morbis Hominis dextri et sinistri; 4to, Gottingæ, 1817. Delpech, De l'Orthomorphie; 8vo, Paris, 1828, tom. i. art. v. And Weber, in V. Gräfe und V. Walther's Journal, Bd. 4, Heft 4, s. 694.

† Van Döveren, Observat. Osteolog. in his Specimen Observat. Acad.; 4to, 1765, p. 194. Sandifort, Observat. Path., lib. i. cap. vii., and lib. ii. cap. vi.

‡ Med. Zeitung der Verein für Heilkunde, 1836, s. 175.

§ Deneux, Récherches sur la Hernie de l'Ovaire; 8vo, Paris, 1813. Fr. Tiedemann, Von der schiefen Bildung und schiefen Lage der Gebärmutter; 4to, 1840, Taf. ii. iii. iv.

sented by a preparation of anchylosis of the head of the femur into the acetabulum in the museum of University College; and similar specimens may be found in almost every anatomical collection. In the museum of Guy's Hospital are several preparations of anchylosis of the sacrum with the os innominatum, in some of which scarcely a trace remains of the existence of the symphysis. It should also be borne in mind, that perhaps no joint in the body is so liable to become anchylosed as the sacro-iliac synchondrosis.*

His second argument is much more weighty; but still, since it is a notorious fact that a bone may become atrophied from a variety of causes in the adult,† we think that other proofs than the mere assertion that it is always congenital in this case, ought to be brought forward by Professor Naegele.

The work of Madame Lachapelle‡ affords instances of great contraction of one half of the pelvis from causes operating long after birth. In a case of unreduced luxation of the femur, in which the bone formed for itself a new acetabulum, contraction of the cotyloid cavity was found to have taken place, with shortening of the ischium, and contraction of the corresponding half of the brim of the pelvis. In another instance amputation of the left thigh was followed by shrinking of the right side of the brim of the pelvis to half its natural size, while the whole pelvis was rendered oblique, and the pubis was forced towards the left side. A similar case is mentioned by Herbiniaux.

The last reason assigned by the author, namely, that no morbid condition of the bones existed, and that no injury had been inflicted, appears conclusive until we examine the grounds upon which he rests his statement. It will then be seen, that of thirty-one of the thirty-seven cases described, the history is altogether unknown. Of the remaining six, in all of which it is said that the persons were well formed, one only came under M. Naegele's own observation during the lifetime of the individual. Of these six, two were noticed to have been unusually backward in learning to walk; and two, including one of those just mentioned, presented curvature of the lumbar vertebræ. In two, the state of the lumbar vertebræ is not mentioned, and in two only they were stated to have been found perfectly straight on examination of the pelvis. In twenty-five of the remaining thirty-one, of which no history is known, no part of the spine appears to have been preserved with the pelvis. In four, there was a lateral curvature of the lumbar vertebræ, in one instance they were straight, and in one it is not stated whether they were straight or crooked. Three of the pelves presented marks of hip-joint disease, or of ulceration about the acetabulum.

* Créve, Von den Krankheiten des weiblichen Beckens, 4to, Berlin, 1795, s. 165. In his own museum, and in those of the Wenzels, of Weidmann, and of Sömmering, Créve met with 70 specimens of anchylosis of both ossa innominata with the sacrum, and 250 of anchylosis on one side. Of these latter 101, were from the male, 149 from the female subject: 171 were anchylosed on the right side, 70 on the left. He likewise states that in other collections he saw quite as many specimens as those above enumerated, and feels himself therefore warranted in stating "that this joint is more liable to anchylosis than any other in the pelvis, or indeed in the whole body."

† J. Shaw, on Distortions of the Spine and Chest; London, 1823, pp. 6-8.

‡ Traité des Accouchemens; Paris, 1825; tom. iii. p. 413.

The assertion, then, that no morbid condition of the bones existed can be understood as referring only to the absence of rachitis or malacosteon. But we are left almost completely in the dark as to the state of the spinal column, except in the few instances where the plates assist us by representing two or three lumbar vertebræ in connexion with the pelvis. It is, however, of great importance to ascertain the condition of the spine, since in a great number of cases of common lateral curvature the pelvis is contracted obliquely and presents almost exactly the appearance of the *pelvis oblique ovata*, except that the sacro-iliac synchondrosis is not anchylosed.* It will be remembered, too, that curvature of the lumbar vertebræ existed in most instances where their condition was at all noticed. In three of the cases there were the marks of previous hip-joint disease; and Dr. Betschler, who thinks, as we do, that Dr. Naegele's arguments are not conclusive, mentions in his review of this work in Busch's Journal, a case where disease of the bones deformed the pelvis exactly in the way which Professor Naegele regards as congenital. This is not the place to enter upon a lengthened discussion of the subject; but we hope that in all future cases of oblique pelvis, the condition of the spinal column will be more carefully examined than it has been hitherto. At present it appears to us that proof has not been brought forward sufficient to show that this deformity is in all cases congenital, and that it is never produced by causes acting after birth.

In the following section the author examines the influence of this form of pelvis upon labour. It operates in two ways: first, by directly retarding the passage of the head, and next, by impeding those rotations which the head makes in its passage through a well-formed pelvis. Of course, the original size of the pelvis will greatly modify the degree of difficulty thus produced. The practitioner should also recollect that it is not the contracted brim of the pelvis which is the sole impediment to the passage of the head, but that the convergence of its walls from above downwards, increases the opposition to the head in proportion as it descends. If, in a case of this sort, it were thought necessary to apply the forceps, the peculiar form of the pelvis should be borne in mind; or, if the child were turned, attention should be paid in extracting the head to bring its long diameter into correspondence with the longest oblique diameter of the pelvis.

In § VIII. Professor Naegele makes some very just observations upon the difficulty of discovering this form of pelvis in the living subject, and on the insufficiency of the ordinary modes of pelvimetry for the purpose. In all those cases the history of which is known, the existence of the deformity was not suspected before labour began. Not only is the measurement with Baudelocque's *compas d'epaisseur* quite unsuited to detect the malformation, but even vaginal exploration, which Madame Lachapelle regarded as a never-failing test of a natural pelvis, would totally mislead. Inability to reach the sacrum with the finger

* Proof of this is afforded by the measurements appended to Choulant's Decas Pelvium Spinarumque Deformatarum; Lipsiæ, 1818. Decas Secunda; 1820, 4to. Jörg also gives a good illustration of this fact in his work, Ueber die Verkrümnungen des menschlichen Körpers; Leipsig, 1840, 4to, Pl. i, fig. 1. The same may be observed on a careful examination of anatomical collections; and a spine and pelvis in the museum of Guy's hospital, afford a striking confirmation of our statement.

instead of affording a proof that the pelvis was well formed, would, i this instance, rather indicate a high degree of deformity.

The old methods of pelvimetry having been seen to be useles M. Naegele made many experimental measurements, and now propose to ascertain,

"1. The distance from the tuber ischii of the one side to the spina poste rior superior ossis ilii of the opposite side.

"2. From the spina anterior superior of one ilium, to the spina posteri superior of the opposite side.

"3. From the processus spinosus of the last lumbar vertebra, to the spi anterior superior of each ilium.

"4. From the trochanter major of one side to the spina posterior super ossis ilii of the opposite side.

"5. From the middle of the under edge of the symphysis pubis to spina posterior superior of each ilium." (p. 74.)

The author observes that all the above-named points are easy of acce and may be readily recognized, and that the employment of all complica instruments to ascertain the distance between them is quite unnecess since a common *compas d'épaisseur* will answer every purpose. chief recommendation of all is, that these dimensions are nearly exactly the same on both sides in symmetrical pelves, but differ in oblique pelvis quite sufficiently to enable us to ascertain the existence deformity. In proof of this he gives tables of the measurements eight oblique pelves in his own museum.

Our remarks have extended to such a length that we must defer another time any examination of the appendix which occupies the la thirty pages of the book. The reader will find there much valuabl information upon deformities of the pelvis, which he would search for in vain elsewhere.

The plates which illustrate the work are some of the most beautiful specimens of the lithographic art we have ever seen.

Art. X.

1. *A Practical Treatise on the Diseases peculiar to Women; illustrated by Cases derived from Hospital and Private Practice.* By Samuel Ashwell, m.d., Obstetric Physician, and Lecturer to Guy's Hospital. Part I. *Functional Diseases.—London,* 1840. 8vo, pp. 208.

2. *A Practical Treatise on the Function and Diseases of the Unimpregnated Womb. Illustrated by Plates, &c.; with a Chapter on Leucorrhœa, Fluor Albus, or "Weakness."* By Charles Waller, m.d., Lecturer on Midwifery at the Medical School, Aldersgate Street, &c. &c.—*London,* 1840. 8vo, pp. 200.

It is quite true, as Dr. Ashwell observes in his preface, that althougl we possess many valuable single essays on female diseases, there i scarcely one complete and practical work. His aim has been to pro duce a treatise on this subject, "so true, simple, and practical, that i may form a safe and efficient guide to the elucidation and curative treatment of many, at least, of these intricate, rapidly-progressing, and dangerous maladies." He devotes his work strictly to pathology and

treatment, not to anatomical detail and physiological research, and it includes little which is not the product of his own practice and observation. Many formulæ of remedies are appended to the various chapters, and this has been done because it harmonized with the practical plan he has adopted.—Dr. Waller's intention is less extensive than Dr. Ashwell's. He is anxious to present to the profession a condensed account of the most common diseases to which the uterine system is obnoxious.—Passing over the brief introductory remarks with which both the volumes begin, we shall briefly abstract and comment upon the views of the writers on the different diseases they treat of; and first of

Chlorosis. Dr. Ashwell's definition of this common and obstinate disease of females may be objected to on account of its length, but its general accuracy will, we think, be admitted. He defines chlorosis to be a peculiar affection of the general health, in which debility, languor, and deranged stomachic functions are prominent symptoms, most frequently occurring when puberty is or ought to be established, although it may exist at any subsequent period, always characterized by anæmia of the system, and a yellowish dirty green pallor of the surface: when a disease of early youth, almost invariably connected either with entire absence of menstruation, or with a scanty, painful, and irregular performance of the function; and if a disease of later life, in addition to these causes it may have been preceded and produced by menorrhagia or leucorrhœa. Chlorosis, Dr. Ashwell says, presents itself in three forms: as a mild and incipient, an inveterate and confirmed, and a complicated disease.* Now this, like all, or at least most nosological arrangements is quite an arbitrary one, simply because nature does not choose either in health or disease to be limited and confined by our abstract rules, but still we agree that these forms which are laid down by Dr. Ashwell are sufficiently comprehensive and correct to embrace the great majority of cases of chlorosis which we meet with in practice.

The incipient form of chlorosis commences almost unobserved: the patient has most likely been "delicate" from infancy, but at the age of twelve, thirteen, or fourteen years without any obviously exciting or morbid cause, other than a negative one, the non-establishment of puberty, a series of distressing and perplexing symptoms are ushered in. The general health declines in connexion either with the entire or partial absence of menstruation. The girl does not pass on to womanhood. She is languid, soon fatigued, dull, and listless, sometimes perverse and sullen, and prone to solitude. The appetite is capricious. The complexion is pale; bowels constipated; tongue of a dirty pasty white; the breath is offensive; flatulence; frequent and severe headachs; palpitation of the heart and pain in the side; quick, weak, and compressible pulse are common symptoms. The menses, if not absolutely retarded, have scarcely appeared, the discharge having been pale in colour, and scanty in quantity. These symptoms, (with, we would add, many variations in different cases that long observation alone can teach,) in an aggravated degree, and some new ones, attend inveterate and confirmed chlorosis. Complicated chlorosis consists in the presence, in the greater or less degree, of the general symptoms of the disease, with a more prominent morbid affec-

* Dr. Copland adopts the same distinctions of chlorotic cases. Dict. of Pract. Med. Part i. p. 316.—Rev.

tion of some distant organ. The predisposing and exciting causes the diseases are, in brief, such as tend to enfeeble the mental and bod powers, and to interfere more or less with the functions of the ovaria a uterus, especially about the age of puberty.

In common with most writers of the day, Dr. Ashwell thinks it m fairly be assumed that chlorosis primarily depends on a morbid conditi of the blood, which secondarily affects the ovaries and uterus by retardi their growth. This opinion is supported by the fact, that in the blood chlorotic patients there is an increased proportion of the serum, witl marked diminution of the crassamentum. Gooch and Andral distinc allude to the deteriorated quality of the blood. The former says* "if girls who have attained the age at which this change is customary, t sexual organs are not developed, a deranged state of the constituti occurs, characterized by peculiar symptoms, &c. In addition, the qual of the circulating fluid is in chlorosis altered; blood has been taken way of experiment, and it has been found to be of a pale red colour a watery like the juice of a cherry." Andral again confirms these view: "If chlorosis, as is now generally admitted, frequently results from defective formation of the blood, the cause of which may reside exc sively in the nervous system, can one with any show of reason re either to irritation or sanguinary congestion, the Proteus-like variety functional derangements which chlorotic patients so constantly prese such as epileptic paroxysms, convulsions, chorea, dyspnea, palpitatio vomiting &c.? Or shall we not approach nearer the truth, in assigni these different morbid phenomena to the same cause which produ them in persons who are reduced to a state of anæmia by the deprivati of food, light, and wholesome atmosphere?.... Venesections, employed such cases to combat an irritation which in reality does not exist, in riably produce a marked aggravation of all the symptoms: on the co trary, it frequently happens that by stimulating the nervous system these chlorotic patients by the physical and moral emotions of matrimo we produce a more natural complexion and colour of the whole cutane surface, thus indicating a corresponding improvement in the process sanguification, and in proportion as the anæmia disappears under t influence of this new modification of the nervous system, the whole tr of diseased action, the difficult respiration, constant sensation of u easiness and listlessness, impaired digestion, gastralgia, vomiting, tym nitis, and limpid urine, together with all the strange nervous symptor which seemed dependent on some organic alterations of the soli gradually subside and eventually vanish, as a fresh supply of blood generated in the system."

It will be allowed, after what has been advanced, that chlorosis is n an inflammatory disease. But still—well as this fact is known, and g nerally as it is admitted—there are certain symptoms which are very co mon to the disease, as severe headach, pain in the side affecting t breathing, distressing action of the heart, &c., which are often mistak for and treated as acute inflammatory affections. And we, too, ha often "witnessed the very injurious consequences of such mistakes, t practitioner having forgotten, what in female diseases it is peculiarly i

* Gooch's Lectures, by Skinner, p. 7.

† Pathological Anatomy, by Townsend and West, vol. i. p. 106.

remember, that the severity of the pain and the rapidity of the
enerally indications of irritability and excitement, not of in-
; demanding narcotics, carminatives, and, at the most, counter-
not bleeding, active purgatives, or spare diet." Dr. Ashwell
uch upon one source of mischievous practice, which we are
ry common one, especially on the part of young practitioners.
friends, and the chlorotic patient herself, almost invariably
e symptoms just enumerated to inflammation. They desire to
d they are bled, more from an easy acquiescence with their
rom a mistaken conviction of the propriety of the practice.
l is of opinion that "there will be no difficulty in distinguish-
is from chronic affections of the abdominal viscera, especially
c, splenic, and renal disease, or indeed from any morbid state,
aemia and pallor are prominent symptoms. The period of
culiar aspect of a chlorotic patient,* and the derangement of
on, will ensure a correct diagnosis." We should have worded
aph more cautiously, because we believe that there is often
ulty, at least, in ensuring a correct diagnosis.
plications of chlorosis, that are briefly described by Dr.
e discharges of blood from the stomach by vomiting; chro-
ement of digestion and nutrition; functional affection of the
nd vascular system; ascites and structural change of the lung.
atter complication especially, Dr. Ashwell's remarks are good
cal; they convey a useful lesson to the inexperienced, by show-
what circumstances there is or is not cause for apprehension as
of the disease. Dr. Waller's account of chlorosis is very short
nsatisfactory.
tment of chlorosis, to be frequently successful, must be adopted
most sedulously prosecuted. In the mild form of the disease
es must be principally of a constitutional kind; directed to
ement of the general health, and to the establishment of pu-
when these points are gained, the uterine functions are not
the delay may probably be attributed to the torpor of the
reproduction, and emmenagogues may be employed. In the
plications of chlorosis, it often happens that the affection of
principally implicated becomes so prominent, as almost to
the shade the original chlorosis and its accompanying amenor-
is is by no means uncommon; but the source of the disease
s be borne in mind. In such cases the treatment will require
n; it will necessarily be less constitutional, and assume more
l character. "And lastly, where structural alteration of the
atened or suspected, the management must have especial and
lusive reference to this alarming complication, every measure
ted to avert this greatest of all dangers. Still, even here, it
e forgotten, that if puberty could be sufficiently developed to
en the partial establishment of menstruation, a very formidable
the complication would disappear." (Ashwell, p. 22.) We
ve, however, that the probability of the ultimate success of
ent in such cases will very greatly depend upon the *early* ap-

* depicted to the life in plate iv. of Dr. Marshall Hall's "Commentaries on of Females."—Rev.

plication of it. If, as too frequently happens, from negligence on the part of the girl's friends, or of the practitioner, the case is overlooked for some months, the danger will, at least, be very much increased. The treatment of the most common form of chlorosis, namely, that accompanying puberty, may be regarded as the type of the treatment of all the others, embodying the principles which, with greater or less modification, are universally applicable. "It is here, at the very threshold of the disease, when its character is not understood, or when it is treated empirically, that the greatest error is committed. It is viewed as a local, not as a constitutional affection, and many are the individuals who have been sacrificed to the vain and ignorant attempt of prematurely establishing menstruation: mercury, drastic purgatives, and emmenagogues having irretrievably destroyed the constitutional power, and paved the way for phthisical disease." (Ib. p. 23.) Often as this statement has been repeated, it still too frequently remains a dead letter in actual practice. With most other well-informed practitioners, Dr. Ashwell would commence the treatment of chlorosis by special attention to the digestive organs and alimentary canal, regarding the disorder of these as second only in pernicious effect to the peculiarity of constitution already mentioned. Drastic purgatives are very properly condemned. "The best aperients are aloes, rhubarb, the sulphate of soda and mannâ, and, if an alterative be necessary, the hydrargyrum c. cretâ. Warm-water enemas, too, are very useful in unloading the large intestines. "The compound decoction of aloes with the compound tincture of cardamoms, the compound aloetic pill with the oil of cassia and hyosciamus, and the vinum aloes with the compound tincture of rhubarb are the forms of these medicines I prescribe." Warm clothing and regular exercise are valuable auxiliaries; and nutritious animal diet and mild malt liquor, as soon as the repugnance to them can be conquered, will be beneficial. The improvement of the digestive organs, indicated by return of appetite and the natural and daily evacuation of the bowels, are generally accompanied by alteration of the complexion and by the partial disappearance of the chlorotic hue; rarely by the immediate establishment or return of the catamenial secretion. At this crisis some of the preparations of iron may be exhibited; "and the sulphate (Dr. Ashwell, p. 25) is probably the most efficacious, and possesses more specific properties than any of the rest." When emmenagogues may be employed is an important question. Dr. Ashwell says, with much truth as we know, that when the health is so far improved that there is less pallor, regularity of bowels, and more and better blood, iron itself is often an efficient emmenagogue. "The ammoniacal injection, composed of one drachm of the pure liquor ammoniæ to a pint of milk, daily injected into the vagina has proved efficient in the hospital." When glandular enlargements and other indications of a strumous habit have been associated with chlorosis, the iodide of iron has been extensively tried by Dr. Ashwell, "and with undoubted success." Leeches and mustard cataplasms to the mammæ (expedients which have both been highly spoken of in this disease) he has tried with doubtful effect. Sixteen cases of various forms of chlorosis are detailed at the end of Dr. Ashwell's chapter on this subject.

We must be brief in our notice of Dr. Ashwell's remarks on

Amenorrhœa. One point we must notice, because we have known the neglect of it tend to the loss of professional reputation, and the unex-

cted distress of the patient. We allude to the occasionally mischievous l even fatal effects that result from the simply effected operation of iding an imperforate hymen. "It must not be forgotten that such ients, with whatever facility the danger may have been removed, and ere incision is often sufficient, are really exposed to the danger of itoneal inflammation." We must be careful then not to speak too fidently of the safety of this operation, and we must watch the patient efully after it has been performed, that we may oppose the possible ger in time. In two cases we have known severe peritoneal inflamma- follow the division of imperforate hymen. Many similar instances on record. Dr. Ramsbotham* mentions several; two of which were al; and we give the following brief extract from his lecture, because are convinced the fact is not generally known. Dr. Waller, for ex- ple, says nothing upon the subject. "The inflammation on these asions seems to have been as severe as puerperal peritonitis itself, and bear much of the same characters. It cannot be referred to the ncture as a cause, so satisfactorily as to the evacuation of the fluid, l the change consequently effected in the situation and condition of ne of the abdominal viscera," (Ramsbotham.)† After offering many d, but we think generally known, remarks upon the limits which ght to be assigned to the use of emmenagogues, Dr. Ashwell gives following useful caution: "Stimulating fluids have been, as em- nagogues, injected into the uterine cavity; and they may perhaps, by e who have not used them, be yet recommended. Death from peri- eal inflammation has several times followed the practice; and in two tances, occurring under my own eye, fatal results had nearly ensued m most alarming attacks of this most formidable malady." (p. 75.) Excepting as a means of arresting hemorrhage, I never now inject the rine cavity." (p. 76.) Dr. Waller, too, (p. 40,) disapproves of the treat- nt referred to in the above extracts, but without deeming it necessary give any other than the rather unsatisfactory reason "that he believes stimulants applied directly to the womb are improper." A fatal case n injecting the uterus is given in our last Number (p. 566); death being posed to arise from the escape of the injected fluid into the abdo- al cavity through the Fallopian tube. Professor Schönlein assures that an injection of ten grains of aloes in a small quantity of warm er, thrown up the rectum when the menses ought to appear, is more tain than any other emmenagogue. Dr. Ashwell has used this enema e or three times a day with advantage. Several of the cases which Ashwell gives of amenorrhœa are rather uncommon, and deserve usal.

Vicarious menstruation is briefly considered by Dr. Ashwell. We may allowed to relate a certainly uncommon case which occurred in our n practice, as a useful commentary upon Dr. Ashwell's remark, that e is not aware any case ever terminated fatally." A young lady, who l always enjoyed good health and good spirits, was exposed to some ere and sudden moral emotions, the consequence of which was a total pression of the menses. After a few months' disappearance of the nses, she threw up from the stomach a small quantity of dark grumous

* Vide Medical Gazette, vol. xvi. p. 327.—Rev. † Loc. cit.

blood mixed with her food, and she continued to do so for many months, the quantity vomited being gradually increased. Between each attack of vomiting there was usually an interval of five or six weeks. Notwithstanding the continued recurrence of these vicarious sanguineous discharges, she remained apparently well in health, nor was her characteristic cheerfulness at all diminished. We gave a decidedly favorable prognosis. The friends, however, became anxious, and a physician of eminence saw the patient with us. He coincided in the favorable opinion we had formed and very confidently stated, of the happy termination of the case; *but in one hour* after we had left this patient, a more than usually severe attack of vomiting came on, and she died faint and exhausted before we could arrive at her residence. Upon dissection the whole mucous surface of the stomach and intestines was found soft, and almost pulpy; and through the whole extent it was evident that blood had been poured forth in large quantities from the capillary vessels. There was no appearance of erosion or lesion of any large blood-vessel. Every other part of the body was perfectly healthy. Let us be cautious then in our prognosis of such cases, and while we admit the very general rule of their safety, let us not forget such occasional exceptions as the above. Gardien speaks with less confidence upon this subject than most other writers; he says,* "Le flux menstruel, par un vice inné, *ou à l'occasion d'une suppression*, peut, par une sorte d'aberration souvent très dangereuse, prendre des voies différentes de l'utérus." The records of medicine teem with cases of "vicarious menstruation," but the most remarkable example we know of is related † by the author we have just quoted.

Dr. Ashwell's description of the various forms of *Dysmenorrhœa* is clear and accurate. We may observe that in some cases of this disease a large quantity of firm coagula is expelled with considerable suffering at every menstrual period; we have known as much as would fill a five or six ounce bottle discharged for several successive months, and a quantity of these coagula are now in our possession. The account of the well-known dysmenorrhœal membrane, which Dr. Ashwell quotes from Dr. Montgomery, is generally correct; but a remarkably splendid preparation of it, which is now before us, rather confirms the statement of Dr. Waller, (page 63,) who calls it "a tough, thick membrane," than that of Dr. Montgomery, who describes it as "thin, flimsy, and very unsubstantial in its texture." In our specimen the whole cavity of the body of the uterus is lined by this morbid product, which closely adheres to the mucous surface of the uterus. We must offer one or two remarks upon the treatment of dysmenorrhœa. "Let the patient," (says Dr. Ashwell, page 109,) "on the first premonition of pain, commence the use of the hot bath at 96 or 98°, and ordinarily remain in it for half or three quarters of an hour," &c. This is good practice in most cases, and will generally be very beneficial; but Dr. Ashwell will, we are sure, allow us to suggest that it is not the best. If, for two or three nights *before the expected menstrual period*, the warm bath is used, or, what is as effectual and much less inconvenient, a piece of flannel, doubled twice or three

* Traité complet d'Accouchemens, tom. i. p. 238, 3me ed.
† Loc. cit. p. 239. (Note.)

times, and soaked in water as hot as the patient can bear it, is wrapped round the lower part of the body, the painful attacks of dysmenorrhœa, even in cases where there had been previously considerable suffering, will frequently be altogether prevented. We have found it necessary, too, in severe cases, to give much larger doses of opium than are recommended by Dr. Ashwell. Dr. Waller, judging from the prescriptions with which he favours us at p. 66, must have been fortunate in meeting with very mild examples of this disease; we think, too, that he will find few practitioners will agree with him in the little confidence he places in the use of the hip-bath, as at least a very useful auxiliary in the treatment.

Upon the subject of *Leucorrhœa*, profuse menstruation, and affections attendant on the decline of the catamenial function, the reader will find in Dr. Ashwell's book concise and instructive descriptions both of the nature and treatment of the maladies referred to. The following statement, the accuracy of which we are fully convinced of from cases which have occurred in our own practice, should inspire more caution than is sometimes exercised by practitioners, upon a question which involves both the honour and happiness of their patients: "One thing is quite true, that, in women of indisputable purity, leucorrhœa is sometimes so acrimonious, as not unfrequently to produce discharge and abrasion in the husband; and, on one or two occasions after abortion, I am almost confident that eruptions and decided ulceration have been among the results of intercourse." (Ashwell, p. 175.)

This first part of Dr. Ashwell's projected work, of which we now terminate our notice, is, upon the whole, creditable to himself, and likely to be useful to those who require the sum and substance of the practical information we at present possess upon the various subjects he treats of. We have no doubt he has drawn his descriptions of disease very faithfully from what he has himself observed; and, as others have done the same before, we meet with much that is contained in other works. Dr. Ashwell proposes to publish in a few months the second part of his work, which will embrace organic diseases; and the third part, shortly thereafter, comprising "the affections of the pregnant and puerperal states."

As Dr. Waller treats in his volume upon many of the diseases described by Dr. Ashwell, we hoped, at the commencement of our notice, to glean some comments from his labours which might be acceptable to our readers. Our attempt, however, which was certainly made with a very impartial spirit, we found to be useless. With regret we are compelled to say that Dr. Waller's sketches of disease are as meager and unsatisfactory as can be well conceived; and we think he has been ill advised in communicating them to the public in their present form. We cannot conclude even this brief notice of his book, without expressing our concern that so respectable a physician should have admitted into his title-page, the superfluous synonymy by which it is disfigured. To professional readers this must at least be useless; and why should Dr. Waller give the ill-natured an opportunity of insinuating that its object was to attract the notice of readers of another class, in the advertising columns of the journals? We ourselves look upon it merely as an example of careless tautology, and condemn it as we do any other literary delinquency; but we are not sure that all our author's readers will judge so leniently.

ART. XI.

Statistical Reports on the Health of the Navy, for the years 1830-1-2-3-4-5-6. (*South American, West Indian and North American, Mediterranean, and Peninsular Commands.*)—*London*, 1840. pp. 323.

WE have already* presented our readers with a detailed account of the valuable reports on the diseases of the army, prepared with so much care and accuracy under the superintendence of Major Tulloch. We now present them with an abstract of the first of a series of statistical reports on the health of the navy, which we owe, in the first instance, to the public spirit of Sir William Burnett, the distinguished Physician-general, to whom the medical department of the navy is under so many obligations, and secondly, to the zeal and industry of Dr. John Wilson, R.N.; and in doing so we cannot but congratulate our profession on the valuable addition thus made to the statistics of health and disease.

In a short prefatory note we are informed that the reports were commenced as early as the year 1836, but that, owing to the sudden death of the officer first appointed to the duty, and the defective arrangements first adopted, it became necessary to begin the whole work *de novo*, and to examine sources of information which had been previously neglected. The officer first employed in drawing up these reports had omitted to trace disease from the ships to and through the hospitals, and had employed an erroneous method of estimating the mean force of the several squadrons. Dr. Wilson carefully avoided these and similar sources of error, and with the able assistance of Mr. George Mackeson, whose services he gratefully acknowledges, has succeeded in giving a faithful report of the health of the navy.

The materials from which these reports have been compiled are furnished by the surgeons and assistant-surgeons in charge of the health of ships' companies. These officers are required to transmit to the physician-general of the navy the following documents, namely, a daily sick-book, monthly or three-monthly nosological returns, and a journal of medical and surgical practice. In the daily sick-book are inscribed in separate columns the patient's name, age, quality, nature of disease or injury, date of discharge from the list, and the issue of the complaint, whether in cure, removal to hospital, invaliding, or death. This journal is transmitted annually. The monthly or quarterly nosological returns are transmitted every month from ships on the home station, and every quarter from those on foreign stations, and still more frequently during the prevalence of unusually severe and fatal diseases. They consist of tabular abstracts of the contents of the daily sick-book; and the diseases are arranged according to Cullen's system of nosology. The journals of medical and surgical practice contain detailed accounts of the symptoms, treatment, and result of diseases, trace them to their source, account for them when practicable, and suggest appropriate remedies. "It is also directed that states of weather, degrees of temperature, the general interior economy of the ships, and whatever else may appear to conduce to health or to induce disease be specified." In addition to these re-

* Brit. and For. Med. Review, No. XV. p. 221; and No. XVII. p. 63.

turns from the medical officers in the charge of ships, there are also returns from hospitals, naval,* military, and colonial,† and from sick quarters.‡

We have been thus minute in describing the sources from which the materials for these statistical reports have been obtained, that our readers may be in possession of the means of judging of their probable completeness. It is the tracing of the several sick and injured seamen, entered in the numerous ship returns, to and from the hospitals or sick quarters to which they have been removed, which constitutes the principal labour of such statistical reports as those which we have now before us; and when we state that the materials have had to be collected from ninety thick folio volumes we shall give some idea of the labour which has been expended. With all the care which could be bestowed on the preparation of these documents, it was impossible to avoid one source of error, we mean that arising from the sickness or death of the medical officers in charge of ships. This of course led to the occasional omission of the several returns to which we have alluded, an omission which could only be imperfectly supplied by stating the average of the force, disease, and mortality of the single ship thus deprived of its medical officers, from the returns of the squadron to which she belonged. Dr. Wilson expresses himself confidently as to the accuracy, ("if not absolute to a unit, at least a very close approach to it,") of the reports in respect of the rate of mortality, but he does not feel the same confidence with regard to invaliding. It appears, indeed, to be absolutely impossible to avoid error in tracing a sick seaman from the ship in which he first fell ill to some foreign hospital, thence to the packet which is to convey him home, thence again to the receiving ship which is to send him to the home hospital or sick quarters, and lastly, from the hospital or sick quarters to his death or discharge, or back to active service, should he be so fortunate as to return to it.

In consequence of the difficulties which stood in the way of a complete statistical report of the health of the navy, it is necessary to employ some caution in estimating the value of the facts established by the report. The same caution is required when we attempt to estimate the influence of climate in the production and cure of disease. In doing so, many circumstances peculiar to the naval service must be borne in mind, of which the principal are the short term (rarely more than from three to four years), during which ships remain in commission, the brief period of sojourn in any particular spot, the great territorial extent of many of the commands, and the consequent frequent and extraordinary change of climate to which the crews are subject. Add to these circumstances the effect, not of climate merely in the ordinary sense of the term, but of the variations of soil and of the emanations arising from it, as well as of food and water supplied by the several stations, and we have a multiplicity of influences brought to bear on the bodies of our seamen in quick

* There are five navy hospitals, namely, at Portsmouth, Plymouth, Malta, Jamaica, and Bermuda, and two marine hospitals, those of Chatham and Woolwich.

† Sick seamen and marines are received into all military and colonial hospitals, in such places as do not contain naval hospitals.

‡ There are fifty sick quarters for the reception of sick and injured seamen and marines, all of which are in the United Kingdom.

succession, the particular value of each of which it is altogether impos sible to define or fix. These observations apply more especially t chronic diseases, whether primary or the consequence of acute affections acute diseases themselves create little embarrassment, and can be com monly traced with care and certainty to the localities which produc them. Thus the various forms of fever, elephantiasis, dysentery, or acut inflammation in general may be fairly ascribed to the influence of th spot in which they arise. On the other hand, "in consumption and othe insidious diseases of the lungs, and in various chronic affections of th liver and bowels, it is often impossible to detect their origin, and conse quently to ascertain the portion of the command to which they shoul be ascribed; it is often difficult to determine whether, and to wha extent, change of place, within the limits of the command acts bene ficially or detrimentally in such cases." Of the great extent of some o the commands, we may take the West Indian and North American a an example. "It extends from the equator to the 60th degree of nort latitude, and embraces every British possession, continental and insula from Guiana to Cape Charles on the coast of Labrador." A comman so extensive, embracing as it does so great a variety of climate, occasion much uncertainty in all our attempts to trace diseases to their exac cause; but there can be no doubt of the beneficial influence of the fre quent changes from high to low latitudes, and from unhealthy to health climates on the health and efficiency of the ships' crews. Thus w learn from the report that "after a certain period of service in the Wes Indies, there is often so much diminution of organic force as to rende men of little original power or advanced years incapable of laborious dut and to make them more susceptible of disease, both acute and chronic In these cases, and after attacks of severe disease, when convalescenc is slow and uncertain, a run to Halifax or the Gulf of St. Lawrenc acts like a charm. Health and strength, and with them spirits, are ac quired at a rate scarcely to be believed by those who have not witnesse their progress, and many who but for the change must have sunk, o been sent to England, have been restored in a short time to full efficienc within the limits of the command, whether they remained in the norther or returned to the southern division of it."

It is necessary to observe that, in the returns of the sick and hurt, th patients are not classed, but all, from the admiral in command to th youngest boy in the ship, are comprehended in one table, in which th diseases and injuries affecting them are arranged, but in which neithe the rank nor line of duty of the individuals composing the ship's com pany is specified. Great difficulties are obviously placed in the wa of any classification which should at the same time describe the variou influences to which the several classes are subject, and without suc description no information of any value could be obtained. Aware c these difficulties, Dr. Wilson has very judiciously abstained from thos minute classifications which only lead to imperfect and erroneous con clusions, and throw discredit on a mode of investigation to which w must owe almost all the valuable information yet to be gleaned fror observation. For similar reasons, all mention of age and its influenc on disease has been omitted from the report, as the age of seamen is nc readily obtained where so many motives to false statements exist.

The introduction to the report, from which the foregoing abstract has been taken, enters into minute detail as to some other points necessary to be known and borne in mind in order to form a correct estimate of the report itself. Amongst these, the important article of *diet* holds a prominent place, and a minute and detailed account is given of the different kinds of provisions with which the ships are supplied. It is sufficient for our purpose to observe that the naval rations are abundant in quantity and excellent in quality; the several articles of food being of the very best kind which can be procured, and care being taken to vary the diet as much as possible. After fourteen days' use of salt food, lemon-juice with an additional quantity of sugar is used as an antiscorbutic. The introduction of tea, coffee, and cocoa, as substitutes for a moiety of the spirits formerly allowed, is not the least among the many important improvements which have been gradually adopted in the dietary of our ships of war. The diet of the sick has kept pace in improvement with that of the healthy portion of the service, and a glance at the list of articles allowed to patients in the royal naval hospitals and marine infirmaries, and as far as practicable on board ships, will show that nothing is omitted which can possibly promote the recovery of the sick.

In another article of not less importance than food, an improvement fully as great has of late years taken place; we mean the supply of *water*. The substitution of iron tanks for wooden casks, partially introduced into the navy before the year 1815, but since that period universally employed, has furnished an abundant supply of pure water, and has thus added materially to the comfort and salubrity of our ships of war.

A few words on the modes of employment and berthing of the men, and on the cleaning and ventilating of the ships, will conclude what we have to say by way of introduction, to the subject-matter of the reports.

Seamen and embarked marines, with the exception of those employed as sentries, are divided into two watches, which work the ship by turns. Each party has four hours' duty and four hours' rest; in other words, is four hours on deck and four hours below, excepting the four hours between four and eight o'clock at night, which are divided into two half, or, as sailors call them, dog-watches. The effect of this arrangement is to alter constantly the periods of labour and rest to each party, so that the men who have the first watch one night have the second watch the next night, and so on. "In this way, though there is a sufficient time for rest, it is never long continued, and sleep is broken into short periods. Whatever effect such a division of labour and rest may have on constitutional vigour, though it is perhaps the best that can be devised, it probably contributes much to the frequency of some affections, such as catarrh and rheumatism, to which seamen are so subject. At midnight, or four o'clock in the morning, the men relieving the deck rush from their beds into the open air, often very inadequately covered, perhaps perspiring profusely, and pass in an instant from a highly-heated and debilitating to, it may be, a really cold, always to a comparatively cold, atmosphere. In such circumstances it is to be expected that catarrhal, rheumatic, and other affections, traced to sudden reductive

changes of temperature, should be numerous. These remarks apply chiefly to ships at sea. In port, the number of men kept on deck lessens with the increasing safety of the anchorage, till, in ships moored in secure harbours, all hands, excepting the officer, petty officers, and sentinels in charge, may pass the whole night in bed."

Seamen and marines mess and sleep on the middle and lower decks in first-rates, and on the lower deck in all vessels under first-rates. Ships of the line, moreover, have a deck (the orlop deck) between the lower gun-deck and the holds, whilst the lower deck in frigates and all smaller vessels is immediately above the holds. The means of ventilation in ships of the line are consequently far superior to those enjoyed by smaller vessels; for in the former there are ports which can be kept open in moderate weather, whilst in the latter there are only small apertures (scuttles) close to the water, which cannot be kept open except in an absolute calm at sea and very fine weather even in harbour. In all ships, whatever their size, in consequence of the great number of men on board, there must be great crowding between decks, and this crowding is greater as the vessel is smaller; in this respect then, as well as in point of ventilation, at least by means of the ports, ships of the line have the advantage over smaller vessels, but it may be doubted whether the interior ventilation through the hatches, when the ports are closed, is not so much more complete in vessels with one deck than with those with two decks as to counterbalance the advantages in point of exterior ventilation which ships of the line enjoy. To show to what an extent this crowding of the sleeping berths takes place, it is sufficient to state that the usual space between the suspending points (clues) of the hammocks is from fourteen to eighteen inches; so that when they are extended by the beds, the bodies of their occupants are in contact. "When at sea, with a watch on deck, the accumulation and pressure are reduced by a half, but when in secure harbour, 500 men perhaps sleep on one deck, their bodies touching each other over the whole space laterally, and with very little spare room lengthways." Under ordinary circumstances, this crowded state of the decks does not appear to exercise any very deleterious influence on health, but its influence on those already attacked by diseases of an infectious character or accompanied by great debility and depression, must not be lost sight of in estimating the effect of a seafaring life on health.

Cleanliness both of the vessels and of the person, we need not say, is well attended to in the navy. There is little doubt, indeed, that in one respect it is carried too far. We allude to the practice of frequently washing the several decks, a practice censured by Dr. Wilson, and condemned by experience. There can be no doubt that this practice keeps the decks of a vessel in a constant state of dampness, which must prove highly prejudicial to health. Cleaning the decks, with the exception perhaps of the upper deck and upper gun-deck in ships of the line, by *dry stoning* is certainly very much to be preferred to repeated washings. The moist state of atmosphere kept up by this practice must be borne in mind in summing up the injurious influences brought to bear on the sailor. Two or three striking examples of the effect of moisture in inducing the scurvy are related in Parry's Voyages, and we are ourselves

indebted to an old naval officer for an apt illustration of its effects drawn from a comparison of the health of two ships' crews, the one English and the other French, occupying for some time the same station. In the English vessel the decks were constantly washed, in the French vessel they were dry-rubbed. The scurvy was rife in the one, while not a single case of it existed in the other. In all other respects the English vessel was at least on a par with the French one.

Ventilation, which has already been incidentally mentioned, has not yet been carried to a high degree of perfection on board ship, nor are its principles as yet thoroughly understood as applied to buildings on land. Wind-sails, which have hitherto been the chief means of ventilation employed, are objectionable, inasmuch as they are inoperative in calms and in wet weather, and even with strong breezes and in dry weather they ventilate but a small portion of the decks. The apparatus lately invented by Captain Warrington is free from these objections, but is yet far from perfect. There is no doubt that it will admit of considerable improvement.

The last iufluence noticed in the report as brought to bear on the health of seamen is amusement and intellectual culture. We need not remind the professional reader of the vast importance of this influence to the bodily health, and we congratulate ourselves that it has been at length so successfully brought to bear on the minds of our seamen. Not the least interesting fact mentioned in the introduction to these reports is the following: That "by an admiralty order, dated August 1838, libraries are directed to be established in each of Her Majesty's ships, for the use of the crew, furnished at the public expense, and placed in charge of the schoolmaster. The books, amounting to 270 volumes for larger, and 100 for smaller ships, exclusive of bibles, are judiciously chosen, with the view of combining amusement and instruction, and making the first subsidiary to the last. Besides the accomplished men now appointed to instruct the junior officers, it is further directed by an order from the admiralty, May 1837, that a fit person shall be appointed to give elementary education, comprehending reading, writing, and arithmetic, to the sailor boys and other seamen and marines who may require it." Though last in order, this is by no means least in importance, amongst those regulations of the admiralty which have tended so strikingly to improve the health and increase the efficiency of our marine.

We cannot quit this introductory chapter of the reports, without adverting to the vast improvement which has taken place of late years in the comfort and health of our seamen, and in the consequent efficiency of our naval service. "Formerly," says the report, "a ship of war was on many accounts an object of aversion," and well it might be. "A diet consisting of very salt beef, biscuits long baked, and puddings made of salt suet and flour..... Water so putrid and offensive, often so thick and green from vegetable admixture and decomposition, and emitting so strongly the fetor of rotten eggs, as to disgust at once the sense of smell and of taste;" the best and almost only luxury of the seamen an exorbitant allowance of spirituous liquors, at sea as on land the fruitful source of disease, and misery, and insubordination, and crime in all its shapes; the ship itself damp, filthy, ill-ventilated; the air of the wells so contaminated that fatal asphyxia was by no means a rare consequence

of inhaling it; personal cleanliness neglected, the clothing insufficient; but little care taken to amuse the mind and none to instruct it: add to all these discomforts, a discipline not merely strict but severe, and punishment too often inflicted at the instigation of momentary passion—and we have a faithful picture of the naval service in former years. But these hardships and deprivations, painful as they are to contemplate, formed nevertheless but a small part of the miseries of our seamen. Disease was wanted to give the finishing touch to the picture, and convert the scene of hardship and deprivation into one of thrilling horror. "Scurvy, putrid ulcer, malignant dysentery, and fever, allied to that of gaols, suddenly swept off the greater portions of many ships' crews, and well-nigh depopulated fleets." The first of these diseases alone has sufficed to place a well-manned vessel at the mercy of the winds and waves. Witness Lord Anson's ship, the Centurion, in 1742, "the crew so weakened by scurvy, that only eight men were capable of doing duty, and these so reduced in health, that had the ship been compelled to keep the sea a very few days longer, it would not have been possible to have brought her to an anchor at Juan Fernandez, and she must have gone adrift in the Pacific Ocean, as once happened to a Spanish ship in the same ocean under the same circumstances."* So dreadful was the mortality at sea from this one cause alone, that the description given by a Portuguese historian of the early expeditions of that nation in pushing their discoveries on the African coast in search of India, seems but a warrantable hyperbole. "If the dead," says he, "who had been thrown overboard between the coast of Guinea and the Cape of Good Hope, and between that Cape and Mozambique could have tombstones placed for them, each on the spot where he sank, the whole way would appear one continued cemetery." In large fleets the mortality was not less frightful than in single ships. Sir Richard Hawkins, the great navigator who lived in the time of Elizabeth and her successor, relates that in the course of twenty years, he "had known of 10,000 seamen" (a number short by only about 4000 of all who served in the fleet which conquered the Spanish Armada) "having perished by scurvy alone." This fearful mortality is not a story merely of the olden time, for even so late as the year 1780, Sir Gilbert Blane found that in a fleet manned with between 7000 and 8000 seamen, the mortality had been one in seven the preceding year.

Turn we from this harrowing picture of hardship, disease, and death, to that which is presented by our ships of war in more recent times. A diet at once abundant, and varied, and wholesome, a plentiful supply of pure water, cleanliness carried almost to a fault, constant attention to the ventilation and drying of the vessel, an ample supply of warm clothing; add to these bodily comforts so beneficial to health the excellent diet and superior medical treatment provided for the sick, and the arrangements lately adopted for amusing and instructing the mind, as well as the improved discipline and admirable limitations set to the capricious exercise and abuse of authority; and we may safely affirm that a ship of war, instead of being as formerly "an object of aversion," cannot fail to present many attractions to the seaman; and sure we are that it is by arrangements such as these, and not by a vain attempt to

* Sir Gilbert Blane, Health of Seamen, prefatory discourse.

compete with the merchant service in the wages offered to the sailor, that our navy will be manned with hardy frames and willing spirits, ready now as heretofore to brave every danger in defence of the country which shows itself thus careful of their health, comfort, and improvement.

The success which has attended these well-directed attempts to preserve and improve the health of our seamen is too well known to require any lengthened remarks. The late expeditions to the Polar seas supply the best commentary on their efficacy. The share which each of the several arrangements which we have specified has had in producing the general result may admit of some controversy; and it might be interesting to enquire how much of the good effected is due to the improved medical treatment of the sick. That this has played no unimportant part in achieving the high state of health now enjoyed in the navy, no one, but especially no medical man, will be disposed to doubt; still it is to hygienic arrangements, properly so called—to improvements in diet, ventilation, clothing, &c.—that the result is mainly attributable. In a national point of view, and as a question of finance, the health of the navy is one of the most important which can occupy our attention; and the success which has attended the methods employed to preserve it, may well raise expectations of still more extensive benefits to be conferred on other sections of the community, whose interests in this respect have been hitherto so grossly neglected.

If it be true, as it undoubtedly is, that these improvements in health have alone sufficed to double the effective force of our navy, to make one ship for all purposes of navigation and warfare equivalent to two of equal force, to enable a vessel to keep the sea for twice or thrice the time which was possible some fifty years ago; if it be true that at the old rate of mortality, all Europe could not have furnished the seamen necessary for our defence and safety during the great revolutionary war,*—then it is a mere waste of words to prove that public health is a national blessing, and the neglect of it a calamity and a curse. Motives of economy alone must influence government to spare no exertion by which the health of our navy may be promoted and secured; for it is clear that if one man can be made to do the work of two, the expense of one man must be saved; and, if this unit be multiplied by the number of men actually employed in our navy, we have a product which can scarcely fail to strike the mind of the economist. But this by no means represents the whole saving to the nation which accrues from such improvements in the health of our seamen; we must add to this all the expense formerly incurred in raising men to supply the place of those whom disease had cut off or rendered useless. But we have not yet exhausted all the arguments which might be urged in favour of a scrupulous and watchful care of the health of our navy. It is not merely that our force has been doubled and our expenses diminished by more than a half, but each individual employed must be made more effective by having his health carefully preserved to him, and his life prolonged to gain increased experience and aptitude for his employment.

We trust that the encouraging example afforded by the navy will not be lost upon those who have it in their power to extend these provisions for the preservation of health to the other branch of the public service,

* Sir Gilbert Blane, Health of the Navy.

and to the community at large. The necessity may not appear so pressing, nor the advantage so obvious, but the one is as real as the other is substantial. Sickness and death in our fleets formerly cost an extravagant outlay of public money; do the pestilential diseases and frightful mortality of some of our large cities cost the public nothing? Do fever hospitals and workhouse infirmaries, and pauper burials, and widows and orphans, and cripples—the victims of diseases, which might have been prevented but cannot be cured—cost nothing? If but a small fraction of the money expended on objects such as these had been employed in constructing sewers and widening streets, and providing places of recreation in the centre of our crowded cities; in letting the light of heaven into their dark and gloomy recesses, and the sweet breath of heaven into their foul and reeking habitations, we should not have now to lament over such scenes as those which the great manufacturing capital of the north has lately disclosed. The preservation of the public health is economy in every sense. It not only saves money, but it makes it. It substitutes men for children, productive for unproductive labourers. It supplies a country with its best riches and its cheapest defence—the arm strong to labour and to fight.

But these reflections are leading us too far from our subject. The report comprises three commands—the South American, the West Indian and North American, and the Mediterranean and Peninsular, and extends over seven years, namely, from 1830 to 1836, inclusive. The diseases, invaliding, and mortality for each year are represented in four tables, of which the first shows " the total number of cases; the number of all diseases and injuries in classes; the number of cases sent to hospital, invalided, and dead; with the ratio of each per thousand of mean strength." The second table exhibits " the total number of cases; the number of all diseases and injuries in classes; the number invalided and dead; with the ratio of each per thousand attacked." The third table represents " the number of cases of principal diseases and injuries; the number sent to hospital, invalided, and dead; with the ratio of each per thousand of mean strength." The fourth table shows " the number of frequent, but not often, fatal diseases; the number sent to hospital, invalided, and dead, with the ratio of each per thousand of mean strength." The diseases included in this table are superficial inflammation of the extremities, rheumatism, catarrh, and diarrhœa. The first two tables are thus subdivided—febrile diseases, nervous diseases, cachectic diseases, and local diseases. As we have already stated, Cullen's nosology is that adopted in the navy. We are sorry for it, as we believe it to be based too much upon mere hypothesis, and to be very badly adapted to statistical reports. For this purpose we greatly prefer the arrangement adopted in the Army Reports by Major Tulloch; and it would certainly have been much more convenient to have been able to compare the Navy Reports with the able statistical documents which preceded them. On glancing over these first two tables, we observe such anomalies as the following: pulmonary consumption classed with " hemorrhages with fever," and forcibly separated from inflammatory affections of the lungs; diarrhœa placed among spasmodic diseases, as a sub-class of nervous diseases, whilst dysentery occupies a place among febrile diseases, because, forsooth, it is a flux, accompanied by fever; then again jaundice, which

most medical men regard as an affection of the liver, or its excreting ducts, is marshalled with syphilis and scrofula, as a cutaneous disease, under the general head of cachectic diseases. Emaciation of the body is made to "include intestinal worms, and certain *obscure consumptive diseases* of the alimentary canal," whilst "swellings and dropsies" form a sub-class of cachectic diseases, though "tumours, including aneurism, varicose veins, and bubo," are "local diseases." We are far from wishing to lay the obvious defects of this sort of classification to the charge of the compiler of these tables. He has been obliged, no doubt, to frame his tables from the reports submitted to him, and these unfortunately follow the nosology of Cullen; which, bad as it is, was, we believe, the best at the period when the plan of these medical returns was introduced. The third table happily dissolves, to a certain extent, this unnatural alliance between diseases which have so little in common, except the faulty hypothesis which first brought them together, and we are presented with a catalogue of the principal diseases, which we copy for the benefit of those who may wish to know what those principal diseases are. The list is as follows: Fevers, inflammation of the lungs, inflammation of the liver, organic diseases of the brain, consumptive diseases of the lungs, expectoration of blood, dysentery, inflammation of the stomach and bowels, delirium tremens, syphilis, gonorrhœa, ulcers, wounds, and accidents.

The tables for each year are followed by a few pages of remarks, either suggested by the tables themselves, or by the several journals and reports from which they have been compiled, and the results obtained from the seven successive years are thrown into four general tables, arranged like those of the individual years, and followed like them by general remarks. Towards the end of the volume an attempt is made to ascertain the "influence of forms of ships on health," by constructing tables similar to the first of those already described for vessels of the first class (seventy-two guns and upwards), for the second class (frigates), the third class (corvettes), and the fourth class (steamers). The report is concluded by tables presenting the individual facts, from which the aforesaid tables have been constructed, and specifying the name and force of the several ships employed.

Having thus described the contents of the reports, the next question is, how shall we make use of the extensive materials contained in it, so as to place the greatest possible amount of information in the hands of our readers? After much reflection, we have come to the resolution to disappoint them for the present of some of the instruction which they perhaps expected to obtain, and to postpone an analysis of much of the most interesting matter comprised in this volume until the remaining reports, which, we are glad to find, are in a forward state of preparation, make their appearance. When all the reports are before us, we propose making a careful analysis of all the information they contain with regard to some of the principal diseases, comparing one command with another, and embodying in one account all the knowledge to be gleaned, not from the tables merely, but from the text of the several reports. If we have leisure for so grave an undertaking, we shall then compare the health and diseases of the navy with those of the army; but, that we may do this, it will be necessary to remodel the navy reports, so as to be enabled to compare them with the more simply and scientifically

arranged reports of Major Tulloch. After this explanation of our intention, it is obvious that our present notice must be considered merely in the light of an introduction to the more laborious articles which we have in store, and we shall accordingly content ourselves with giving such an account of the several commands as may throw light on the discussions into which we shall have to enter.

The South American Command. "The South American Naval Command embraces a great extent of coast and cruising ground. It extends on the east side of the peninsular continent, from Para to Cape Horn, and on the west from Cape Horn to Panama, and thence to California. It reaches from the 30th degree of north to the 58th degree of south latitude, across the Pacific, from the equator to 58 degrees south, across the South Atlantic; and from Cape St. Royal in the 35th, to California in the 120th degree of west longitude. It is therefore exposed to almost all degrees of atmospheric temperature, from the highest to the lowest, with the ordinary meteoric agencies thence arising. Many of the ports and places of anchorage, and adjacent territories, differ from each other in almost every manner and degree. In relative position and nature and extent of exposure, in respect of contiguous land, its general form, and distance, and influence on wind and rain, and in the soil and products of the soil, there is great, sometimes extreme difference in the various places resorted to by British ships. The places principally frequented are Rio de Janeiro, Buenos Ayres, Bahia, Panambuco and Para, Valparaiso, Callao, Coquimbo, Panama, and San Blas. In some of them art has been busy and effective; large towns have been built, and the ground has been cleared and cultivated in the neighbourhood of the towns, and on the margins of the bays and harbours; in others, beyond rearing houses and stores, things remain very nearly in their natural condition. Hence extensive marshes are in close contact with many of them; while in others, but more from natural formation than the labours of man, dry land, and little productive of vegetable matter, abounds. The places named are, with three exceptions, situate within the tropics, some near their external limits, some close to the equator, and others at different intermediate points; yet with all such difference in position, soil, products of soil, and climatorial heat, the inhabitants of the shores of this vast continent, whether permanent or occasional, enjoy a high and a singularly uniform degree of health.......It is peculiar to this command, that during peace the ships are employed altogether on an alien coast. Along its whole extent, and nowhere within the limits of the command, excepting the recent small settlement at the Falkland Island, is there any British possession. Hence there are no hospitals for the reception of British sailors; the want of which, though it may not materially and directly affect life, increases the necessity for invaliding. Many men labouring under mere chronic forms of disease might be cured in hospital, and saved to the squadron, who cannot be restored to health on board ships."

The aggregate numerical force of the seven years was 17,254, giving an average of 2,465 per annum. Taking one year with another, the number of vessels employed was twenty-five, of which one was a ship of the line, five or six frigates large and small, and the remainder sloops and brigs, including packets. It has already been stated, that the inhabitants of the shores of the continent included in the South American command enjoy a high state of health. The same remark applies to the naval forces employed, the mortality being 7·7 per 1000, a mortality much inferior to that of persons of corresponding ages in the United Kingdom.

West Indian and North American Command. This command "extends from the equator to the 60th degree of north latitude, and em-

braces every British possession, continental and insular, from Guaiana to Cape Charles on the coast of Labrador. It consequently contains almost every degree and variety of climate, dependent on temperature, atmosphere, and soil, and their reciprocal relations, many of them being of course common, some of them in a great measure peculiar." The great extent of this command, and the variety of climate which it embraces, render it very difficult to draw conclusions of any value concerning the influence of climate on health. To arrive at any satisfactory result it would be necessary to separate the West Indies from North America, and to group the Bahama islands and the Bermudas with the former. As this cannot be done, we must content ourselves with such mixed results as can be obtained from the tables. The mean annual force amounted to 3,326, and the number of vessels of all descriptions, many of which were of small size, to forty-seven. The annual rate of mortality in this command, on an average of seven years, from disease alone was 18·1 per 1000 of mean strength, from fever alone 11·1.

The Mediterranean and Peninsular Command. "This command embraces the seas and shores of the Mediterranean and Gibraltar. It extends over less space than some other commands, comprising fewer degrees of latitude, and is less exposed to extreme difference in degrees of temperature; it does not include more than 12 degrees, those, namely, from the 32d to the 44th north. As respects geographical position, therefore, the term 'temperate' has been especially applied to it, and it has obtained high reputation for its influence on health. Yet the difference between the south and north shores, as regards temperature, is often great, especially in winter; the vicissitudes on the north coast are sudden and violent, and the effects of the climate either in the prevention or remedying of some diseases, particularly those affecting the lungs, are perhaps not so great as they have often been represented. The sirocco, south-east wind, blowing from the African continent, is singularly depressing, suddenly producing such languor, oppression, and feelings of feebleness, as neither its temperature, nor other appreciable quality can account for. It seldom continues to blow during many consecutive days; what its ultimate influence on health might be, if long-continued, cannot be ascertained, though there can be little doubt that it would be highly prejudicial; but during the period it lasts, notwithstanding the distressing power it exercises over sensation, it does not appear to injure the organic powers of life. Its influence is most felt near the coast of Africa, but it is often powerful at Malta and Sicily, and sometimes reaches the north shores of the Mediterranean. Malta, on account of its central situation, its arsenals, and the excellence of its harbours, is the principal naval station. During nine months of the year, the temperature is moderate, the weather being generally clear and fine, which, with the excellence of fresh meat and vegetables procured there, produces highly beneficial effects on the health, comfort, and efficiency of the naval force employed in the Mediterranean. The other three months are hot, sometimes in a high degree; but either because it is not sufficiently long continued, or because there is not much of morbific material for it to act on, the heat has not, except on some very rare occasions, proved injurious to health to any extent. Every anchorage in the circuit of the Mediterranean is visited, and more or less frequented, by ships of war. The ports most resorted to are Malta, Smyrna, various Greek and Levantine islands, and Gibraltar, the extent to which they are occupied differing much at different periods.......The numerous ports and places embraced by this command differ greatly as to extent, exposure, and physical structure, and considerably in regard to external heat; they therefore act very differently on the health of ships' companies resorting to them; but on the whole, and apart from some rare but severe epidemics which have affected some of them, their influence,

combined with that of the sea-climate, is very favorable.......The naval force employed on the coasts of Spain and Portugal is included in the Report for the Mediterranean."

The mean annual strength of the squadron in this command was 7,958, number of vessels forty-five to fifty-six, including many ships of the line and large frigates. The mean annual mortality in this station was, from disease alone, 9·3 per 1000 of mean strength. Comparing the mortality of the three commands included in the report, we find the result as follows:

	Disease and accident.	Disease alone.
South American	8·9	7·7
West Indian and North American ..	19·6	18·1
Mediterranean and Peninsular	11·1	9·3

The South American command, therefore, is the most healthy, next to that the Mediterranean and Peninsular; and, by far the most unhealthy, is the West Indian and North American.

We do not intend at present, as we have already stated, to enter into the subject-matter of these reports at greater length. We must remind our readers a second time that the present sketch is merely introductory to a more minute and careful abstract, which we hope to render both interesting and instructive. We shall notice the forthcoming reports in the same way, and reserve ourselves till we have them all at the same time under our eye. In the meantime we dismiss the present volume with much commendation to the compiler for the industry and intelligence he has displayed, but with regret that the adoption of a defective nosological system has impaired the value of his labours.

ART. X.

La Chirurgie de M. Dieffenbach. Par CHARLES PHILLIPS. Première Partie, avec quatre Planches.—*Berlin*, 1840. 8vo, pp. 200.

The Surgery of M. Dieffenbach. By CHARLES PHILLIPS. First Part, with four Plates.—*Berlin*, 1840.

THIS short treatise, written by a gentleman who, as M. Dieffenbach's assistant, had ample opportunities of observing closely his practice, describes the method of operation and the general results in certain subjects of surgery to which M. Dieffenbach has more particularly given his attention. We shall consider these subjects separately, briefly dwelling on any new features that the method of operation or the after-treatment may present.

The first few pages are devoted to the description of Professor Jungken's operations for staphyloma and artificial pupil. The former differs in no degree from that usually followed by all surgeons, namely, removal of the projecting cornea. In making an artificial pupil, the professor introduces a knife across the cornea, which he freely incises; the aqueous humour being expelled and the iris protruded, he seizes a portion of the iris with a fine hook and cuts it off with scissors. This mode of operation has the advantage of preventing the adhesion of the iris to the cornea, which commonly takes place when the former membrane is made to protrude through a small opening in the latter.

His boldness and success in advancing orthopedic surgery, one of the most important improvements that has been made in the modern treatment of disease, would have alone rendered the author's name celebrated. "Dieffenbach," says Mr. Phillips, "has, up to this time, operated on 300 distortions of the foot, sixty cases of wry-neck, a great number of contracted arms, hips, and knees. He has reduced a dislocation of two years' standing, and cured eighteen* cases of strabismus. In all these cases he has never experienced any considerable hemorrhage or caused lesion of any of the great nerves or blood-vessels."

In treating the various forms of talipes, he has divided all the tendons that pass from the leg to the foot; and, not content with a single operation, he repeats it until the deformity disappears: in one case the tendo achillis was divided twenty times before a complete cure was obtained. In valgus, the peronei muscles and extensors of the toes; in varus, the tendo achillis, flexors of the toes and of the great toe, the plantar fascia, tibialis anticus and extensors of the toes, have all been divided at the same time. The subcutaneous section of muscles has also been applied to the reduction of a dislocation of the shoulder.† The new capsule of the joint, the tendons of the supra and infra spinatus and the latissimus dorsi required to be cut across before the replacement of the bone in its proper situation could be effected. We think it doubtful how far such practice is to be recommended. In a dislocation of two years' standing, the head of the bone has formed for itself a new articulation; the muscles have adapted themselves to the displacement, and motion has become tolerably perfect; the former articulating surface of the bone has also become altered in form. These considerations would make us hesitate before we recommended a patient to submit to the risk of so severe an operation as dividing muscles deeply situated in the axilla would be, with the prospect of imperfect success. The tendons of the hip-joint and knee have been cut across in contractions of these articulations with a most favorable result; and cases are mentioned in which, when even anchylosis of the knee-joint had occurred, and the osseous union was broken up by the forcible extension of the joint after division of the tendons, the patient recovered a considerable degree of motion in the limb.

We have already noticed in our Journal the operations for strabismus.

Rupture of the perineum is the next subject noticed, and, in the treatment of this injury, cases of extraordinary success are mentioned. Dieffenbach attempts to unite these lacerations immediately after their occurrence, when he has the opportunity. In cases of older date, he is of course obliged to pare the edges of the divided parts before bringing them together. The union is effected by means of a great number of interrupted sutures placed very deeply: the twisted suture is occasionally used. The plan of treatment by operation is modified according to the variety of the case. He considers the success he obtains is not attributable to any peculiar mode of operation, but to the after-treatment. After the operation the patient is placed on her back in bed: the legs are not tied together, as is the usual practice; the secretions of the vagina that cannot but take place immediately after delivery, and when so severe an

* Since this was written he has operated on hundreds. See our last Number, p. 570.

† See our last Number, p. 566.

accident has occurred, instead of being allowed to remain in that cavity, and passing between and irritating the edges of the wound, are cleared away by frequent injections of water. Opium is administered to produce constipation, and at the end of seven or eight days an injection of oil is given: an evacuation having been procured by this means, constipation is again produced by the same medicine. By this treatment a great part of the lacerated surfaces will be united. Should any unnatural opening remain, it is touched with tincture of cantharides and argent. nitrat. In some of the cases a fistulous opening remained between the rectum and vagina: it was allowed to contract as far as it would naturally; the edges were then pared, brought together by the interrupted suture, and the same after-treatment pursued as is above indicated: if one operation is not successful it is repeated. This mode of treatment is evidently a great improvement on the usual method of tying the legs together, pressing the lacerated edges of the wound one against the other, and keeping them constantly bathed in the irritating secretions of the vagina and rectum.

There is nothing peculiar in Dieffenbach's mode of operation for fissures of the palate. After paring the edges of the fissure, he introduces small needles from behind forwards by means of a *porte-aiguille.* The ligatures are made of lead; the great recommendation of which he seems to think is, that the operator is enabled to tighten them without introducing his fingers into the mouth. It appears to us that this is rather an objection to metallic ligatures. The fingers of a careful operator would certainly be much less likely to irritate the patient than the instruments that must be used in this operation.

Tumours of the face and operations on the upper and lower jaw have occupied the attention of Dieffenbach to a considerable degree, but we cannot commend his practice in this department of surgery. He does not want boldness to execute, but there is a great want of judgment in the choice of the cases submitted to operations: all sorts of tumours, whether malignant or otherwise, are mentioned as being attacked by the knife. Now it cannot be doubted that the removal of the whole or portions of the upper or lower jaw are among the most successful of surgical operations, when undertaken in proper cases; but it is only to tumours of slow growth, firm consistence and benign character, that operation is applicable; and we cannot but think it a matter of cruelty, and perfectly useless, to subject a patient to these severe operations, when, from the nature of the disease, we may predicate an almost certain return. Such is the case in many of Dieffenbach's operations; and repeated removals of portions of the jaws, together with free applications of the actual cautery, have failed to eradicate the disease. Dieffenbach recommends, in removing the upper jaw, to make incisions in the skin, commencing from the zygoma inwards towards the upper part of the nose, passing under the lower eyelid, and another incision down the centre of the nose and through the upper lip: the flap of integument is then dissected back. We think that preferable incisions, and not likely to produce so much deformity, are those carried from the prominence of the malar bone to the angle of the mouth, and by the side of the nose, round the ala, and through the centre of the upper lip. The flap of skin is easily raised; sufficient space is given for the future steps of the operation; the Stenonian duct is

avoided, and but few branches of the portio dura are wounded. Dieffenbach removes the bones by means of small saws, strong scissors, &c., and applies the actual cautery most freely to the cavity. These instruments are far inferior to the strong cutting forceps, and the application of the actual cautery is a piece of needless cruelty. No operation of this kind should be undertaken, either where the disease is such that it has contaminated the surrounding parts, or where it cannot be entirely removed by cutting in the healthy structures: hence the perfect inutility of the cautery.

In plastic operations, Dieffenbach's practice is somewhat different from that usually pursued. In restoration of the whole of the nose, he borrows the skin from the hairy scalp, and connects it with the part to which it is to be transplanted by means of a long narrow pedicle down the centre of the forehead. The object of this mode of incision is to prevent the deformity that a large cicatrice in the forehead produces. The hairs soon cease to grow when the skin is transplanted to its new situation. In restoration of the ala of the nose, a very curious operation is performed. A central incision is made down the nose, and the skin is dissected off on each side from the bones, on the sound side, carrying with it the cartilage of the ala; the central cartilage is thus exposed, and out of this a triangular piece, the apex downwards, is cut; a similar piece is cut from above the ala on the sound side of the nose, which is by these means shortened to a sufficient degree to correspond with the shortened ala on the diseased side. Of course by such an operation the form of the nose is much altered, and a most determined "nez retroussé" is formed. We cannot but think the transplantation of a portion of the cheek to supply the deficiency in the ala is a much easier and less painful operation, and less likely to alter the general contour of the features. Several other cases of restoration of the eyelids, point of the nose, &c., are mentioned, which do not present any great interest, and for an account of which we must refer our readers to the work itself.

The latter pages of this little book are occupied by a number of unconnected surgical cases, for which operations have been performed. Among these, a new way of treating prolapsus uteri is mentioned. When this disease is very well marked, and the greater part of the uterus prolapses through the os externum, Dieffenbach, first having reduced the prolapsus, introduces a red-hot iron into the vagina and cauterizes the lower part of it and the os externum. When the sloughs separate, union takes place between the sides of the vagina and the labia, and a barrier is formed which completely prevents the prolapsus descending. This is rather a severe proceeding, and there are few patients who would be induced to submit to such an operation; and, luckily, it is rare that the prolapsus is to such an extent that relief cannot be afforded by properly constructed pessaries.

In strictures of the urethra, where retention exists, and a catheter cannot be passed through the stricture, Dieffenbach's practice is to cut transversely across the urethra, posterior to the stricture. He observes that the operation of cutting on a catheter in the middle line is a very difficult one. We should think he never could have seen practised or have attempted to perform this latter operation. By placing the finger in the rectum and passing a sharp bistoury, with the back towards the bowels, guided by the finger up towards the prostate, the dilated portion of the urethra can be most easily opened in the middle line, and the knife, being drawn

forwards towards a catheter, introduced as far as it can be into the urethra anterior to the stricture, divides every obstruction to the immediate introduction of the instrument into the bladder. This opération will be found a very successful one, and, after a few weeks, the urethra will be restored to a very healthy state, requiring only the occasional introduction of bougies.

We shall close our observations with a case of cancer of the rectum, in which Dieffenbach attempted to extirpate the diseased parts. He first made a free incision at the anterior and posterior edge of the bowel; he seized the diseased parts with a vulsellum, and, having brought them towards the anus, removed them with scissors. We can only remark, that if this was a case of true cancer such a proceeding is worse than useless.

The perusal of this book will leave no doubt of Dieffenbach's great skill as an operator, and of the numerous original views and plans he has introduced in his peculiar department of science; but an impression is also left on the mind of the reader that he is deficient in carefully distinguishing those cases in which operation will be really beneficial, and that he excels more as a skilful manipulator than as a sound surgeon.

ART. XIII.

1. *A Treatise on the Diseases of Infants; founded on recent Clinical Observations and Investigations in Pathological Anatomy, made at the Hospice des Enfans-Trouvés; with a Dissertation on the Viability of the Child.* By C. M. BILLARD, M.D., &c. *With Notes*, by Dr. OLLIVIER, of Angers. Translated from the third French Edition, *with an Appendix*, by JAMES STEWART, M.D.—*London and New-York*, 1839. Large 8vo, pp. 620.
2. *A Practical Treatise on the Management and Diseases of Children.* By R. S. EVANSON, M. D., &c., and HENRY MAUNSELL, M.D., &c. Third edition, revised and enlarged.—*Dublin and London*, 1840. 8vo, pp. 498.

THE emphatical sentence of Morgagni may be applied with just as much propriety at the present day as it was by that great pathologist at the time when he wrote, "How vast and new is the space that is still open before us for the study of the diseases of young children." In this country there are few, or indeed no opportunities upon an extended scale for the exercise of talent and industry in the prosecution of this study. We have no public hospitals expressly for young children, and many causes conspire, especially the prejudices of parents and the meddling interference of nurses, to render any practical knowledge of the diseases of infants that can be derived from even the most extensive private practice, not a little uncertain to the practitioner himself, and but little profitable to science, in comparison with that which may be obtained in a public institution where we can be assured our treatment is not interfered with. "They manage these things better in France;" and it is perhaps only in Paris that the diseases of children may be studied to the greatest advantage. In 1828 the number of beds at the Hôpital des Enfans was nearly 600, and we believe it is now considerably increased. The establishment is appropriated to the treatment of sick children, from

two to fifteen years of age, of either sex, and whatever may be the disease under which they labour. In this admirable school for practical instruction, M. Billard held for some time a very important station, and there he laboured with almost unrivalled industry and talent to shed new light upon the general pathology and treatment of infantile diseases. The principal object of his work is to exhibit the peculiar characters of these, and to consider them in relation to the alterations which the organs have undergone. He passes each system successively in review, and describes the varieties of form and appearance of every organ, with reference to its healthy, abnormal, and pathological condition; carefully estimates the value and importance of the leading symptoms of disease, and then points out the method of treatment. The development of organs is briefly discussed, and only those congenital malformations are noticed which more or less disturb the function of organs. Neither fevers nor intestinal worms are treated of, because they are of rare occurrence in new-born and sucking infants: to which class the author confines his attention. The very general absence of all febrile reaction in young infants, when at the same time there exists various serious lesions, and the readiness, on the contrary, with which fever is excited by the slightest cause in those who are teething, impress on these two periods an important difference in the character of their diseases.

The work of Drs. Evanson and Maunsell, the first edition of which we noticed in our Third Volume, is an excellent compendium of the best practice of the day on the management and diseases of children; and, as a book of reference and instruction for students and young practitioners, we think it likely to be more useful than that of M. Billard, which must be regarded rather as a valuable pathological record than as a ready guide in the moment of doubt and difficulty. M. Billard's work, too, is almost exclusively confined to the consideration of infantile disease, while Drs. Evanson and Maunsell take the wider range of the whole period of childhood.

Before he enters upon the study of particular affections, M. Billard considers, in the first part of the work, the general phenomena which are presented upon examining the external condition of the child. The second part comprises the history of diseases developed both during intra-uterine life and after the period of birth.

It must be evident, although the fact is too frequently lost sight of, that a precise knowledge of the phenomena which are exhibited upon the examination of the child, and which it is necessary to consider in all diseases, such as the expressions of face, crying, circulation, &c., is indispensable. If we are familiar with these in a state of health, it will, of course, be much more easy for us to appreciate the modifications they undergo in disease.

The colour of the skin of new-born children equally deserves attention. Infants recently born are almost always of the same colour. Blood predominates in their tissues, and communicates to them its hue, and the face, body, and limbs are all strongly coloured. From the fifth to the eighth day after birth this hue diminishes, but sometimes continues longer. This red colour is purely accidental, and upon its disappearance is followed by other hues of various character. If it continues, it is not so intense as at first; it becomes of a violet hue, particularly in the hands

and feet. The alteration, however, is not always an evidence of health, for it often coexists with an œdematous swelling of the limbs. If the finger be applied to the skin of an infant the red colour disappears at this point, and it becomes yellowish: afterwards the blood returns by degrees in the capillaries from which the pressure had removed it, and the yellow tint is replaced by the previous red. Very often we observe, after the red colour has disappeared, that the skin exhibits a universal tint of yellow, and sometimes of a copper colour. By physicians in general, this appearance of the skin is erroneously thought to indicate an affection of the liver.

The phenomena which precede, accompany, and follow the separation of the funis are described in a very novel and instructive manner. The desiccation of the cord and the time of its separation from the abdomen differ much in different infants. The desiccation of the cord is altogether a physiological phenomenon. That part of the cord attached to the placenta does not dry like the portion attached to the abdomen of the child, but shrinks and decays like a dead substance, whilst the abdominal portion is not so affected. Here the desiccation ceases as soon as life is extinct: it either does not proceed in still-born children, or it is considerably retarded. Instead of dying and separating at the end of a few days, as is observed during life, the cord undergoes in the dead body a perfect decomposition, differing entirely from its normal desiccation. This fact, which it may be difficult to explain, should be borne in mind, as it is important in reference to legal medicine. If a fœtus be examined some time after birth, if the cord still remain attached, we ought to examine whether it exhibits the characters of a normal desiccation, or whether it is soft or in a state of putrefaction, like the general condition of the dead body; for, in the former case, the child could not have been still-born, but might have lived one or two days, since the desiccation, which only exists during life, had already commenced, while in the latter the infant was still-born, or had lived but a short time. Such is the importance of this fact, that M. Billard particularly calls attention to it, as it may, in conjunction with other circumstances, prove whether the child was born alive; as the principle can be laid down, that in every instance in which the cord is dried, flattened, twisted, and blackened upon the dead body of an infant, it has lived—at least one day; this condition never being produced on a dead body. It appears that the ordinary time at which the cord separates from the infant is the fourth or fifth day; but, as this rule is subject to many exceptions, it is not entitled to much confidence in a legal point of view, when we wish to determine the precise age of the infant.

Exfoliation of the epidermis does not occur until after birth. M. Billard knows no case in which it ever commenced before; and it is worthy of remark, that premature children never exhibit this phenomenon: some time must elapse, and the infant arrive at a certain age, before it occurs. Authors on legal medicine have endeavoured to draw certain inferences from the separation of the epidermis in relation to the age of the infant. Orfila, desirous of investigating the statements of Chaussier, Capuron, &c. upon this subject, made considerable researches with M. Thierry, and he concludes that the epidermic exfoliation exhibits at first a preparatory stage, next an elevation of the epidermis, and, lastly,

ration; and that the preparatory stage can be observed from the the eleventh day, the elevation of the epidermis on all parts of y from the twentieth to the thirtieth day, and the complete exfo- from the thirty-fifth to the fortieth day: these authors also state tain diseases retard or suspend this process. M. Billard has not le clearly to observe the preparatory stage mentioned by Orfila ierry. He found that, in eighty-six infants, the commencement xfoliation and its duration were very variable, and that from three lays appears to be the age at which the epidermic exfoliation is at test height. In forty-two of these infants it was not observed at ıot unfrequently occurs insensibly; the epidermis comes off in a ıd the different periods of exfoliation cannot be detected. The f the epidermic exfoliation in new-born children is susceptible of ictory explanation. The epidermis, until the period of birth, is in the liquor amnii; when exposed to the air it becomes suddenly nd loses the suppleness maintained by the fluid medium in which mersed during intra-uterine existence: hence results a cracking ling of the epidermis, and its final separation in the form of scales ler. It is difficult to establish any constant conformity between aration of the epidermis or that of the funis, and the age of the even the attempt to draw any general inferences from these phe- ı has been fruitless. It is certain, however, that the epidermic ion of young infants is a natural and healthy phenomenon.

very young infant, the means of expression are limited to the cry pearance of the face; and both require attention, that we may be l to distinguish a state of health from one of threatened or really ; disease. M. Billard enters minutely into these subjects, and out many hints that well deserve the notice of practitioners. It : worth while to observe that very young children rarely shed hen they cry: the secretion of the lacrymal gland is excited, as is own, immediately and sympathetically by sorrow; but are children y tender age under the influence of mental emotions? Is this secre- oduced by any other influence than the nervous excitement pro- ; from some moral cause; and are physical distresses, which ap- be the only kind endured by a being whose brain cannot as yet ıe ideas, and from which there appears to emanate no volition, e of acting on this gland? It is difficult to answer these ques- The lacyrmal gland at this period is perfectly developed; it re- ırteries and nerves, and anatomically resembles other glands, but 's flow while the young infant cries from sickness or pain. This physiologically curious, and is a remarkable example of the parti- nfluence of the nervous system on the functions of certain organs body. We must refer to the work itself for M. Billard's "ana- ›f the distinctions between the infantile cry of health and pain ase.

t to the cry, the expression of the face is one of the principal means ch the child manifests the sensations it experiences. In order to our knowledge of the symptoms of disease in children, M. Jadelot ›posed a *physiognomical semeiology*, which no doubt assists our ıs of the diseases of infants. M. Billard gives a slight sketch of lelot's doctrine, and describes minutely and accurately the diffe-

rent expressions of the infantile countenance with which the physician should be acquainted. Of this system of signs, the reader will find some account in our Second Volume, p. 356.

After a few brief remarks on the "state of the pulse in children and feebleness at birth," we come to the second part of the work. The first chapter, on diseases of the skin, contains no important addition to our previous knowledge of the subject, with the exception of the section on "Œdema, or induration of the cellular tissue of new-born children." This hard or indurated state is manifested by a swelling of the limbs or face, which are more or less coloured, and give firm resistance to the touch; the sensation, therefore, produced by touching gave rise to the above term. Anatomical examinations, however, have proved the vagueness of such expressions, and the denomination compact œdema has been proposed instead of induration. This title is more correct, inasmuch as in this disease there is no induration of the cellular tissue: it feels hard because it is distended with fluid, and undergoes no other change than that which results from mechanical distension. There always has been, and there still exists much difference of opinion, even among the best authorities, as to the nature of this disease. M. Billard's opinions are entitled to attention, as he has had numerous opportunities of investigating every circumstance connected with its origin and progress. He believes that the following truths have been established: 1st. The induration of the cellular tissue in young infants is simple œdema, analogous to the œdema of adults: it may be either local or general, and ought always to be distinguished from induration of the adipose tissue. 2d. It is more common in winter than in summer, and more frequent in young infants than in those of more advanced age; the predisposing causes are the natural feebleness of the child, and a state of general and congenital plethora: a superabundance of venous blood in the tissues, and a dry state of the skin before the exfoliation of the epidermis.—The immediate causes are, 1st, an obstruction in the course of the blood, resulting from its quantity in the blood-vessels; 2d, its engorgement in the cellular tissue, to which it furnishes too much materials for secretion; 3d, the action of external agents on the skin, which, without condensing the venous blood, as has been asserted, are yet capable of suspending the cutaneous transpiration, and thus favour the accumulation of serum in the cellular tissue: the sanguineous engorgement of the liver, lungs, and heart, the persistence or closure of the fetal openings, are not the exclusive and indispensable causes of this affection. 3d. When œdema is general, and the venous congestion is carried to a high degree, all parts, where there exists cellular tissue, undergo a disturbance in the functions which they discharge. Thus the glottis becoming œdematous at the same time that the lungs are the seat of congestion, the cry of the child is generally painful, acute, and smothered. The slowness of the circulation easily accounts for the coldness of the limbs, and the great debility; and thus may be explained all the symptoms described by authors. 4th. The therapeutic indications are, 1, to relieve, by suitable evacuations, the general plethora; 2, to excite the skin by irritating frictions, by the use of woollen garments next to the skin, and the adoption of all means proper to establish cutaneous transpiration. Vapour-baths are less useful than frictions and woollen to the skin: Billard has often

known these succeed. The respiration of a child during its continuance in the vapour-bath is painfully accelerated, and congestion or effusion on the lungs or brain has been known to follow its employment. Dr. Maunsell observes (p. 184), that no febrile condition accompanies this affection; wherein it differs from infantile erysipelas, which, in other respects, it much resembles. The child will not suck, is restless, and continually whines. As to the treatment, he states correctly that the earlier plans had for their object the application of heat by means of the warm and vapour bath, and wrapping the child in wool or cotton. "The lowness of temperature, however, is merely a symptom of the state of commencing asphyxia, and that state is not likely to be removed by the application of external heat alone. We must endeavour to lessen the venous congestion, and, if we succeed in doing so, the production of heat will proceed naturally. For this purpose we would recommend friction with warm flannel; the administration of an emetic of ipecacuan, for the purpose of removing mucus, and exciting respiration, and the internal use of stimulants, as warm wine-whey. We cannot put forward this plan as the result of experience, but we conceive it to be based upon a rational view of the pathology of the disease. Paletta recommends leeches to be applied to the œdematous parts, as a means of promoting circulation; but his plan has not suceeded in other hands." (Maunsell, p. 185.) Dr. Carswell makes some interesting remarks upon this disease in his article "Induration," in the Cyclopædia of Practical Medicine. One fact is worthy of notice. The great mortality of the Hospice des Enfans has been attributed to induration of the cellular tissue; this, M. Billard believes, is incorrect. There often exist at the same time affections of the brain, lungs, and intestines, much more serious than œdema, and much more fatal. Fifty children died in 1826 of œdema, or induration of the cellular tissue, without any serious lesions of other organs. In May and November the greatest number affected with induration were admitted, and all these patients died of affections of some important organ, and more frequently of the lungs. When œdema is local, or if it be general and yet not severe, it is not to be regarded as a fatal disease, nor will it become so until it is complicated with disease of some vital organ. Of all the phenomena accompanying œdema of infants, icterus is one of the most common. In seventy-seven infants affected with œdema, thirty were jaundiced, but no organic lesion was detected that could account for this difference. The whole subject of exanthematous diseases is more practically, and therefore more usefully considered by Dr. Maunsell than by M. Billard. Dr. Maunsell very properly corrects one statement of M. Billard with respect to the eruption of measles. "Billard describes the eruption of measles as not feeling elevated above the surface, which is decidedly contrary to the fact." (Maunsell, p. 382.)

The chapter on the diseases of the digestive apparatus contains much valuable information, especially as to the pathology of the various maladies that are described. We shall confine our notice to the section on gangrene of the mouth, in preference to giving a mere sketch of each division of this elaborate and instructive chapter. Gangrene of the mucous membrane of the mouth may occur in infants in various ways. It is not an unfrequent termination of aphthæ. When aphthæ become gangrenous,

their edges shrink and assume a burnt, torn, and flabby aspect; then a brown eschar often forms in the centre, which soon detaches itself, leaving a granulated surface of a vermilion colour. In place of an eschar, the centre of the ulcer sometimes gives off a creamy substance of a brown colour, and of a gangrenous odour. The surrounding parts tumefy, assume a violet aspect, become softer, and are easily depressed. A ropy saliva flows from the child's mouth. The face becomes pale, the patient remains drowsy, and sinks without having exhibited any febrile reaction or cerebral excitement. The pulse always remains very feeble, and the skin is remarkable for its paleness and insensibility. To these symptoms are often added vomiting, diarrhœa, distension of the abdomen, and sometimes hiccup and frequent eructations. This termination of aphthæ is extremely fatal, for it happens usually at a period when the child, wasted by the previous phlegmasia, affords no opportunity for appropriate treatment. As soon as the gangrene has formed it should be touched with slightly acidulated gum-water. Should this application effect no alteration in the aspect of the ulcer, sulphuric or muriatic acid must be used. In order to apply them in the easiest manner, a glass capillary tube may be used, immersing one end in the acid, and drawing up one or two drops, which are to be dropt on the surface of the ulcer with the end of the tube. After the application of these acids, and when the eschar is detached, the remaining gangrenous part must be touched with solid nitrate of silver sharpened at the point: for by using the acid again, it might touch the parts deprived of the eschar, which are in a state of extreme irritability. It is much easier to moderate and limit at will the action of nitrate of silver. These practical remarks will apply to all ulcers of the mouth which assume a gangrenous character. There is another kind of gangrene of the mouth, which does not follow any well-marked inflammation, but which appears to be caused by a particular alteration of the parietes of the mouth. This disease has long attracted the attention of the profession, but it is but recently that we have had any very satisfactory account of it in the works of Baron,* Guersent, Jadelot, and Isnard;† M. Billard gives the detailed account of several examples of it which occurred at the hospital. There are usually two well-marked stages of this disease: 1st. An œdematous circumscribed tumefaction, characterized by an oily aspect of the skin and by a central body more or less hard, on which there is sometimes an obscure red spot, either on the internal or external surface of the cheek: this is the first stage, and in young infants is not accompanied by fever or any symptom of reaction. 2d. This central part presents an eschar which usually forms from within, the mucous membrane becomes disorganized, the bones are laid bare, all the soft parts, even to the periosteum mortify and separate in shreds, at the same time that the mucous or bloody matter, mixed with the remains of the gums or sides of the mouth, flows out, exhaling a disgusting odour: this is the second stage. Gangrene of the mouth must not be confounded with malignant pustule, for, as Rayer has well observed, the gangrenous inflammation commences in the interior of the mouth, and from thence spreads to the skin. It is not proved to be contagious, and it is usually observed in one patient at a

* Mém. sur une Affection gangreneuse de la Bouche. Bulletin de la Faculté.
† Diss. sur une Affection gangreneuse particulière aux Enfans. Paris, 1818.

ime in an hospital, even when there are many other children in the ame ward. It appears difficult to explain the cause of this gangrene; he fact should be borne in mind that œdema and indolent tumefaction lways precede the formation of the eschar. M. Billard thinks it does ot proceed from inflammatory action, but that it is the result of an inolent engorgement, analogous to that which constitutes anasarca.

M. Billard attaches but little importance to any general treatment or this severe disease; the slowness and uncertainty of its action will ever compensate for the advantages of caustic applied directly to the eat of a disease, the progress of which is of so frightful a nature. The trength of the child should always be supported by nourishing diet, as qual parts of milk and broth, and "a few teaspoonsful of Malaga wine in the course of the day." Internal stimulants, however, must be cautiously employed, for notwithstanding the apparent feebleness of the hild, the intestinal canal is not unfrequently the seat of great irritation, or even inflammation. Dr. Evanson, treating of the same disease in hildren of a more advanced age, recommends (p. 222), in addition to he early application of muriatic acid, as the only efficient local application, the administration of bark, wine, and the mineral acids, with a nutritious diet.

It would be useless for us merely to enumerate the various maladies of he digestive apparatus of infants which M. Billard describes; but in justice to him we must say that he has given an admirable account of the whole subject, and that his opinions are instructively illustrated by the detail of the most striking cases that came under his notice.

Under the head of diseases of the respiratory apparatus, an equally minute account is given of the principal affections of the nasal fossæ, the larynx, trachea, and lungs. There is one condition of the larynx and trachea, which, without any lesion whatever of the mucous membrane, especially merits the attention of accoucheurs. We allude to the abundant secretion of mucus which in some infants obstructs these passages so as materially to impede the establishment of respiration. This affection is usually accompanied by a peculiar alteration of the cry, which is husky and incomplete. "It is probable," M. Billard says—we should say certain, "that this mucus is accumulated in the larynx and trachea before birth." The effects of this accumulation are generally trifling, and of short duration; sometimes, however, at the time of birth the respiration of the child is thus seriously impeded; and if the practitioner is ignorant of the fact, or careless respecting it, a state approaching to asphyxia may arise, which may be prevented by removing the abundant mucus by a feather introduced into the entrance of the larynx, where it usually adheres. M. Billard's observations upon croup are brief but interesting. Croup, it is well known, consists of an inflammation of the larynx and trachea, complicated with the rapid formation of a pellicular concretion spread over the walls of the larynx, which extends in some cases to the trachea and bronchi. The remote causes appear to be the same as those of laryngitis or bronchial catarrh; but it is difficult to explain in a satisfactory manner the immediate cause of the formation of the false membrane which occurs in this affection.* It

* We would especially recommend young practitioners to refer to Valentin's Recherches historiques et pratiques sur le Croup.—Rev.

is almost always during the prevalence of epidemic catarrh, or hooping-cough, that the croup is most rife; it precedes or accompanies one or the other of these phlegmasiæ, and is sometimes even a complication of them. M. Bretonneau has in vain attempted to separate the connexion existing between the catarrhal affections and croup, and to controvert the opinions that have been held for half a century by Home, Rosen, Michaelis, and supported by Jurine, Double, Vieusseux, Royer Collard, Blaud, Valentin, Bricheteau, and Desruelles.* A few writers, adopting M. Bretonneau's views, have endeavoured to prove with him that there is something specific in the nature of croup; but without admitting this the formation of the false membrane may, M. Billard thinks, be explained to a certain extent. He has discussed this subject in detail in another work,† and the following are, in brief, the reasons he adduces to show in what the peculiar nature of croup consists. 1st. There exists, as it were, but a degree between the thick, tenacious, filamentous mucus with which inflamed mucous membranes are covered, and the membranous exudation of croup. 2dly. The membrane of croup presents nearly the same chemical elements as this mucosity where fibrine predominates. The same analogy exists between the pellicular excretion of muguet and the mucosity of catarrhal affections; so that the puriform mucosity of catarrh, the false membrane of croup, and the excretion of muguet appear to be but alterations of the same secretion, and vary only in form, and the parts they occupy. 3dly. Before this membrane appears, the mucous membrane is always much inflamed, red, and gorged with blood; the subjacent tissue participates in this injection, and when the inflamed membrane is at the same time the seat of sanguineous exhalation, this exhalation is accompanied or followed by pellicular concretions, from which it is to be inferred that croup is a catarrhal phlegmasia, but that the blood destined to the secretion of mucus is, in the case under consideration, concentrated in greater abundance, or rendered plastic by inflammation, and imparts to the mucosity that part of its composition which concretes the quickest, that is, the fibrine; whence arise the striæ, pellicles, and white patches with which the mucous membranes, affected with muguet or croup, are covered. Children at the breast are much less subject to croup than those of a more advanced age; it is most prevalent from two to ten years of age. Young infants however are liable to pellicular inflammations of other mucous membranes, such as those of the mouth, œsophagus, and nasal fossæ, whilst the opposite condition exists in children of more mature age. Age, therefore, and the organic modifications which belong to it, and which can more easily be understood from their effects than by their physical appearances, seem to produce a difference which ought to be noted, though we are unable to explain it. "But, on the other hand, the readiness with which symptoms of suffocation arise when the slightest inflammation arises in the air-passages of young infants, renders the ordinary tracheal and laryngeal affections almost as dangerous as croup." We have seen many cases which proved the soundness of this statement, and which showed how careful and guarded should be our prognosis of,

* Traité Théorique et Pratique du Croup. Paris, 1824.
† Archives Générales, &c. December, 1826.

how narrowly we should watch every attack of laryngo-tracheal nmation in infants. We do not stop to notice the rather unsatis- ry description M. Billard gives of the symptoms and treatment of . He "pushes" calomel farther than most of his brethren, for he er employed calomel except in doses of 18 or 20 grains in twenty- hours." He always conjoins with calomel direct antiphlogistic s, as leeches to the larynx and trachea, and emollient drinks and il applications. The practice advised by Bretonneau is, in our on, very wisely deprecated, of opening the trachea and introducing sel or alum to destroy and remove the membranous pellicle. In one M. Billard did introduce a tube into the larynx covered with a solu- of alum, and the child died convulsed in five minutes.

Billard observes that death almost always suddenly terminates this ful disease, against which the resources of art are too often power-

Dr. Maunsell (p. 324) wisely abstains from so very fearful a iosis. He admits that croup is always a dangerous disease even r the most favorable circumstances; but it is also one which admits e use of decisive means, and is thereby remarkably within the con- f art. When we see a patient then at an early period of the disease ay hope to relieve him. Upon the much-disputed point too of spas- c croup as a distinct disease, Dr. Maunsell's observations exactly with our repeated experience:

Every affection of the larynx is subject to exacerbations, which partake of a spasmodic character; and a paroxysm of this nature may occur at an period of true croup, and destroy the patient before there has been any for any very important results of inflammation to be produced. Similar xysms have also been observed without previous or subsequent symptoms of nmation, and have subsided without active treatment, so giving rise to the n, that there existed no distinct form of spasmodic croup. There are, ver, no means of distinguishing between the two affections, (if two distinct tions exist,) beyond the degree of violence of the symptoms. Whenever, fore, we meet with the symptoms already enumerated, as indicating the of croup, we should be upon the alert; and as soon as any permanent diffi- of breathing sets in, we should forget all hypothesis of the spasmodic re of the disease, and treat it as an active inflammation, persuading our- s, like Dr. Kellie,* that there is truly no essential difference between them smodic and inflammatory croup) other than what arises from degrees of nce and the obvious circumstance of intermission and continuance." unsell, p. 324.)

From frequent experience at the Hospice des Enfans, M. Billard infers pneumonia of infants exhibits peculiar characters, in which it differs the same disease in adults. Instead of being an idiopathic affection, ing from irritation developed in the pulmonary tissue, the pneumonia oung infants is evidently the result of a stagnation of blood in their s. Under these circumstances the blood may be considered as a ign body, and it concurs in producing an alteration in the pulmonary ie with which it combines, and is identified with it so as to form atization of the lungs. It appears, therefore, that inflammation of lungs, which produces hepatization, arises in infants, in general, from e mechanical or physical cause, which is not the case in adults; be- s, the inflammation is usually very circumscribed, and is almost ys found limited to a point primarily engorged; and the pleura, which

* Letter in Cheyne's Pathology of the Larynx and Bronchia.

generally is inflamed with the lungs at a more advanced age, is no affected in young infants. The inflammation once developed, as in the adult, may give rise to various alterations of tissue, from simple hepatization to a great disorganization of parts. The details of several cases are given to illustrate these opinions.*

In the chapter on diseases of the "circulatory apparatus," M. Billard gives the result of his researches on the establishment of the independent circulation, which differs in many respects from the account usually given by other writers. He considers in succession the period at which the fœtal openings are obliterated, their mode of obliteration, and the physiological and pathological consequences which arise from these changes. In a number of children five days old the fœtal openings remained open and none of these children exhibited any peculiar symptoms which appeared to have their seat in the circulatory apparatus. On the eighth da the fœtal openings are usually obliterated, but even at this period the are sometimes found open. Even on the twelfth and fifteenth days th foramen ovale or ductus arteriosus may still be open without the existenc of any particular symptom. It is evident, then, that the fœtal openings are not obliterated immediately after birth, and that the period at whic the obliteration occurs is variable. The changes which take place aft birth in the vascular system occur in the following order: the umbilic arteries are first obliterated, then the umbilical vein, next the ductu arteriosus; and, lastly, the foramen ovale. The existence then of th fœtal openings for some days after birth ought not to be considered as a disease, since it is not uncommon to meet with it without any symptoms of disease at all. We can verify these statements of M. Billard by several dissections we have made. Cyanosis, or the "blue diseases of infants," is by no means the constant result of the persistence of the foramen ovale, nor of the passage of venous blood into the arterial system; there are many examples of malformation of the circulatory apparatus existing, of which we have ourselves two specimens, which might have produced the disease without its ever having appeared. Cyanosis may exist with or without a malformation of the heart, provided the blood, in passing through the lungs, does not undergo the vital and chemical modifications which ought to occur. If, notwithstanding the communication between the two auricles, cyanosis does not take place, it is because the blood passing through the lungs is in sufficient quantity, and sufficiently oxygenated to impart its oxygenation to the venous blood with which it is mixed. On the other hand, if the cavities of the heart are in a normal state, but the peculiar disposition of the lungs does not permit the oxygen of the air to transform the venous into arterial blood, cyanosis will be the result. Local or general cyanosis, however, in new-born children, is in most instances the effect of a sanguineous congestion about the heart and lungs, and the best method of relieving it is that recommended by Corvisart, to hold the child near the fire, and to rub the head and body gently with hot cloths. This may require to be persevered in for some time.

In the chapter on diseases of the cerebro-spinal apparatus, M. Billard treats on congestions and softening of the brain, and inflammation of the brain and spinal cord, of each of which maladies he adduces instructive cases. The description he gives of the progress of purulent ophthalmia in infants, and the destructive consequences that too frequently arise in

* See M. Valleix's observations on this affection, B. and F. Med. Rev. Vol. VII. p. 77.

the structure of the eye from carelessness in parents, or negligent treatment of the practitioner, is not merely more minute, but certainly more instructive than Dr. Maunsell's account of the same serious disease. The treatment is, in our opinion, much too briefly dismissed by both authors. M. Billard contents himself with giving a brief extract from Mr. Lawrence's work; and Dr. Maunsell's remarks upon the treatment are not, we think, sufficiently precise for the information of students and young practitioners upon a subject, which, if not well understood, involves the dreadful calamity of loss of sight. In the general principle of treatment recommended in both works we perfectly agree. Bleeding by leeches, we quite coincide with Dr. Maunsell in thinking very rarely necessary; or the early use of astringent or stimulant collyria in preference to warm emollient applications. But the practitioner will very imperfectly perform his duty if he merely prescribes the proper application. He must use it himself at first, and carefully instruct the nurse how she is to employ it. Upon this very important point nothing is said in either of the works before us; and from want of due attention to it, we have known in more than one instance complete destruction of the eyes to follow, which we have no doubt might have been averted if due caution had been exercised. Again, too, we find no recommendation for the application of the red precipitate ointment to the edges of the eyelids at night, for the purpose of preventing their adhesion, which we think essentially necessary. In our former notice of Drs. Evanson's and Maunsell's work,* we gave it as our opinion that the latter gentleman "underrated the value of chalybeate medicines" in the treatment of scrofula. In the present edition an interesting note is added at p. 468, which seems to confirm our opinion. In a letter lately addressed to the Royal Academy of Medicine, of Paris, by M. Coster, the virtues of iron, as a preventive of the development of scrofula, are highly extolled. Two years ago M. C. placed a number of dogs, rabbits, &c., in the circumstances most favorable to the development of the scrofulous diathesis. Thus many of the unfortunate animals were shut up in dungeons, without light, incapable of moving, and exposed to a moist cold by means of wet sponges which were hung up in the cages. Some of the animals placed in these conditions were fed on their ordinary diet, others were fed with ferruginous bread, containing half an ounce of carbonate of iron to the pound. All the former became ill, the greater part tuberculous, but not one of those fed on bread containing iron presented a trace of tubercles.†

A medico-legal dissertation on viability with reference to the pathology of new-born children terminates M. Billard's work. The Appendix by Dr. Stewart contains many interesting illustrations of and additions to the opinions of M. Billard upon various subjects of practical importance.

This work is decidedly a very valuable contribution to our knowledge of the diseases of infants. No other writer, foreign or English, had better opportunities of studying the pathology of infantile disease than the much-lamented and highly-esteemed Billard; and with strict justice we may add, that none could have turned them to better account. The translator has performed his task in a very correct and creditable manner, and his Appendix forms a very important addition to the original work.

* British and For. Med. Rev., vol. III. p. 446. † Bull. de l'Acad., Jan. 31, 1840.

We have before expressed the high opinion we had formed of the great ability of Drs. Evanson's and Maunsell's much required work; and we need only add, that the present edition is still more deserving of our commendation. It would be unjust to these gentlemen not to observe that their work, although necessarily and properly containing much valuable matter from preceding writers, affords abundant proof of their personal experience and practical judgment.

ART. XIV.

The Anatomy of the Arteries of the Human Body; with its Applications to Pathology and Operative Surgery; in Lithographic Drawings: with Practical Commentaries. By RICHARD QUAIN, Professor of Anatomy in the University College. *The Delineations* by JOSEPH MACLISE, Esq., Surgeon.—*London*, 1840. Parts I-II. 8vo, pp. 90. *Super royal Folio, Ten Plates.*

THIS is a work excellent both in its conception and execution, and meriting the warmest patronage of the profession. Its plan and object cannot be better stated than in the author's own words:

"Several years have elapsed since I became impressed with the belief that the difficulties which have often occurred in the performance of those surgical operations in which the larger arteries are concerned, have arisen in great part from want of sufficient acquaintance with the differences in anatomical disposition to which these vessels are subject—not merely those deviations in the origin of large branches, which are usually named varieties, but other peculiarities of various kinds which are liable to occur, such as those which affect the length, position, or direction of the vessels. Under that impression I was led to observe these circumstances more closely, and finally determined to obtain a record of the condition, whatever it might be, of the more important vessels in a considerable number of cases—a record to be made especially with a view to points bearing on practical surgery.

"With this view, I examined with more or less attention the bodies which were received during a series of years for the study of anatomy into the School of Medicine in University College. These bodies, to the number of 930, were with rare exceptions so inspected with reference to the subject of my enquiries, that anything very unusual could not escape notice; and, in order to insure accuracy, when other occupations allowed, the arteries were carefully examined and their condition noted at the time, attention being always particularly directed to those vessels and to the points in their history which seemed to be of importance in the practice of surgery. This detailed investigation was continued until the number of cases observed appeared such as would afford grounds for reasonable conclusions both as to the limits of the deviations from the ordinary standard, and as to the relative frequency of their occurrence. At the same time that the observations thus made were written down, drawings were obtained of all the important peculiarities which presented themselves, and when it was practicable the preparations were preserved. The varieties in the arrangement of the blood-vessels thus noted grew, as may be supposed, to be very numerous; but instead of difficulties multiplying with the number of observations, it was usually found that as the facts accumulated, the transition fr[om] one state to a very different one ceased to be abrupt or without method, others from time to time interposed which served to link them together.

"Originally these observations were intended exclusively for the benefit of [my] class; but as their number and connexion seemed likely to render them [more] extensively useful, I resolved to publish them. On examining with a vie[w to] publication the materials which I had collected, it became obvious that [their] utility would be very limited, unless as a part of a full history of the ar[teries] with adequate delineations. In consequence, a series of drawings, showi[ng]

arteries according to their usual arrangement, has been prepared, and to these are appended the observations previously alluded to. The work has thus grown under my hands, and has gradually assumed its present form. To carry out my views as to the delineations, I obtained the assistance of my friend and former pupil, Mr. Joseph Maclise. In reference to that gentleman's labours, it may be allowed me to say, that while I have had the co-operation of an anatomist and surgeon, obviously a great advantage, the drawings will, I believe, be found not to have lost in spirit or effect.....

"In the present work, the arteries are in the first place represented according to their most frequent arrangement, without the accompanying veins and nerves. 2dly. They are shown in connexion with the larger veins and nerves. 3dly. The deviations from that which has been taken as the standard, because the most frequent condition of the arteries are illustrated in a series of sketches. Into this part of the subject as much of arrangement or classification has been introduced as my information admits. 4thly. Such peculiarities in the veins, and occasionally of the nerves and muscles, as appeared likely to be of importance in surgical operations are represented on a reduced scale. 5thly. At the end of the publication will be given illustrations of the state of the arteries after the operations for aneurism.

"The letter-press, besides an explanation of the drawings and remarks on them, will contain: 1. A series of Tables showing, in a considerable number of cases, the condition of the arteries as to some of the points of most importance in their anatomy. 2. Practical Commentaries; which will consist for the most part of inferences from the facts previously set forth, and their application in performing surgical operations." (Preface, pp. 5, 6, 8.)

The only previous works on the same subject with which Professor Quain's publication can be compared, are Haller's *Icones Anatomicæ*, Scarpa's plates in the treatise *Sull' Aneurisma*, and Tiedemann's *Tabulæ Arteriarum*; and it is no small praise to say that it can well bear the comparison; indeed, it is but justice to state that while it is as original and as accurate as the best of these works, it is far superior to them all in its relation to practical surgery. Judging from the specimens before us, we believe that it will not only give us a much more accurate and more complete anatomy of the whole arterial system than we already possessed, but that it will give all the most precise information relating to every point connected with the blood-vessels and nerves, which it most imports the practical surgeon to have. The plates do the highest credit to Mr. Maclise as works of art. In addition to their beauty and accuracy, they have the great advantage of representing the objects of their natural magnitude, a point of first-rate importance in surgical anatomy. Most of them are also coloured.

Considering these circumstances, it is surprising that the work can be offered to purchasers at so low a rate as twelve shillings per part, (each containing five plates;) but we presume, the author, like his distinguished predecessors in the same path, had higher objects in publishing it than mere emolument. We, however, consider it our duty most strongly to recommend the work to our readers for its intrinsic merits, and with a view to their own advantage; and we shall feel more regret for the loss of credit to the profession, than for any pecuniary loss that may be sustained by the author, should it fail to obtain that ample encouragement and wide circulation which it so richly merits. At any rate, should the work proceed as it has commenced, and of this we have no doubt, it will constitute an enduring memorial of the merits of its authors, ranging in the same class with the productions of William Hunter and Joseph Swan, of which England has such just reason to be proud.

ART. XV.

Die Krankheiten des Foetus. Von Dr. J. GRAETZER.—*Breslau*, 18 8vo, pp. 286.

The Diseases of the Fœtus. By Dr. J. GRAETZER.—*Breslau*, 1837.

IN the year 1702 the first treatise on diseases of the fœtus was pu lished under the auspices of the celebrated Hoffmann; and there ha appeared since that time many contributors to this department of me cal science. Few, however, have done more than record one or t isolated observations in some journal or other, where they were hidd from the notice of subsequent students rather than made available the furtherance of their enquiries. The collection of these observati and their suitable arrangement was the laborious task that M. Graet undertook, and which he has executed with much diligence and sou judgment. He makes no pretensions, indeed, to have fulfilled ev object that might have appeared desirable; nor does he profess to ha furnished a complete systematic treatise on intra-uterine pathology; a attempt at which, in the present state of our knowledge, must have be fruitless. It is but right, however, that we should allow M. Graetzer state for himself what has been his aim in this work, and by what prin ples he has been guided in its execution.

"The object of a scientific pathology of the fœtus," says M. Graetzer, not merely to state with accuracy the diseases to which the fœtus is liable, also to show how each disease is modified by the different stages of developm of the embryo. Whenever this is accomplished we shall have obtained tr worthy data from which to deduce the real causes of these affections. In sp of the labour of nearly two centuries, however, the execution remains far hind the idea, and makes even now but small advances towards its realizati Many diseases indeed have been observed in the fœtus, and the greater num of those persons who have investigated the subject have not confined themsel to a bare description of what they saw, but have almost invariably attempt to attach to the morbid conditions names such as are applied to diseases of t adult. This, however, has been done apparently without the slightest suspici of the important pathological questions which are here presented for solutio It would, then, be unwise to attempt now to arrange our materials accordi to this idea, for our observations do not afford premises from which we mig deduce definite results. Our present object, therefore, has been to furnish complete critical history of all the diseases of the fœtus: a work which has n hitherto been satisfactorily executed." (pp. 4, 5.)

In the arrangement adopted by M. Graetzer two grand classes general and local diseases are established. The following extract fro his scheme will best exhibit the numerous topics treated of in the wor as well as the order in which they are discussed.

"I. General diseases.

a. Of an acute character.

1. Fever.

α. Unattended with any eruption on the surface of the body: as intermitte fever.

β. With an eruption: as the acute exanthemata.

2. Inflammations.

b. Chronic diseases.

1. Dyscrasiæ affecting especially internal organs: as hypertrophy, atrop syphilis, dropsy, &c.

2. Dyscrasiæ especially affecting the skin: as elephantiasis, and some other cutaneous affections.

II. Local diseases.
a. Of the organic fluids.
1. Of the blood: scurvy.
2. Of the lymph: scrofula.
b. Organs of vegetative life.
1. Intestinal canal.
2. Its glandular appendages," &c. (pp. 7-8.)

Some of these diseases might perhaps have been better classed under other heads, but M. Graetzer proposes this arrangement merely as a temporary one until increased knowledge may enable us to diminish the class of general diseases, and to refer each affection to the organ whence it derives its origin.

After giving a good sketch of the literature of his subject, the author begins the consideration of the general diseases of the fœtus, with some observations on intermittent fever. (§ 8.) This disease, how problematical soever its occurrence during intra-uterine life may appear, has certainly been observed a very short time after birth. In addition to the cases related by M. Graetzer, we may refer to two instances of congenital intermittent fever recorded in Von Siebold's Journal, (bd. xvii. p. 318.)

Much research is displayed by the author (§§ 9-15) in his remarks upon the smallpox, measles, pemphigus, and petechiæ, in the fœtus; all of which he classes, though with some hesitation, among the exanthemata. The following conclusions may be drawn from the author's observations with reference to variola in the fœtus. 1st, That smallpox may be communicated by the mother to the fœtus. 2d, That the mother may transmit the smallpox to the fœtus without herself suffering from the disease. 3d, That there is no well authenticated instance in which the mother has suffered smallpox and the child has escaped. 4th, That where both mother and child have been attacked by the disease the latter has not been affected until a considerable time after the former. Thus, in the case which Mr. Lynn relates, a pregnant female was attacked by smallpox: on the eleventh day of the disease the pustules dried up; and on the twenty-second day she was delivered of a child. The whole body of the child was covered with the eruption of smallpox, and three days after birth the pustules were distended with matter. From this it would appear that the first symptoms in the child could not have occurred sooner than the sixth day after the pustules in the mother had dried up. The case related by Mr. Hunter, and others observed by different persons, bear out the same conclusion.

Under the head Inflammations (§§ 17-19), the author details cases of spontaneous amputation of the limbs in the fœtus, with the various explanations of the accident which have been proposed.

Among the Dyscrasiæ principally affecting internal organs, hypertrophy, atrophy, syphilis, helminthiasis, lithiasis, hydrops, and icterus are noticed in succession. (§§ 20-30.) By hypertrophy the author understands preternatural development of fat, and might surely have found some more appropriate word to express his meaning. The observations on atrophy are short and unsatisfactory; but M. Graetzer has treated of syphilis at much length (pp. 85-107), and has presented us with a bet-

ter summary of all facts relating to this interesting subject than we have ever met with elsewhere.

The existence of stone in the kidney or bladder of the fœtus is undoubtedly an exceedingly rare occurrence, though several instances of it have been collected by the diligence of M. Graetzer. (§ 27.) The first case on record, which he quotes from Hoffmann, was that of a daughter of the Princess Moritz of Zeis. The princess, while suffering from stone in the kidney, gave birth to a daughter, who very soon began to suffer from difficulty in voiding her urine. When only three weeks old she died; and, on a post-mortem examination, a stone the size of a peach was found in her bladder. Geyer, Löscke, Nicolai, Feiler, and Prael have likewise related somewhat similar cases. Prael found many calculi in both kidneys of a female child who died when six months old. Most of them were hard, as large as a grain of millet, and contained phosphate of lime, uric acid, and albumen. The child when born was stout and healthy, but suffered from birth from obstinate constipation, and made water but very seldom. Her urine was dark-coloured, and had a very strong urinous smell. Diarrhœa afterwards supervened, and the child died in convulsions. Orfila has observed two instances of stone in the fœtal bladder; and in both of these cases calculi were also present in the pelvis of the kidneys. The bladder was evidently inflamed, and the tissue of the kidneys seemed also to participate in the same condition, if one might judge from its sanguineous congestion, its colour, and extreme friability.

The jaundice of new-born infants is an affection familiar to all who are conversant with the diseases of children; but English writers, with the exception of Underwood, who alludes to it incredulously, appear to be unacquainted with its occurrence in utero. M. Graetzer has applied his usual diligence in the illustration of this subject (§§ 29-30), and has likewise detailed fully M. Lobstein's observations on the disease which he calls Kirrhonosis, and which he distinguishes from ordinary icterus.

M. Lobstein understands by the name Kirrhonosis a disease in which there exists a deep yellow colour of the serous membranes and of the substance of the nerves. It resembles an internal jaundice of the peritoneum, pleura, pericardium, and arachnoid; and differs from ordinary icterus in the circumstance that the subcutaneous cellular tissue and that which penetrates into the parenchyma of the organs, as well as the substance of the organs themselves, are quite free from this yellow colour. These observations, which M. Lobstein first made on two five-months-old fœtuses, he repeated on others, and found that the substance of the nerves was tinged; and that neither washing nor maceration in water or spirit could remove the yellow colour of the serous membranes. We are ignorant of the causes of this affection: it appears to differ from the ordinary icterus of new-born infants only in its situation; but, as M. Andral remarks with truth, it is by no means certain that even there the yellow colour is produced by bile.

The second part of the book, which treats of local affections, opens with a consideration of scurvy and scrofula: the former being regarded as a disease of the blood, the latter of the lymph. (§§ 34-5.) The interesting subject of scrofula and tubercle is, however, treated in a far too cursory manner. Aphtha, inflammation of the œsophagus, stomach, and peritoneum, and umbilical hernia, follow among the diseases of the organs

of vegetative life. (§§ 36-42.) The observations on peritonitis are few, and add nothing to what has been so well done by Dr. Simpson in his valuable papers in the Edinburgh Journal. Diseases of the generative organs occupy but a short space. (§§ 46-9.) The author notices congenital inguinal hernia in the male; but the occurrence of hernia of the fallopian tube and ovary in the female seems to have escaped his observation. Notices of this variety of hernia may be found in Deneux, (Recherches sur la Hernie de l'Ovaire, Paris, 1813;) Meckel, (Handbuch der pathologischen Anatomie, bd. ii. p. 429;) Billard, (Traité des Maladies des Enfans Observat. 57, p. 474, and Atlas, Pl. x.;) and Busch, (Neue Zeitschrift für Geburtskunde, bd. viii. heft 2, p. 272.)

Diseases of the circulatory and respiratory systems come next under review. (§§ 50-4.) It is here curious to observe how frequently the lungs which during fœtal life have no distinct function to perform are the seat of disease. These diseases too are various in kind and differ in the part of the organ which they affect. Inflammation of the pleura, terminating in sero-purulent effusion or in the formation of adhesions, is by no means an unusual occurrence. Cruveilhier has noticed the frequency of inflammation of the lungs in the fœtus, and the slight influence upon its nutrition which the disease appears to exert. Tubercles in the lungs are not infrequent, but it is not usual for them to have passed the crude state during intra-uterine life: instances to the contrary, however, are related by Husson and Cruveilhier; and in one instance Lobstein found the lung to contain calcareous concretions. M. Graetzer does not notice tubercular degeneration of the bronchial glands, although it is more frequently met with both in the fœtus and the infant than tubercle in the lung.

In investigating the diseases of the locomotive organs in the fœtus, M. Graetzer treats first of rhachitis (§§ 55-6), with his usual erudition. We pass over his observations on this subject, however, in order to notice his remarks on fracture of the bones occurring before birth (§§ 58-60), an accident of considerable importance in a medico-legal point of view. Eighteen cases of the accident are detailed by M. Graetzer, of which the following, quoted by him from Pr. Carus, is one of the most remarkable:

"A healthy woman, aged twenty-five, fell from a ladder upon her abdomen when in the sixth month of her pregnancy. At the moment of the accident the child's movements became more violent than ordinary; but afterwards they grew weaker, and at the end of her pregnancy she was delivered, after a natural labour completed without manual interference, of a puny and emaciated child, which gave but feeble signs of life. On the right leg of the child was a wound three fourths of an inch in length, which ran transversely from the outer to the inner ancle, and had divided the skin and muscular substance; and the tibia itself was broken at its lower extremity in such a manner that the epiphysis was detached. The lips of the wound were pale, bloodless, and flaccid. The epiphysis was completely detached from the lower end of the tibia which protruded from the wound, was directed outwards, had lost its periosteum, and presented an unhealthy appearance. Reduction was attempted; but the attempt was not persevered in, because the edges of the wound were affected with sphacelus, and necrosis of the bone was evidently advancing. The disease spread and the child died on the thirteenth day. Carus regards this case as a proof that affections may be borne during intra-uterine life, which become speedily fatal after birth. He looks on it likewise as important in a medico-legal point of view; since, had any manual interference been resorted to in the progress of the labour, the

suspicion of injury having been inflicted on the child by awkwardness or carelessness might easily have arisen." (pp. 198-9.)

Other cases are related in which fractures in the fœtus have been found completely healed. D'Outrepont mentions having found in a stillborn child the right thigh misshapen and a hard prominence projecting from the middle of its shaft. This induced him to examine the other limbs, when he found similar prominences on both fore-arms and on the left collar bone, and in those situations there was an evident deposition of callus. It seemed, indeed, that this callus had been recently formed, since the union of the fracture of one of the fore-arms was not quite firm.

Injuries to the fœtal skull in its passage through the pelvis naturally arrange themselves here, and accordingly the author treats of them in the next two sections, (§§ 61-2.) He next gives an abstract of Dupuytren's valuable paper on spontaneous luxation of the femur; and he also establishes satisfactorily that the subject had not been altogether unnoticed before the time of that celebrated surgeon. He even quotes (p. 222) a passage from Hippocrates (De Articul., sect. vi., Nos. 26-9), in which this affection is clearly described though not distinguished from hip-joint disease occurring in infancy.

Cephalæmatoma, to an examination of which M. Graetzer devotes §§ 67-8, is an affection which in England has been but little investigated, having been confounded with the ordinary caput succedaneum, and regarded merely as the result of pressure on the head in its passage through the pelvis. This notion, however, was adopted without a due examination of the subject, for the ordinary tumour of the scalp differs greatly from that effusion of blood between the cranial aponeurosis and the skull or between the bone and its pericranium, to which the name cephalæmatoma is applied. Moreover, the two affections have been known to coexist and to occupy different regions of the head; and cephalæmatoma has been observed in cases where the head met with no difficulty in its passage through the pelvis. The investigations of Michaelis, Paletta, and others have shown that it is often connected with a morbid state of the cranial bones; but this was asserted by some observers to be merely an occasional and accidental consequence of the extravasation of blood. This opinion was supported by Professor Naegele, and Graetzer is in doubt to which side of the question to incline. Since the publication of M. Graetzer's work, however, the subject has been elucidated by the labours of M. Valleix in France, and of M. Burchard in Germany, from which it appears that the cranial bones are implicated in every instance of cephalæmatoma. (See our notice of these works, Vol. VII., pp. 74-87; See also Vol. I., p. 182.)

Cataract is the only affection of the organs of the senses noticed by M. Graetzer; and a few observations on diseases of the cerebro-spinal system (§§77-80), followed by a general recapitulation, conclude the work.

The above notice of the principal contents of this book may convey some notion of the labour and research which it displays, and which render it, in spite of all defects in its arrangement, indispensable to every one engaged in the study of intra-uterine pathology. We trust that M. Graetzer will follow out his original plan, and furnish us, in a second volume, with a view of the diseases of the placenta and other parts of the ovum.

PART SECOND.

Bibliographical Notices.

ART. I.—*The Pathology and Diagnosis of Diseases of the Chest, comprising a Rational Exposition of their Signs; with an Appendix, containing various opinions and experiments on the Motions and Sounds of the Heart and on the Bronchi.* By CHARLES J. B. WILLIAMS, M.D. F.R.S., Professor of Medicine in University College, &c. *Fourth Edition, much enlarged and illustrated by Plates and Tables.—London,* 1840. 8vo, pp. 331.

THE number of editions this book has gone through being sufficiently indicative of its merits, we might be content to let it pass unnoticed were it not for the particular importance of the subject of which it treats, and the admirable manner in which this is handled by the author. We are sorry to observe that the practice of auscultation, notwithstanding the many excellent teachers of it in the metropolis, is still very imperfectly studied in our schools, and, consequently, that a large proportion of the junior members of the profession enter into practice incapacitated for the most effective treatment of a large proportion of the diseases they meet with. It is for this reason we have always been anxious to employ our pages in promoting the cause of the physical diagnosis of chest-diseases; and we now feel it no less a duty than a pleasure to recommend to the notice of our younger readers the manual of Dr. Williams now before us. It is, without question, the very best of the kind that exists in any language; and it is so full of well-digested and well-arranged knowledge, that there are few practised auscultators who will not benefit by its perusal; to the student and young auscultator it is indispensable.

The work is divided into three parts. The first contains two chapters, one devoted to the Physical Examination of the Chest, the other to the Examination of the Chest through the vital properties or functions of its organs: the second part treats of the Pathology and Physical Signs of the Diseases of the Bronchi, Pleura, and Lung: and the third is devoted to the Pathology and Diagnosis of Diseases of the Heart and Great Vessels. This last part is divided into five chapters, treating, respectively, of the physical examination of the heart; of functional diseases of the heart; of inflammatory diseases of the heart; of structural diseases of the heart; of diseases of the great vessels. The Appendix contains several Reports, formerly published, on the Motions and Sounds of the Heart, and a new "Report (by the Author) of Experiments on the Contractility and Sensibility of the Lungs and Air-tubes," read at the meeting of the British Association in 1840.

We regret extremely that the press of other matter prevents us from making any further extracts from this volume than the brief summary given by the author of the results of his experiments on the contractility

of the lungs and air-tubes, contained in the last memoir. The experiments themselves are very interesting, and the enquiry one of much practical importance.

"I trust that many results of the preceding experiments are sufficiently evident without much further comment. Almost all of them prove that the air-tubes are possessed of irritable contractility, excitable by electric, chemical, and mechanical stimuli. The contractibility resembles that of the intestines, or of the arteries more than that of voluntary muscles or of the œsophagus, the contractions and relaxations being gradual, not sudden. They are, however, much less tardy than those of the arteries. The irritability of the contractile bronchial fibres is speedily exhausted by continued stimulation, and may be in some degree restored by rest, even in the lung removed from the body for an hour or more. The contractility of these fibres seems much influenced, in case of sudden death, by the mode of death, being for a short time suspended after death caused by a blow on the back of the neck, and sometimes by death caused by pithing, not by hemorrhage. Several vegetable poisons destroy or impair this contractility. Extracts of stramonium and belladonna produced this effect most completely. Strychnia, extract of conium, and bimeconate morphia, also, to a great degree. Hydrocyanic acid, on the other hand, did not impair it at all. The action of these poisons on the bronchial fibres does not correspond with that on other contractile tissue, such as the heart and arteries, œsophagus and intestines, and the voluntary muscles; these in many cases having retained their irritability when that of the bronchi had been destroyed. It is possible that some medicinal agents (strychnia for example) may impair the contractility of the bronchial fibres by fixing them in a state of tonic spasm. The preceding experiments do not determine this point. The different state of the trachea, in some contracted, so that the ends of the cartilaginous rings overlap; in others expanded, so that they do not form three fourths of the caliber of the tube, seems to countenance this supposition, and deserves further investigation. The bronchial fibres seem not to be excitable through the nerves of the lungs; for mechanical irritation of these nerves produced no effect on them, and passing the electric current through the nerves to the lungs was much less effectual in causing their contractions than when the current was passed through the trachea." (pp. 330-1.)

ART. II.—*Remarks on the Surgical Practice of Paris. Illustrated by Cases. Being a Thesis to which a gold medal was assigned by the Senatus Academicus of the Edinburgh University at the Graduation of* 1840. By W. O. MARKHAM, M.D.—*London,* 1840. 8vo, pp. 114.

THIS work appears to be little more than a series of scraps selected from the clinical lectures of various Parisian professors, and put together with little order or arrangement. Did we feel that the opinions of the author were worthy of serious consideration we should go through the work and criticise the treatment said to be practised in the hospitals of Paris. But as this is not the case, we merely select a few extracts, on points of some interest, which appear to contain rather statements of fact than matter of opinion. We commence with a method of treating abscess, said to be pursued by M. Roux, which appears most unwarrantably severe.

"M. Roux has a treatment of some kinds of abscesses which, I should imagine, is quite peculiar to himself. His method is to extirpate the whole of it or all of it that he can conveniently. Thus, where an abscess has been situated on

ie knee, in front of the patella, and caused by a fall, I have seen this gentleman ake an incision on either side of the patella, in the axis of the limb, then join ese two by a cross incision over the middle of the patella, and dissect up and wn, and cut off the flaps above and below this cross incision; also in a scrolous abscess of a gland in the axilla. I have seen him enlarge the free incisions ready made in a crucial sense, dissect back the four angular flaps, and then it them each severally off." (p. 10.)

According to our author, the French hospital surgeons occasionally ommit gross blunders. We observed one or two instances in the work hich will appear startling to English practitioners. For instance, Dr. larkham says he has seen M. Blandin "thrust the trocar into the tescle in a small hydrocele, and where, by the aid of a light, the testicle ould be distinctly seen lying at the back of the scrotum, on withdrawing ie trocar, no liquid escaped, and examination, with a light, showed ie trocar sticking in the testicle." (p. 19.) M. Ricord observed that nce, in a great hurry, he sent the trocar right through the testicle of a arber. (*Ib.*) M. Ricord once injected a quantity of wine into the petoneum without a shade of a bad result, and he mentioned that he reembered M. Richerand having done the same thing. (p. 22.) M. icord said that he had seen Dupuytren and his own predecessor at the lôpital du Midi, cut off the glans penis as well as the prepuce, in opeating for phymosis by circumcision." (p. 76.) Some dreadful accounts re also given of the results of M. Louvrier's operations for anchylosis, ut Dr. Markham says he has seen several cases of anchylosis of the nee admirably treated by M. Lisfranc, by means of a gradually extendig apparatus. We are told that "the treatment of nearly all kinds of ractures by the starch bandage is now almost generally adopted by the 'arisian surgeons," and that "according to my judgment it is one of he greatest acquisitions of modern surgery." (p. 70.) Autoplastic opeations appear to be boldly practised. The following case is related:

"M. Blandin operated on a man who, in his youth, had had a malignant pusule beneath the left eye, which had destroyed the whole of the lower eyelid and portion of the face below it; of course the eye always remained in great part ncovered, and when he came to the Hôtel Dieu, there was, as might be exected, much inflammation in the conjunctiva, and two or three small ulcers on he cornea. M. Blandin dissected a portion of healthy skin from the temporal egion, commencing just below the outer angle of the eye, and continued the lissection upwards as high as necessary; he then turned round the portion nd united it to the surface freshly denuded below the eye. The operation sucreded perfectly; it enabled the patient to close his eye, and removed all the deormity which existed before." (pp. 79-80.)

The only other remarks we think worthy of extraction are on a subect now beginning to excite considerable attention, that of animate ontagion.

"The presence of animalcules in the discharge of chancres has been deternined by Dr. Donné, and by him supposed to be the veritable specific virus— species of vibrio (vibrio lineola of Müller), has been observed by him in ulers of the gland, prepuce, vulva, and vagina (and the same also in the pustules f inoculation;) myriads appear to exist in a little drop. Another and peculiar pecies has been determined in the secretion of the vagina. Dr. Donné is led o suppose that the cause of chancres and gonorrhœa are these vibriones, and ence that their cause is identical; but no vibrio has yet been seen in the disharge of gonorrhœa." (p. 97.)

In concluding our notice of this book, we are constrained to state that

the fact of the gold medal of the University of Edinburgh being assigne to it says very little for the merit of rival essayists. The author in h own preface gives a very correct character of his work, stating that it "at best but a partial view of the matter, written desultorily, without an distinct purport." Under these circumstances it may be questione whether its publication is not something more than injudicious.

ART. III.—*Demonstrations of Anatomy ; being a Guide to the Disse tion of the Human Body.* By GEORGE VINER ELLIS, one of t Demonstrators of Anatomy in University College.—*London,* 184 8vo, pp. 620.

THIS is a novel and appropriate title to a book intended expressly a guide in the dissecting-room. But after the appearance of so ma "guides" and "dissectors," we had hardly hoped to find anything ne in the plan or execution of the one now before us. In this we hav been agreeably mistaken. In the present volume the information is solid and practical; there are none of those panderings to idleness in the shape of cuts and diagrams, which, however they may be tolerated or even be serviceable in works of mere descriptive anatomy, we think are quite unnecessary in a demonstrative work like the present, which is intended to be used with the objects described before the eyes of the student. We cannot better or more concisely indicate the plan of Mr. Ellis's book, as compared with that of the "dissectors" usually employed, than by quoting his own words:

"Anatomy may be studied in two different methods, the one demonstrative and the other descriptive: the former treats of the parts only as they are exposed in dissection, and of their relative position; the latter gives minute details of objects unseen, describing the different structures without reference to the order in which they appear, and has been very generally employed in the numerous works of instruction in practical anatomy; but every one acquainted with the difficulties to be overcome by the young dissector will readily admit that this method is insufficient as a guide to his progress." (Preface.)

The difficulties alluded to are almost or entirely obviated in the plan of the present work.

"The great divisions of the body, head, neck, thorax, bulk, extremities, and abdomen have been placed in different sections, and the dissection of each has been conducted by the employment of successive stages, determined either by certain apparently natural limits, or by those most convenient in practice; by means of full directions for the performance of the different steps; by describing at one time only so much of a part as is visible, and by noting each as it appears." (Preface.)

We would refer, as instances of the general utility to the student of the plan of this work, to the dissections of the portia dura and of the orbit, or of the back, in which the vessels and nerves are described as well as muscles, and the method to be followed in exposing each part is laid down with such care and precision as must greatly facilitate the progress of the dissector. This attention is not confined to one set of structures or portion of the body. Even in the dissection of the abdomen, where the student is often much at a loss how to proceed, owing to the little attention that has been paid in most works of anatomy to the order in which particular parts should be studied, and the stages of the dissection in which they may be best seen, the author has bestowed the

ıme care and attention as on other regions. As an example of the recision with which the dissections for these "demonstrations" have een followed out, we may instance that of the deep temporal branches f the fifth cranial nerve. It is usually stated in most anatomical works ıat several cutaneous filaments perforate the fascia, ramify in the integu- ıents, and join with the portia dura: but the author shows that the ranches that perforate the fascia to join with the portia dura nerve are erived from the orbital branch of the superior maxillary nerve, or second ivision of the fifth. (p. 99.) Again, the nerve of Wrisberg is correctly escribed as arising from the brachial plexus, and its junction with the ıtercosto-humeral is alluded to. (p. 318.)

We are much pleased to observe that the author has paid rather more ttention than is usual in most English works to the dissection of the ervous system, not so much perhaps in the addition of new facts as in -what in our estimation is almost equally meritorious — carefully xamining what has been done by others, and dissecting to verify the ame. Thus at page 569 he has described the *accessory of the obturator* of Schmidt, which is so frequently present, but is commonly passed over n silence.

In the dissection of the brain and spinal cord, the author has availed ıimself of the valuable manuscript notes of Dr. Sharpey, and throughout ıis whole work has carefully added whatever new anatomical fact has een recently established or has been overlooked. We think that the study of relative and surgical anatomy will be greatly assisted by the descriptions given of the different regions, as in the triangles of the neck, axilla, bend of the elbow, &c.; in a word, we think Mr. Ellis's "Demonstrations" are in every way fitted for the purpose for which they are intended, and we therefore strongly recommend the work to the notice of every member of the profession. We are convinced that it will quickly become the general text-book of every working student in anatomy.

Art. IV.—*Cursory Notes on the Morbid Eye.* By Robert Hull, m.d., Physician to the Norfolk and Norwich Hospital.—*London*, 1840. 8vo, pp. 249.

We are told in the dedication to this "tractate" that it "contains some digressions from the main object into the region of professional morals." And truly it does contain digressions. In the dedication the good old times of the profession are indiscriminately lauded, and the present times abused; and a great part of the preface we find occupied in decrying general medical reform, but praising the College of Physicians and the late alterations in its bye-laws.

The practical point which has struck us most in the perusal of Dr. Hull's book is his extensive experience in the use of turpentine in different inflammations of the eye. He has often found turpentine useful in iritis in alternation with mercury, when neither alone proved efficient. He has found children bear the medicine better than adults, and the least disagreeable mode of taking it to be "floating on water, or weak hollands and water, or diluted gin." In traumatic ophthalmiæ, in which the iris is the most evident sufferer, Dr. Hull has "found the turpentine maintain the good character which it had earned in spontaneous and simple iritis; in rheumatic iritis; in strumous; in syphiloid." In kera-

titis, our author has also found turpentine of great value. He relate several cases, and then remarks, "The impression on the mind of th witnesses of the terebinthinate methodus medendi is, that it tells quicke than the mercurial or the ordinary: that this is eminently visible in th wretched scrofulous child."

In Dr. von Ammon's monography on iritis, which we have alread noticed, it is well remarked, "Autocratia naturæ in iritidis decursu d bilis esse solet, sæpissime nulla; artis est hunc morbum cito et tu sanare." In a similarly just strain, Dr. Hull says that iritis is essential formidable and should never be left in quiet, but detected and attack as soon as possible. Dr. Hull, however, perfectly accords with Holland, that, generally, we make "no due allowance for the tender which all parts have to assume a healthy action if left in quiet." this Dr. Hull dwells more fully at p. 168, where he says (and the say well deserves to be laid to heart by all young practitioners),

> "As to active interference in disorders generally by professional men, th medicine expectante appears to be as rashly neglected here as it is said to be fatally indulged across the British channel. If the French physicians do, indeed, carry their expectations too far, still it may be, of two evils, much the least; and surely they deserve credit for the adoption of a mode of practice so different from the native precipitation of the Gauls. The vis medicatrix is virtually assumed to be a poetical figment; and when once the pharmacopolist has made an entry on the premises, he fancies that his sole duty and glory consist in working without her." (p. 168.)

Notwithstanding the oddities and affectation which prevails throughout this work, there are in it many glimpses of practical shrewdness and much pertinency of remark. Besides this, the author displays considerable familiarity with Greek and Latin; but it is to be regretted that he should have so frequently prostituted this to the damage of the English language.

ART. V.—*A Probationary Essay on the Special Pathology of the Accessory Organs of Hearing; submitted by the Authority of the President and his Council to the examination of the Royal College of Surgeons of Edinburgh, when Candidate for admission into their body, in conformity to their Regulations respecting the Admission of Ordinary Fellows.* By JAMES MERCER, M.D. &c.—*Edinburgh*, 1840. 8vo, pp. 133.

HAD we ourselves never written any article on the subject of this pamphlet, and had we been ignorant of its literature, we might have awarded the author considerable praise; but, unfortunately for him, we have both written and redd on the subject, and we regret to be obliged to say that, in perusing his essay, we have met with very much that we had redd word for word before, and some things even which we ourselves have penned;—and all not only unacknowledged, but given as original. As one example of this plagiarism, we would refer to the sections on the anatomical structure of the membrana tympani, of the tympanic cavity, and of the Eustachian tube, which are almost wholly a reprint of the descriptions given by Mr. Wharton Jones in the article "Organ of Hearing" in the Cyclopædia of Anatomy and Physiology. The depredations committed on our own domain are not quite so extensive, but his *drafts on* Kramer are far from inconsiderable.

Art. VI.—*Derangements, primary and reflex, of the Organs of Digestion.* By Robert Dick, M.D.—*Edinburgh*, 1840. 8vo, pp. 382.

This is one of a class of books becoming common, and, as such, characteristic of an age when all subjects are discussed in print, and redd by those who have the requisite means, inclination, and leisure. It is a medical work, written under the impression that the public would peruse it, and, for their benefit, common terms are occasionally explained, prescriptions are translated, and (to use the author's own words) "episodical and divergent discussions" are indulged in. Indeed, Dr. Dick suspects that the book may get into the hands of the boys and girls of this reading, inquisitive generation, "potest accidere ut hic liber veneat in manus juvenum utriusque sexus," and, on this account, he veils in a garb of medical Latin some very useful observations. In former times poets alone had such youthful readers, but now a writer on indigestion may say

"Virginibus, puerisque canto."

Owing to this double object, the author becomes unnecessarily explanatory for his medical reader, and, consequently, he is often tedious. Few have time enough to pick out what is new from so much that is well known—to wade through an octavo volume instead of mastering a pamphlet. Besides this diffuseness of matter, Dr. Dick's style is destitute of conciseness; he frequently repeats the same idea (and that a very common-place one) in different words, and he seems seeking too often for the longest word and the most sounding sentence. Digestion is turned into "stomachic processes," to empty itself is "to disembogue," grief becomes "moral chagrin," &c.

But, in adducing these minor defects of composition, we do not mean to condemn the book: it is not a common clap-trap compilation of other men's opinions. Dr. Dick has evidently treated dyspepsia, and has suffered from it himself: he has thought much and read more about the disease; his powers of observation are considerable, and his practical remarks are, for the most part, judicious and moderate. His descriptions of the appearance and manner of patients suffering from dyspepsia, and of their mental condition, have an air of truth and freshness, such as those alone can produce who observe nature minutely and thoughtfully, and have, besides, adequate descriptive powers. We could have wished that Dr. Dick had given himself time enough to have written a shorter book—a treatise for his medical brethren only, giving them credit for knowing the meaning attached to the words life, death, vital principle, nervous influence, and such elementary matters; whilst, in the shortest compass, he had endeavoured to embody his own experience and reflection. It has been said by one of the writers in the Memoirs of the French Academy of Surgery, that "the increase of knowledge is not like that of other things; for as our opportunities increase, there is often a great diminution in its bulk:" a golden precept for all medical writers to meditate upon deeply.

ART. VII.—*The Maternal Management of Children, in Health and Disease.* By THOMAS BULL, M.D., Lecturer on Midwifery, &c.—*London*, 1840. 8vo, pp. 310.

DR. BULL's work is written with nearly the same views and intentions as that of Dr. Combe, reviewed in our last Number. We did not, however, think it proper to place them together in the same article, as it would have been unjust to Dr. Bull to subject him to a close comparison with so very philosophical and so very superior a writer as Dr. Combe. Dr. Bull's work, nevertheless, is one of fair average merit, and contains many useful directions for mothers as to the best mode of managing children in health and disease.

ART. VIII.—*Elements of Geology for popular Use; containing a Description of the Geological Formations and Mineral Resources of the United States.* By CHARLES A. LEE, M.D.—*New York*, 1840. 12mo, pp. 375.

THIS is another of those excellent manuals for which America is indebted to Dr. Lee, and which successfully aim at bringing down the lofty truths of science to popular apprehension. The present little volume cannot fail to be most useful to the class for whom it is written, and will add to its author's reputation. We do not know a better work for putting into the hands of beginners in geology, and we therefore notice it in our pages. It has the usual American blemish, of wretched woodcuts; but for this the author is not responsible.

ART. IX.—*A Treatise on the Structure, Functions, and Diseases of the Foot and Leg of the Horse; comprising the Comparative Anatomy of these parts in other animals, &c.* By W. C. SPOONER, M.R.V.C.—*London*, 1840. 8vo, pp. 337.

THIS is a very valuable work, and merits the attention not merely of such members of our body—a vast majority—as are directly interested in the practical part of the subject of which it treats, but also of those who pursue the study of comparative anatomy and comparative pathology, as objects of scientific enquiry. The author has evidently studied his profession in the proper manner.

ART. X.—*The Retrospect of Practical Medicine and Surgery for the year* 1840; *being an Analysis of the British and Foreign Medical Journals and Transactions, or a Selection of the latest Discoveries and most Practical Observations in the practice of Medicine, Surgery, and the collateral Sciences for the past year: made chiefly with reference to the Treatment of Disease.* By W. BRAITHWAITE, M.R.C.S.—*London*, 1840. 12mo, pp. 208.

THE nature of this little work is accurately indicated in the above title; we will only add that the plan is extremely well carried out in the present number, and that the undertaking merits the patronage of the profession. We hope Mr. Braithwaite will meet with such support as to induce him to continue his labours.

PART THIRD.

Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

Memoir on the Relations which exist between Blood, Pus, Mucus, and Epidermis. By L. MANDL, M.D.

THIS is a long memoir read before the *Société Médicale d'Emulation*, on June 3, 1840. We have only space for the general conclusions of the author; they are as follows: 1. The fibrinous globules of the blood, the globules of mucus, and those of pus, are identical. 2. All the globules are the product of the coagulation of the fibrine in the serum, which has transuded through the walls of the blood-vessels. 3. The liquid in which the globules swim constitutes the difference between pus and mucus. 4. If the fibrinous globules remain fixed to the surface of the membrane where they are secreted, they become the nuclei of epidermoid cellules, which constitute the elements of the epidermis. 5. If, on the contrary, the fibrinous globules remain free on the surface of the membrane, they are expelled by the organism, and form an element of pus and mucus. 6. These two liquids are simply filtered blood; that is to say, they contain all the elements of the blood, except the globules: the serum at the same time undergoing chemical alterations.

Gazette Médicale de Paris. Juillet, 1840.

Menstruation in a Child. By Dr. LENS, of Danzig.

A healthy woman, the mother of several hearty children, was delivered two years ago of a well-formed girl, which she, as usual, suckled herself. In the course of the year the child was weaned, and in the second year continued growing; but its parents were not a little surprised, when, in its eighteenth month, a flow of blood from the genitals took place, continued for some days, and then ceased. When I saw the child it was two years old and perfectly healthy, although the discharge of blood had, since the time mentioned, recurred regularly at every new moon. The breasts and genital organs were just like those of other children of the same age and sex; but the parents stated that the latter were hotter than usual, and somewhat swollen, before and during the discharge of blood, and that at the same time the child was always ill, with disturbance of the vascular system and heat about the head, great thirst, redness of the face, loss of appetite, and so on; and that when the discharge ceased, the child was always immediately after restored to its good health. The flow of blood always continued for three or four days, and, according to the parents' account, amounted to two, or, at the most, three ounces each time. As the discharge did not appear to interfere permanently or seriously with the child's health, the author did not attempt to control it.

Casper's Wochenschrift. October 3, 1840.

On the natural Existence of Copper and of Lead in the Human Body. By Professor F. DE CATTANEI DI MOMO, of Pavia.

HAVING conceived the idea that the copper and lead, stated by Devergie to exist naturally in the human tissues, might have been received into the body with the food and from other sources, the author instituted experiments to determine whether those metals existed in the bodies of children that died a few days after birth, and had taken nothing but their mother's milk.

He took, therefore, 1st, the stomach and small intestines of a seven-months' child that had lived two days, and had taken only a little milk; 2d, the lungs, heart, and whole intestinal canal of another seven-months' child, fed, and having died in the same way; 3d, the lungs, digestive canal, liver, and spleen of a child born at the full term, and that had lived on its mother's milk for twenty-five days. The digestive canals of all three, the hearts and lungs of the first two, and the liver and spleen of the third were carbonized separately in Hessian crucibles, to prevent any suspicion that copper might be extracted from some alloy of silver if vessels of that metal were used, and that lead might proceed from the varnish if porcelain vessels were employed. The animal matters being thoroughly charred they were incinerated, and to accelerate the operation, and for the sake of contrast, nitric acid was used with one product and chlorate of potash with the other. Acetic acid was then poured on the ashes, and its action assisted by heat; an excess of ammonia was then poured into the solution, and, after standing for a time, the clear liquid was poured off. It had no azure tint, nor was any precipitate obtained from it by the double yellow cyanuret of potassium, or by hydrosulphuric acid, or by the immersion in it of a piece of polished iron. The solid portion, obtained by the addition of the excess of ammonia, was then redissolved in acetic acid, and, after filtration, there were added successively to portions of the clear fluid iodide of potassium, chromate of potash, and hydrosulphuric acid; and a piece of zinc was placed in it. The only reagent that could excite any suspicion of the existence of lead was the hydrosulphuric acid, which made the liquid slightly brown. The deposit that subsequently formed was redissolved in very dilute hydrochloric acid, and yellow cyanuret of iron and potassium being added to the solution, produced a precipitate of an azure blue colour; showing that the brown colour before seen had depended on the existence of sulphuret of iron.

From these analyses it was concluded that not a trace of copper or of lead had been obtained from the viscera. The author was proceeding to experiments on portions of adult bodies, when he learnt that a commission of the French Academy of Medicine had been unable to confirm the opinion of M. Devergie respecting the existence of copper in the body. The report of the commission did not allude to the occurrence of lead, but the author conceives that the results he obtained are sufficient proofs of its non-existence.

Annali Universali di Medicina. Aprile, 1840.

Some Researches on the specific Odour of the Blood. By DR. C. TADDEI DE GRAVINA.

THE object of the experiments whose results are given in this paper, was to test the truth of the well-known principle laid down by Barruel, and more or less acknowledged by other chemists, that the blood of each individual exhales an odour closely resembling that of the cutaneous perspiration, and so peculiar that the species, and even the sex of any animal from whom a given quantity of blood has been drawn, may be determined by it.

The details of the previous investigations may be found in any extended work on medical jurisprudence; those of the present included examinations of the blood of the ox, cow, and very young calf, an old and a very young hare, the goat, sheep, hog, horse and mare, dog, man and woman, and numerous species of birds. The blood of each was subjected to the action of pure sulphuric acid,

e exhalations that were then given off, the peculiar odour was in every ived. The only cases in which the odour of one kind of blood could nded with that of another were those of the ox, cow, and sucking calf, of the hare and leveret; but in each of these cases the aromatic prin- e blood of the adult animal, though similar to, appeared stronger and rant than that of the young, although the respective quantities of each, e sulphuric acid with which they were treated, were the same. Nor dours of the blood of a man and of a woman unlike each other, nor the horse's and mare's blood. The odour of the blood in each case ifferent from that of the cutaneous exhalation of the several animals; e manner, the blood of several cocks and hens, and of not a few ibjected to the action of sulphuric acid, or left to putrefy, always ex- culiar odour; in the former case reminding one of that perceived on poultry-house, and in the latter of that of a dove-cote: odours which xhaled from the skin of the chest and under the wings of those birds. analogous to that of these parts of the skin, was given out under cumstances from the blood of thrushes, sparrows, linnets, goldfinches, s, and turkeys; and none of these had in its odour anything in com- that of the above-mentioned mammalia or of man, or of any of the birds. It follows, therefore, 1st. That it is true that the blood of ebrate animal has in it an odoriferous principle, identical in all the s of the same species, and similar to the odour of the cutaneous tran- or, more properly speaking, of that part of it which gives to each characteristic smell. 2d. That the notion of those who pretend to to which, among a number of individuals of the same species, a given blood belongs, is false.

taining these results, the author proceeded to the similar investigation od of persons labouring under different diseases and under various liar circumstances; but in none of these cases could he detect any ling differences in its odorous exhalation. From all these, therefore, sion is, that neither the differences of age, of constitution, of tem- of sex, of habit and customs, or of modes of living, nor diseases, me- pregnancy, induce any change in the specific aroma of the human that it always preserves the same general odour; being only some- e fragrant and acid, or more garlicky and nauseous in some than in ons.

Annali Universali di Medicina. Febbrajo, 1840.

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ary Enlargement of the Kidneys in the Fœtus, from the Development of s in their Substance. By Dr. F. OESTERLEN, of Murrhardt, in berg.

STERLEN was called in January last to a patient in labour, whose deli- etarded by the extraordinary size of the child's abdomen, but before the fœtus was expelled by the natural efforts.

ld, a female, was still-born; it appeared to have reached the full ts weight was nine pounds, that of the placenta, which appeared ro pounds. On opening the enormously large abdomen scarcely any- to be seen but two large bodies, which were situated one at each side ity, and had forced the liver up close under the diaphragm, so that ll part of it was visible. The two bodies were ascertained to be the they resembled each other exactly in size, structure, colour, and r shape was natural, and they were both divided into lobules, as is e fœtus; they each weighed nine ounces, were about five inches and a and four inches broad at their broadest part; they were smooth on e, of a red colour, in parts verging towards violet, and studded every- little granules of a dark blue or gray colour. These little bodies ds, which, on dividing each kidney, were seen to occupy the whole

of its substance. The smallest hydatids presented a diameter of less than a quarter of a line, the largest of about two lines, but the average diameter was about a line. The smallest were near the surface, and they became larger towards the centre of the organ. Their form was spherical, and they were perfectly transparent.

The pelvis of each kidney was small, and contained a little clear watery fluid. The only remains of the true parenchyma of the organ consisted of a firm, fibrous, reddish tissue, which was by no means abundant. The kidneys were well supplied with blood. The bladder contained only a few drops of a perfectly limpid fluid. All the other organs in the body were natural.

Neue Zeitschrift für Geburtskunde. Band viii. s. 384.

On the Microscopic Constituents of Milk. By Professor Nasse, of Marburg.

After a careful microscopic examination of milk from pregnant and suckling women, as well as from a cow and a bitch, and a comparison of his results with those of Donné and other preceding observers, the author says that the following may be enumerated as the microscopic constituents of the normal secretion of the mammary gland: 1. The smooth, homogeneous, transparent oil-globules, to which, in addition to the common milk-globules, belong also the fine, scarcely measurable particles and the larger drops of oil which swim on the top of the milk. 2. The cream-globules which are distinguished from the oil-globules by their opacity and their facette-like aspect. 3. The granulated yellow corpuscles. 4. The lamella of epithelium. 5. The more or less turbid medium, in which the four preceding kinds of corpuscles are suspended.

The first, the common milk-globules, are composed entirely of fatty matter, which dissolves completely and rapidly in ether. No membrane can be seen investing them. In the first nine days after delivery, the largest globules measure $\frac{1}{200}$ of a line in diameter, afterwards they are as much as $\frac{1}{100}$, but many are found of a much smaller size, and through all periods of lactation, the microscope, as well as other means of examination, show that the proportion of oil-globules in the milk varies greatly in different persons and under different circumstances.

In perfectly fresh warm woman's milk, no other globules than these are sometimes found. But as soon as the milk has stood for some time exposed to the air, other corpuscles are discernible in it which are distinguished from the preceding by a greater definiteness, a less degree of polish, and an appearance of facettes. In size they are nearly similar to the oil-globules, but if the milk be examined some time after it is drawn a number are found much larger; $\frac{1}{30}$ of a line or even more in diameter. They are not so easily soluble in ether as the common milk-globules; they do not break up in drying, but they become clearer; acetic acid and ammonia have no influence upon them; they diminish for a time when the milk is boiled, but they reappear gradually as it cools again; when left at rest they collect on the surface and form the cream; they easily stick together, and butter is formed when they are collected in one homogeneous mass. It is evident that they acquire their peculiar characters after they are drawn from the gland-ducts, for the author, as he watched them on the field of the microscope, could see individual globules which were originally clear, becoming on a sudden quite dark, and assuming the several characters of the cream-globules.

The yellow granulated corpuscles are almost peculiar to the colostrum; after the first few days from delivery they cease to occur in the milk, and they disappear from it earlier in those who have borne several children than in primiparæ. They are not all spherical; the majority are flat. Their diameter is at most from $\frac{1}{300}$ to $\frac{1}{100}$ of a line; some are found measuring $\frac{1}{30}$ in length and $\frac{1}{75}$ in breadth. They consist of small clear globules of fatty matter, which are connected together by a firm cement which is unalterable by either ammonia or concentrated acetic acid, or by boiling. When the milk is left at rest, these globules collect on its surface; and when they exist in considerable numbers render it

unfit for making butter. The author believes that they are not, like the preceding globules, formed by the action of the air, but that they are produced by the secreting surface of the gland-ducts, and are analogous to the mucus-cells which are cast off from the surfaces of many mucous membranes, and to which they are in many respects very similar.

Müller's Archiv, 1840. Heft iii., p. 258.

Experimental Researches relating to the Mode of Transmission of Rabies. By M. BRESCHET.

AT the sitting of the Royal Academy of Sciences, September 21, 1840, M. Breschet brought forward proofs of the contagious nature of hydrophobia. Horror of liquids, and other hydrophobic symptoms, have been observed in man independently of contagion, but they essentially differ from the rabies of inoculation. The term hydrophobia is not applicable to animals, as in them the horror of water is not an essential symptom. Rabies is always produced by inoculation, and MM. Breschet and Magendie proved that it may be transmitted from man to the dog by inoculating a dog with the saliva of a hydrophobic patient. Thirty-eight hours afterwards the dog became furiously rabid; he bit several other dogs, and these became successively rabid. Some of these observations appear to show that the virus becomes enfeebled, or loses its deleterious properties in passing through several persons. The period of latency of the poison is about twenty or thirty days. Some of the dogs drank water with avidity. The virus is transmitted from carnivorous to herbivorous animals; in an ass bitten by a mad dog the disease was evidenced in its most intense degree. Two horses inoculated by the saliva of a dog had the disease less marked. The saliva of the ass above mentioned was put under the skin of several dogs and rabbits, and they all became rabid. Birds were killed by inoculation, but did not exhibit symptoms of rabies.

Is the blood altered in rabies? M. Breschet answers that he has several times transfused the blood of a rabid into the veins of a healthy dog, but has never determined the development of rabies in this manner. It appears that the saliva of the diseased animals alone affords the necessary conditions for the transmission of rabies. The saliva, or slaver, is really an altered fluid, a humour in a true morbid state; or the vehicle of a deleterious principle, of a true rabic virus newly secreted, but the nature of which is as yet completely unknown to us. Rabies then is a virulent contagious disease, and not the effect of a moral affection. *Archives Générales de Médecine, Octobre*, 1840.

Bronchocele in a Fœtus of about Eight Months. By Professor F. MONDINI.

THE subject of this case was born in a prison in which its mother was confined for a theft. It was a male child and born dead; the placenta and membranes were unnaturally large, and the liquor amnii very abundant, and it was evident that it had been alive to within a very short time of its birth.

At the anterior part of the neck there was a large tumour, a bronchocele. It occupied chiefly the left side of the neck, and was of an oval tuberculous form, and was here and there spotted with bright red. Commencing above at the margin of the lower eyelid, and hideously pushing up the nose and the mouth, it extended laterally, and protruding the lips, descended of an enormous size down to the lower apex of the sternum, constantly tending more to the left than to the right. It was longer than it was broad, measuring in one direction three inches and eight lines, and in the other three inches and a half. From a more minute anatomical examination, it appeared that this tumour consisted of a peculiar alteration of the cellular tissue interposed between the blood-vessels that form the network, and covering the network itself; the network in the thyroid gland, which, as Soemmering says, may be compared with the so-called rete mirabile, which is found on the carotid and the ophthalmic arteries of ruminants. *Novi Commentarii Acad. Scient. Instituti Bononiensis v.* iii., *and Annali Universali di Medicina.* Ottobre, 1839.

and as Magendie has shown that when these crura are divided, the balance of the body seems to be lost, it is clear how an irritation of the trigeminus induces giddiness, and how that symptom is most marked when the signs of irritation are passing off or gone.

The vomiting is explained partly by the affection of the spinal cord and partly by the influence of the nerves on the sight. (In explanation of this the author refers to his own investigations on vomiting recently published.)

The shivering is the result of the stimulus of the central extremities of the sensitive nerves in the spinal cord; and it is comparable with the results of several of the author's experiments, in which he has observed that on exposing the spinal cord in a dog or other animal, and gently stroking it with a feather, all the characters of a shivering fit are immediately produced. In this, as in all other cases, the condition of the central extremity of the nervous fibre is felt as if it were that of its peripheral extremity.

The sneezing at the end of the paroxysms probably results from the nasal branch of the trigeminus becoming involved in the excitement when the irritation of the frontal is at its greatest height. The excitement is in this case also probably communicated in the nervous centre though felt where the fibres are distributed; and it is communicated not to the sensitive nerves alone, but by reflex action to the nerves of the respiratory muscles.

The author, believing that the essential cause of the symptoms was a congestion of the vessels of the frontal nerve, had ten cupping-glasses applied every eight days near the vertebræ at which pain was produced by pressure; these were applied three times; mercurial ointment was rubbed in morning and evening, and four grains of sulphate of quinine with two of extr. nucis vomicæ were given in two doses every day. Their result was most striking; the patient, who had for twenty-two years been entirely unrelieved by a variety of methods of treatment, had no severe attack after this was employed; after several months had elapsed she continued well, and even the divergence of the right eye was in some degree lessened.

Casper's Wochenschrift. October 3-10, 1840.

Effects of Over-eating. By Dr. Ritter.

A girl, three years old, went with her father to gather plums; as they fell from the trees she kept eating an enormous quantity of them, and then, feeling very thirsty, drank cold water from a spring close by. Soon after returning home, having seemed previously quite well, she suddenly fell down as if struck dead by lightning; her eyes were convulsively pulled about, and she lay quite senseless. The author being immediately sent for found her with her eyes staring wildly, unconscious, insensible, and speechless; her pulse was suppressed, scarcely perceptible, and small, her respiration short and quick, frequently interrupted by sighing, and her face pale. The precordial region was distended and very tense. An emetic, containing one eighth of a drachm of vinum antimonii and one eighth of a grain of tartar emetic, was immediately given, and was repeated every five minutes, but after several doses produced no effect. Three powders were then given at intervals of ten minutes, each of which contained a grain of tartar emetic, and six grains of ipecacuan, but these excited only retching; vomiting was at last brought on by tickling the pharynx with a feather. The materials discharged consisted of a huge mass of tough, half-digested plums with some frothy fluid. The severest convulsions now ensued, especially on the right side of the body. The eyes rolled irregularly and convulsively in their orbits; the mouth was drawn over to the right side, and watery froth kept running from it; the right arm and foot were set into a constant swinging motion, the face was earthy, the glance oblique, and the nose pointed, in short there were all the appearances of closely impending death. Some strong coffee and some wine and water were given, but the convulsions still continued. A mixture of equal parts of castor oil and almond oil

was next prescribed, and a spoonful of it was given every ten minutes; clysters, containing oil, vinegar, and salt, were also administered. Soon after the second of these, borborygmi commenced, and were presently followed by the discharge of an enormous mass of half-digested plums, which was again repeated for several times. The child not long after came to herself again, and though weak was still free from apparent danger. She slept well during the night, and next morning, with the exception of dullness and repeated diarrhœa, was well and in good spirits.

Medicinische Zeitung. October 14, 1840.

On the Medical Action of the Bezoar of the Lacrymal Fossa of the Deer (Deer's tears) in Nervous Diseases. By Dr. LÖWENHARDT.

THE substance here described is a moist, strongly-smelling, fatty matter, which appears to the author to be secreted by the mucous membrane lining a fossa just below the anterior canthus of the red deer (*cervus elephas*). It is found in this situation all the year, but in greatest quantity, it is said, during the rutting-season, it appears to serve by the diffusion of its powerful scent to attract or to keep off other kinds of animals. It exists only in the adult individuals of the species.

The smell of this bezoar is peculiar, very penetrating, strongly balsamic, much like that of amber mixed with a little benzoin, only still more pungent, and adhering half or all the day to the substances with which it comes in contact. Its taste is in like manner balsamic, and its most active principle is a peculiar resin which is of a yellow colour and lighter than water. Its analysis yielded resin (with a mixture of etherial oil), a very small quantity of fatty oil, wax, cellular substance, ammonia, and colouring matter, with muriate of soda and phosphate of lime; but it is probable that a part of these constituents are derived from the hair which is usually entangled in it.

It appears, by several cases which the author describes and refers to, to have a certain value in all the diseases in which castor, musk, valerian, and the other allied articles of the materia medica have been hitherto, and often unsuccessfully, employed; as hysteria, epilepsy, difficult menstruation, prolonged hooping-cough, &c. The modes of administration which the author recommends are, 1st, in substance, in doses of from 5 to 10 or 15 grains, twice or three times, in pills with mucilage; and 2dly, in tinctures composed of one part of the bezoar to six parts of rectified spirit, or of etherial spirit in doses of ten, fifteen, or twenty drops twice or three times a day.

Medicinische Zeitung. August 12-19, 1840.

Cure of a Severe Gouty Pain by the Electric Ray. By Dr. A. CHABRETES.

N. suffered three months without any evident cause from a very troublesome gouty pain in the first and second joints of the right fore-finger. He had used all the usual means against it, both internal and external, without benefit; on the contrary, the disease increased so much that he could not sleep during the greater part of the night, and in the day could not hold his pen. Thus he was going on one morning, trying to think of some new remedy by which he might possibly relieve his pain, and carefully avoiding meeting any one, lest an incautious touch might renew it now it was a little assuaged, when a fishing-boat drew near to sell the fish her men had just caught. Among them the patient remarked the electric ray, and being acquainted with its electric properties, he bought it and carried it home alive in a vessel filled with sea-water. Although the fish was but small, yet it possessed a remarkable electric power; and the patient had scarcely touched it with the middle finger of the diseased hand, when he felt a severe shock and a peculiar pain in the elbow, with a numbness of the whole arm and of the fore-finger that had been the seat of the

pain. After the lapse of a quarter of an hour, the numbness gradually ceased, and there remained only the peculiar sensation in the elbow, and that in a less degree. The fore-finger still gave indeed severe pain, but only when it was moved. Two hours after a second experiment was performed, of which the results were similar but more violent, and with this difference, that the fore-finger could soon afterwards be voluntarily moved without any pain, and when moved by the other hand, gave not the usual severe suffering, but only an unusual and peculiar sensation. This condition continued without material alteration till seven in the evening. At that time when the patient again repeated the experiment and touched the fish with the fore-finger itself, he felt a slight shock; but when he stimulated it with a piece of iron, he received seven shocks successively, and the fish soon after died.

That night the patient slept quietly; in the second he still felt only a numbness and weakness in the motions of the finger, which were completely removed by cold baths and the cold douche in five days more. Since that time the finger has been quite freely used, and there has not remained the least trace of its former condition.

[Dr. Bouros, by whom this case is communicated, has added some notes respecting the uses which the ancients made of electric fish, but they are not of much interest, and may be found in the Dict. des Sciences Med. *art.* Poisson. He believes the fish used in this case was the Torpedo Narke. There is no reason given for calling it a gouty pain (*Gichtschmerze*) of the finger; was it not rather neuralgic?]

Casper's Wochenschrift. August 29, 1840.

On the Uncertainty of the Signs of Peritonitis and on a New Character of that Disease. By Luigi Sementini.

The chief object of this memoir, is to prove the fact which the author says he has tested by constant observation for upwards of forty years, namely, that in all cases of peritonitis, in whatever part of the abdominal cavity the inflammation is seated, there is pain in the pubes and upon the great trochanters which if not spontaneously felt is always developed by pressure, and of which the severity is directly proportionate to that of the peritonitis.

This fact, which is said to be confirmed by the clinical observation of others, the author believes is explicable by the relation of the nerves of the parts in which the pain is felt to the peritoneum, and by its connexion with the fasciæ and muscles about them. In addition to its value in the diagnosis of even the most obscure and latent cases of peritonitis, in all of which this sign is present in a degree proportioned to the severity of the disease, the author has found it of value as an indication of treatment, and has obtained great benefit from the application of leeches and blisters over the trochanters instead of on the abdominal walls.

Annali Universali di Medicina. Settembre, 1839

Case of Convulsions of a peculiar kind. By G. Terrone.

R. S. a country labourer, of sanguine temperament, came to the hospital in March, 1838. He said he had always enjoyed good health, but had been constantly subject to poverty and hard work. Six months ago he began to suffer from a painful contraction of the back, which tormented him from time to time, and was not alleviated by any of the means that he had employed. On examination, the affected part presented a curious appearance. The mass of muscles between the fourth and the eighth dorsal vertebræ, and for two inches width over the corresponding ribs rose up in the form of a compact, resisting, and hard globe; continued so for a few instants, and then disappeared and left the part in its natural condition. Immediately after a similar swelling rose upon the opposite side; and so on, disappearing on one side when it appeared on

the other. The diaphragm, and the abdominal and intercostal muscles participated in these convulsions, and they were accompanied by remarkable constriction of the throat, and frequently by slight hiccup. They occurred every five minutes, and when they ceased they left the patient in a perfect state of health.

The reader can form as good an opinion as the author does of the pathology of this affection. It is sufficient to add that it was gradually cured by cupping and the application of tartar-emetic ointment along the dorsal region of the spine, and by warm baths, with the occasional use of sulphuret of antimony and acetate of morphia.

Annali Clinici dell' Ospedale degl' Incurabili. Ottobre, 1839.

Apoplexy, with Hemiplegia, in consequence of Fright. By DR. RITTER.

THE only peculiarity of this case is its cause. A robust and rather plethoric woman, thirty-eight years old, was in perfect health and speaking to a neighbour, when her servant-girl frightened her by brandishing a bright spiral wire over her head, so as to make it look as if a snake were falling on her. In her fright the woman suddenly fell down as in an apoplectic fit, and remained for some time nearly unconscious. When examined she complained of a noise and beating in the left side of her head, deafness of the left ear, and of blindness and loss of taste on the same side. She could not move any part of the left side of the body, and in every respect resembled a patient suffering from hemiplegia in consequence of sanguineous apoplexy. By active antiphlogistic treatment, and various other measures, she was gradually restored from this state in about three months.

Medicinische Zeitung. September 9, 1840.

Observations on the Therapeutic Efficacy of Ammoniuret of Copper in Chorea. By DR. FEDELE DI FIORE.

Two cases are related of genuine chorea to prove the value of this once esteemed remedy, which in Italy passes by the name of "the specific of Stissero." In each case nearly all the usual and most active methods of treatment had been adopted before this was resorted to; bleedings, leeches along the spine, anthelmintics, purgatives, cold baths, and various antispasmodics had all signally failed to produce benefit, when the ammoniuret of copper was commenced. In doses beginning at one eighth, and gradually increased to half a grain twice a-day; the latter effected a gradual but perfect cure; in one case within two months, in the other in one month.

Annali Clinici dell' Ospedale degl' Incurabili. Ottobre, 1839.

On a Sedative Lotion in Headaches, Congestions, and Cerebral Fevers. By M. RASPAIL.

PROFESSOR RASPAIL, in a letter to the editor of *l'Expérience*, gives the mode of preparing a lotion, the sedative effect of which, he says, is almost instantaneous. It is as follows:

Liquor of ammonia (Qy. the strength ?) . .	100 parts
Distilled water	900 „
Purified marine salt	20 „
Camphor	2 „

Essence of rose or some other scent, in the necessary proportion. The whole dissolved cold.

A piece of linen is to be steeped in this solution and applied over the part of the head that the patient points out as the seat of pain, taking care, if it is on the forehead, to apply a thick bandage over the eyebrows, to prevent any drops of the fluid passing into the eyes.

M. Raspail says he has seen headaches intolerably violent, accompanied by photophobia, and retraction of the globes of the eye, disappear completely, from a quarter to half an hour after the application of one wetted cloth. The linen is to be soaked as often as a new access of pain is threatened, and left on the head until it is necessary to soak it anew. In the numerous trials the author has made with this solution, first on himself and afterwards on others, he has been struck by two circumstances of interest in connexion with organic chemistry, and symptomatology. When in a violent attack of cerebral fever, we apply on the principal seat of the inflammation a concentrated solution of marine salt, an evident odour of chlorine is disengaged, the diseased reaction being analogous to the decomposing and deoxygenating action of manganese, in the elimination of chlorine from marine salt, by means of sulphuric acid. Is this sign constant in affections of this class? It is for experience to decide. When on the contrary we employ a solution of ammonia, a strongly characterized *hircine* (goatish) odour is manifested. The same odour has been disengaged on the application of hydrochloric acid to the skin. M. Raspail has drawn the attention of the profession to this subject in order that they may employ this formula, and fix their attention on the analyses of the disengaged substances, as they may become characteristic of special affections.

L'Expérience. 24 *Juillet*, 1840.

Case of Posthumous Variola. By Dr. Chansarel.

A girl, aged five years and eight months, who had been vaccinated, was suddenly attacked, when in perfect health, with a varioloid eruption and fever. About twelve hours afterwards the redness of the skin and the pimples totally disappeared, cerebral phenomena supervened, and death twenty-six hours after the first appearance of illness. On the next day, thirteen hours after death, "I saw," says M. Chansarel, "to my great astonishment, a great number of pimples which had appeared after death, and which were much more developed than those I had seen during the life of the child. They were seated on the face, neck, chest, and buttocks; those in the latter region being larger, equalling the size of a lentil. They had the aspect of variolous pustules, surrounded by a red areola, depressed in their centre, and contained a small quantity of fluid. The body exhaled a fetid odour, and exhibited some red and livid spots, putrefaction not having been so slow as it appeared."

L'Expérience. 27 *Août*, 1840.
Extracted from Bulletin Medical du Midi. Juin, Juillet, 1840.

Mode of Resolving Engorgements of the Spleen. By M. Voisin, of Limoges.

The author has employed the following treatment with success in eight or ten cases of these engorgements after intermittent fevers. In three or four of these cases the diseased organ occupied about two thirds of the left half of the abdomen. The treatment is simply to apply a mercurial plaster (*vigo cum mercurio*) with which is incorporated six or eight scruples of the sulphate of quinine, more or less. It is to be renewed when the matter of which it is composed is exhausted, that is, from forty to fifty days. The advantages of this mode of treatment are: 1. It saves the patient from the disgust which he undergoes when the quinine is administered by the mouth. 2. The absorption and consequent action of the remedy are continued. 3. This absorption and this action are accomplished in the immediate neighbourhood of the diseased organ. 4. Owing to the continuance, the fever does not reappear.

This treatment has alone sufficed for the cure. The period of cure will vary as the spleen is more or less engorged, the patient more or less aged, and the skin more or less readily absorbing. Ordinarily, two or three months suffice.

Gazette Medicale de Paris. 12 *Septembre*, 1840.

the Utility of Graduated Compression of the Abdomen by Bandages, in the Treatment of Ascites. By Professor L. MORELLI, of Siena.

M among many cases that were either cured or materially relieved by this of treatment, the author relates one in which its benefit was most strikingly . It was that of a woman upwards of fifty years old, who from poverty, her sources of misery, had become gradually more and more ill, and had th come to the Clinical Hospital with ascites from diseased liver. She en salivated, and had undergone a variety of treatment, and had been four times. The author recommended his system of bandaging, but for time the patient refused to adopt it, and in the interval it was found neces- perform paracentesis eight times more. After the twelfth operation the ;, now reduced to a most perilous state, agreed to submit to the ban- , of which she had seen the good effects in a dropsical child. The best bandage consists of a band extending from the lower third of the sternum pubes, and round to the loins, and there gradually tightened by means of of leather, with several holes in them for the passage of the points of small The whole is prevented from shifting upwards by two strips of strong which pass downwards, and are stitched behind in the neighbourhood of ro-iliac symphyses. This apparatus being put on, and gradually tight- he abdomen of the patient slowly returned to its natural size; the urine abundantly, her appetite and sleep returned, and, at length, after nearly ears' illness, in which she had used a variety of medicines, and had been twelve times, she was restored to good health. A year afterwards, having tinued the bandage, the ascites appeared again in a slight degree, but ive medicines and the reapplication of the compression again speedily ed it.

ts first application, and for some days afterwards, the bandage produces nconvenience; so much, that many patients refuse altogether to wear it ufficient length of time; but by a proper selection of cases, and careful ement of them in other respects, the author asserts that compression found one of the most successful means that have ever been employed t ascites. It must be confessed, however, that he gives no indications, es he himself seem to have learned in which of the very different stances in which ascites occurs, his method is either applicable or likely ve useful.

Annali Universali di Medicina. Marzo, 1840.

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ydrophobia and the relation between the Retina and the Respiratory Nerves in Disease. By Dr. A. TRIBERTI, Physician in Ordinary to the *Ospedale ggiore* of Milan.

main object of this paper is to recommend that hydrophobic patients be kept in almost perfect darkness, and carefully excluded from the view thing bright or glistening, to avoid the severe paroxysms which the sight h objects has never failed to bring on in all the cases that the author has These have been seventeen in upwards of thirty years' practice. In all these the hydrophobia did not once come on before the 25th, nor after the 60th om the reception of the bite. All were hydrophobic when they were ht to the hospital, and in all, the wound which had cicatrized a few days he bite, was found red, with heat, burning, and a painful and spasmodic ion of the whole limb, or of all the bitten part. In five of the seventeen he wound had opened again, and presented a malignant ulcer, discharging l serous fluid. All died on the second or on the third day from the com- ment of the attack. All complained of languor and lassitude, which were panied by anxiety and deep sighing; all were desirous of being left alone the dark; in all the pulse was quick and irregular, and in some rather After the first twenty-four hours of their being in the hospital, the patients

all complained of tightness and oppression of the præcordia, with dyspnœa and excessive thirst, which they could not alleviate by drinking fluids, though they could swallow small pieces of solid food. The spasms of the muscles of the pharynx produced in all the patients a distressing sensation of a ball or globe in it, which was always increased at the sight of any drink, but was diminished by swallowing any solid substance. The spasm of these muscles was also in all cases accompanied by convulsive movements of the head, and by severe tremblings of the other muscles of the neck.

Annali Universali di Medicina. Givgno, 1840.

On a stony Concretion in a Human Nose By STEFANO GRANDONI.

A WOMAN of the province of Brescia, aged 32, was brought to the hospital for a disease of six months' standing, consisting of a calculus, which was situated in the left nostril. Of this affection she was entirely cured by the extraction of the calculus, which was effected with a pair of polypus-forceps. The concretion, in the condition into which the operation had reduced it, was in fragments of unequal sizes, covered with small rough prominences, furrowed longitudinally, and invested by a kind of crust inseparably united to its chief mass, slightly tinged of an iron-rust colour, and beset with minute pits. The fracture of this substance showed that the texture of the whole mass was very similar to that of the compact calcareous earth, in which there is some crystalline glittering. The concretion had a specific gravity of 1·4, and therefore sank in water; at the ordinary temperature it did not emit any odour; it weighed altogether seventy-six grains. In none of the fragments could there be recognized the nucleus of the concretion, unless one could regard as such a little seed of some grass which occupied the centre of the largest fragment. On being subjected to an accurate chemical analysis (the details of which are given), a hundred parts of the calculus yielded the following constituents:

Phosphate of lime	55
Carbonate of lime	18
Carbonate of magnesia	7
Organic matter, with traces of iron . .	20
	100

Commentarii dell' Ateneo di Brescia, 1837-8;
and *Annali Universali di Medicina. October*, 1839.

Angina Pectoris, terminating fatally by Rupture of the right Coronary Artery. By DR. A. GALLARDI.

A SHOEMAKER, aged about 40, was received into the Milan Hospital, May 21, 1834. He was of good constitution, well formed, of rather nervous temperament, and somewhat addicted to strong drinks. He said that for some months past he had at times, while at work, felt some difficulty of breathing, with a loss of power in the left arm, and subsequent slight lipothymia. Twice in the last month, on going out of his cottage into the open air, he had felt severe pain in the heart, extending to the left scapula and arm, and accompanied by a momentary reeling and slight obscuring of the senses. These symptoms had occurred with unusual severity two days before his admission, after drinking some spirits, and were followed by a death-like syncope; from which, however, he was quickly and completely restored by a large bleeding.

On the day of his admission the patient was found suffering from severe dyspnœa, of a character which led the author at once to suspect a disease of the heart, although it was combined with a number of anomalous symptoms that could only be referred to hypochondriasis. On percussion, both sides of the chest had a natural resonance; but in the median line and the adjacent part

of the left side, there was dullness over even a greater extent than in most cases of hypertrophy with hydropericardium. The movements of the heart were scarcely perceptible to the hand placed on the cardiac region, but posteriorly they were even more sensible than natural. The pulse was in no degree intermitting, but small, sometimes very frequent, at others quick, and very hard. The patient was bled to twelve ounces and had other antiphlogistic remedies, and on the following day was much better. On the 23d, however, all his symptoms were worse than ever; he had excessive dyspnœa, severe pain in the heart, constant dry cough, and frequent faintings. The heart could be only very obscurely felt in front, but close by the vertebral column its action was more than ever distinct; the pulse was still very small and hard, and the left hand and forearm were œdematous. The patient was repeatedly bled and subjected to severe antiphlogistic treatment, but all his symptoms continued or grew worse; the œdema spread gradually over the whole body, and on the 31st he died suddenly in a fit of syncope.

The body was examined thirty hours after death. The contents of the skull presented nothing unusual. The pleural cavities contained a large quantity of serous fluid; the lungs were both œdematous, and their lower lobes were much compressed both by the fluid and the immensely dilated pericardium. This sac was evidently distended by some dense consistent fluid containing small grumous masses, and pressing the heart unnaturally far backwards. On cutting it open along its anterior wall, a large quantity of grumous blood and serum flowed from the pericardium; and "at the aperture thus made there soon appeared a body whose surface was of a whitish colour, beset with innumerable uniform oblong appendices, which might remind one of the surface of the villous heart, as described by pathological anatomists. This pseudo-organism, which was of a conical form, was fixed on the heart, and adhered by its base to the base of the ventricle and the right auricle: it was as large as the heart itself. On washing the parts, it was found that the base of this tumour adhered to the parts just mentioned by false membranes, and that it had obtained by them an adhesion to the right coronary artery, in which there was a laceration about two lines in length with elevated edges;" through which the blood had been poured into the pericardium, and, becoming organized, had been developed into this remarkable tumour. At the left side the adhesions by which the tumour had before been attached to the heart had been broken down, and thus the exit of the additional quantity of blood which had been found in the pericardium had been permitted. The heart itself, and its valves, were perfectly healthy, but the aorta and its great branches presented signs of arteriasis, and there was a cartilaginous and osseous elevation close by the origins of the coronary arteries: the pulmonary artery and its branches were similarly diseased. The contents of the abdomen were generally healthy.

To this case, which we believe is unique, the author has added very lengthened observations, of which, however, those only are sufficiently important which refer to the probability that the rupture of the coronary artery and the first effusion of blood took place when the patient was seized with the very severe symptoms two days before his admission, and that the second and fatal effusion occurred at the instant of the fainting fit, in which the patient's life was terminated. It is to be regretted that the general condition of the coronary vessels is not mentioned.

Annali Universali di Medicina. Decembre, 1839.

On Tubercles of the Larynx and Fauces. By Dr. POPKEN, of Jever.

THE disease to which I have given this name, at the risk of its being objected that it does not consist of true tubercles, but only of diseased mucous follicles, has often presented itself to me since my attention was first drawn to it, and exhibits so much that is peculiar in its phenomena and course that I cannot refer it precisely to any of the generally known chronic diseases of the larynx. I have never seen it before puberty, nor in advanced age, but always in young subjects,

and chiefly in males; neither could I ever trace any connexion between thi any constitutional chronic diseases, as syphilis or scrofula. I have so r found it coincident with other local affections, and especially with those o lungs, that, in a doubtful diagnosis, I rather regard this disease in the thro a sign that there is *not* a vomica in the lungs.

In general the patient, in whom this peculiar alteration of the mucous n brane lining the fauces and larynx exists, begins, without any evident caus cough and hawk very frequently: but he complains of no pain in the aff part, and is usually in perfectly good general health, so that the whole dis appears like an obstinate cold, and is annoying only from the incessant irrit that excites the hawking. The disease may thus continue apparently wit alteration for weeks or months, till at last (and usually in the morning drinking) the ejection of one or more small masses of an elastic matter, just chalcedony in colour and semitransparency, attracts the patient's attention, though followed by temporary relief, makes him anxious.

If the fauces be examined at this period of the disease, one finds the who their posterior wall thickly beset with small, round, bright red, semitransp bodies, varying in size from that of a pin's head to that of a pea, and just lik granulations which Laennec and Louis describe as the commencement of tu cles. On some of these granulations one sees adhering firmly to them the op mucous globules already mentioned, and just similar in size and form to bodies beneath them. This granulated condition of the mucous membr which, as far as I know, has not been before remarked by any one, extends d as far as the eye can reach, and is always a certain sign of the existence of lar tubercles in the mucous membrane of the larynx, and of threatening la geal phthisis; for, although the latter disease does not necessarily, and still rapidly follow, yet instances of its occurrence, as a signal of the milder a tion just described, are not wanting.

This fatal result of the disease, however, may, the author asserts, be al averted by early and judicious treatment. The means which he has empl with the best success are a nutritious, but not stimulating diet, constant war round the throat, and the administration of aromatic and tonic medicines, as ammoniacum and myrrh, or myrrh, extract of bark, and sulphate of iron

[The disease described by the author must be familiar to every practitio but we are not aware that the anatomical condition on which it depends been before pointed out, though it is distantly alluded to by Henle (U Schleim-und Eiterbildung, in Hufeland's Journal), who found the expector substance to consist of morbidly altered epithelium cells. The name which author has given it is most objectionable, expressing only what the disea *not*; it is an affection of the mucous membrane, exactly analogous to acn the skin, and its name should express, if possible, that it has its origin in di of the mucus-glands.]

Casper's Wochenschrift. August 22, 1

Dropsy after Scarlatina, unaccompanied with Albuminous Urine.
By Dr. Philipp, of Berlin.

During the spring of the year 1840, scarlet fever was very prevalent in Be especially in the northern and eastern parts of the city. A hundred c of the disease came under Dr. Philipp's notice, who found that it resembled late London epidemic, in being invariably followed by dropsy. Some case the fever, indeed, terminated fatally from head affection; in other, and more merous instances, the patients died from putrid sore throat; but whenever was not the case, dropsical symptoms, more or less severe, occurred from twelfth day up to the fourth or fifth week after desquamation had commen In some children, too, dropsy came on without being preceded by any sy toms whatever of illness.

There were two points in which the Berlin epidemic differed from that wl

prevailed in London, one was, that, instead of being a fatal disease, not one child died; the other, that, although the author tested the urine in sixty cases, he never detected the presence of albumen by heat alone, and it was only in a few cases that nitric acid gave signs of its presence.

He states, as in some measure accounting for this, that albuminous urine in dropsy is very rare in Berlin; for, during two years in which he has been physician to the poor in one district of the city, although he has had 150 new cases in a month, he has met with but two instances of Bright's kidney.

Casper's Wochenschrift. Aug. 29, 1840.

Case of Idiopathic Lymphadenitis. By Dr. POGLIAGHI, of Milan.

C. C. aged forty-eight, a man of robust constitution, but of very debauched habits, after a few days' illness, was attacked by a troublesome inflammation, which appeared first at the sides of the face, extending from the front of the ears along the margins of the lower jaw, and meeting together at the chin. Emollients were used without benefit, and fever coming on, a physician purged and bled him twice. Being in some measure relieved by these means, the patient neglected himself and suffered the disease to make slow though scarcely perceptible progress. After some days of apparent calm, small almost painless tumours appeared at the lower part of the neck; to these succeeded others in the axillæ and the groins, and after these, fresh ones in the hams and at the bends of the elbows; all of which in the course of three days had increased so as to render the movements of the joints inconvenient. The patient now resorted to various quack medicines, and at last growing worse was sent to the infirmary, having been ill altogether about seven weeks.

At first sight it was supposed that he had the disease known by the name of *orecchioni* or of parotiditis rheumatica, for his face presented at either side a slightly painful swelling, commencing in front of the meatus auditorius externus, following the lower margin of the jaw, and meeting that from the opposite side at the chin; and feeling as if it were formed by an agglomeration of divers lobes. The further examination found some tumours as large as pigeons' eggs at the base of the neck, others much larger in the axillæ, others as large as those in the neck at the elbows, and again others as large as a strong man's fist in the groins, and of various sizes in the hams. The tumours on the left side were generally larger than those on the right; the skin covering them all had its natural colour, and there was no increase of heat. Motion of the joints was painful. The patient had slight fever and rather a full pulse; his face was livid red, and his eyes prominent as if he had a ligature on his neck; his head was heavy, his tongue coated, his appetite not destroyed, his respiration free but accompanied by frequent sighing, his abdomen presenting nothing morbid.

The patient was bled nine times (for reasons which are given in detail), leeches were twice applied to the neck, and mild purgatives, followed by pills containing calomel and extract of aconite, were given. After the first bleeding, (from which as from all the rest the blood was buffed), the patient seemed somewhat relieved, and the tumours appeared less hard; but subsequently the fever became more severe and had regular evening exacerbations, and the patient had frequent attacks of dyspnœa. At first these were slight and occurred most frequently in the night, but afterwards they become more severe and prolonged, and in one of them he died suddenly, nine days after his admission into the hospital, and about two months from the commencement of his illness.

At the examination of the body, some of the enlarged glands were found in a state of incipient inflammation, others with points of beginning suppuration at their centres, and others transformed into cysts containing a substance of the consistence of cream and of the colour of dregs of wine. In some of these last the cavity was as large as a pigeon's egg, and in two in the axilla it would have contained a hen's egg. The alteration had advanced furthest in the glands of the left axilla, less in those of the left groin, and still less in those of the right groin. The thoracic viscera were healthy except for a slight serous

effusion in the right pleura; but the glands at the bifurcation of the trac were enlarged to the size of a big fist. Internally they presented the vari degrees of the inflammatory process observed in the other glands, though in a advanced state; some presented points of suppuration, but the matter had the lurid wine-dregs-colour seen in the glands of the external parts of the bc In the abdomen nothing anormal was found except incipient inflammation the mesenteric glands. *Annali Universali di Medicina. Marzo*, 18

On the State of the Blood in different Diseases. By MM. Andral and Gavarr

At the sitting of the Royal Academy of Sciences on the 27th of June 1 M. Andral read in his own name and that of M. Gavarret a first memoir on variation of the blood in connexion with diseases. This work is the re of examinations of the blood of 200 patients, and 360 bloodlettings. The p ceeding followed by the authors is that which has been pointed out by M Prevost and Dumas.

In 1000 parts of blood, the fibrine has been found to vary in proportion fr 1 to 10, the globules from 185 to 21, the solid matters of the serum from 10 57, the water from 915 to 725. It is very rarely that the different princi of the blood are simultaneously augmented or diminished under disease. M frequently one or other of these principles is alone altered, but sometimes are modified at the same time, and then it is in an inverse sense; thus, when fibrine increases the globules diminish, and the contrary, and thus there res a remarkable change in the relations of quantity, which these principles sho preserve with each other.

The diseases considered in relation to the changes which arise in the com sition of the blood are divided into four classes. The first comprehends diseases in which the fibrine is constantly augmented, as the phlegmasiæ. ' second includes the diseases in which the fibrine is never augmented and o diminished, as the pyrexiæ. In the third class are the diseases in which tl is a uniform diminution of the globules, as chlorosis. Lastly, in the fo class are arranged the morbid states or fundamental alteration of the blood which the albumen of the serum is diminished, as in Bright's disease. But is not all; the facts are not always thus simple, and it is often that many mo acts, each of which causes a different modification in the blood, complicat For instance, when pneumonia seizes a chlorotic female, the blood still cont a small quantity of globules, but the quantity of fibrine is augmented. " have seen," say the authors, "these results produced so frequently, that if should find in the blood of any patient that there was more than five of fibr we should not fear to affirm by this alone, in that patient, a complication of of the morbid states comprised in our first class, and on the other hand, wl we found less than two of fibrine instead of more than five, we should deny existence of such species of complication."

Besides diseases, losses of blood and privations of food notably modify the c position of the blood and add their influence to that of the disease. This fa generally admitted; but the researches of MM. Andral and Gavarret have l directed to determine in what manner the composition of the blood is modi Abstinence and losses of blood act principally by diminishing the globules. the diseases in which the authors practised bloodlettings, these constantly dered the number of globules less and less considerable in proportion as were repeated. But, it is to be remarked, that the globules did not dimi from one bloodletting to the other in the same proportion in all the patie in one the globules lose scarcely 2 or 3, but in another from 30 to 40. while in all cases bloodlettings diminish the quantity of globules, the fib most frequently preserves its own proportion, being rarely diminished sometimes increased. When the disease is of such a nature that an increas the proportion of the fibrine is one of its necessary elements, this incr occurs notwithstanding the bloodletting and the diminution of the glob In order that the losses of blood may have the power of diminishing the

portion of fibrine, they must be very considerable, and as in this case the globules are greatly diminished, it follows that all the solid elements of the blood are simultaneously diminished.

With this general exposition, M. Andral passes to the consideration of the facts relating to the alterations of the blood in the four classes of diseases which he has established.

FIRST CLASS. *Diseases in which the fibrine is augmented.* The augmentation of the proportions of this element has been established in two orders of diseases, namely, in the phlegmasiæ and in tubercular phthisis. The phlegmasiæ in which the blood has been examined are articular rheumatism, pneumonia, capillary bronchitis, pleuritis, tonsillitis, erysipelas, cystitis, acute suppuration of the lymphatic glands, and a furuncular eruption with fever. The blood has been examined in eighty-two individuals attacked by these diseases, and in 153 bloodlettings which were practised upon them. In all the cases where the diseases showed themselves in their acute form and were accompanied by fever, a very notable increase of fibrine was always found, varying in different diseases and different cases of the same disease. Thus, taking the proportion 3 as a normal medium for the fibrine, the following degrees of elevation were found: 1. In acute articular rheumatism, the mean quantity of fibrine varied between 7 and 8, the minimum being from 4 to 5, the maximum 10. 2. In pneumonia, the mean quantity of fibrine was the same as in the acute articular rheumatism, the maximum and minimum being the same. 3. In acute capillary bronchitis, the mean quantity of fibrine is not so great as in the two preceding diseases—it was between 6 and 7, the maximum being below 9. 4. In acute pleurisy, the mean quantity of fibrine again descends; it was from 5 to 6, the maximum not passing beyond 6. In acute peritonitis, the mean is also between 5 and 6, the maximum being 7. In the other diseases named above, the mean was about 5. In no case was the minimum below 4, and very rarely 5.

Thus in all the diseases called inflammatory, in which the blood has been examined, whatever their seat and degree of intensity, the fibrine has notably exceeded its normal proportion, the limits being 5 on one side and 10 on the other. But to support this rule there must be the double condition of acuteness and fever. For if the disease be primitively chronic or has become so, if fever has not existed or has disappeared, the fibrine ceases to be in excess in the blood. As to the state of acuteness, the increase in the proportion of fibrine is ruled by the intensity of the local symptoms and of the febrile disturbance. No acute inflammation produces more fibrine than pneumonia, and, after that, than acute articular rheumatism.

As the inflammation declines, the quantity of fibrine diminishes; if after recovering the patient relapses, the fibrine augments again. And if any acute inflammation supervenes during the course of any disease, it is immediately marked by an augmentation in the fibrine of the blood.

The globules in no case undergo increase in consequence of the state of phlegmasia; indeed in the commencement of affections of this class the globules rather appear to have diminished. In all the phlegmasiæ they decrease in proportion as the disease is prolonged, and the patients have been subjected to diet and bloodlettings. A great diminution in the proportion of the globules does not prevent inflammation from arising, increasing, or arriving at the fullest development; and on the other hand, a greatly increased proportion of the globules does not appear to favour its production. The observations of MM. Andral and Gavarret indeed show that inflammation is compatible with the most variable proportions of globules, as from 148 to 60.

In all these inflammatory diseases, the solid materials of the serum presented no alteration worthy of remark. The water varied from 771 to 840.

We have said that it is not only in the blood of individuals attacked with inflammation that an augmentation of fibrine occurs, but also in cases of pulmonary tubercles. At whatever period of phthisis the blood is examined, there is an augmentation of fibrine and a diminution of the globules; but the elevation

of the first of these elements and the depression of the second are not equally marked in all the phases of the disease. When the tubercles are in the crude state, the fibrine is not considerably augmented, the mean proportion being about 4, and the diminution of the globules, though perfectly manifest, is not very great. When the tubercles begin to soften, the fibrine has a mean of 4½, and the globules continue to descend. When the lung is broken up by caverns, the fibrine has a mean of 5, and is sometimes elevated to nearly 6, but it never reaches the mean of pneumonia. When the disease has reduced the patient to the last degree of marasmus, the fibrine begins to decrease with the other solid elements of the blood, and descends below the normal proportion. In general, the greatest excess of fibrine in the blood of phthisical patients is shown when continued febrile disturbance is established.

The globules, in an inverse ratio with the fibrine, become less and less abundant in this period of phthisis. In the first stage of the disease they continue below 100, and never reach their mean quantity. In the second stage the proportion is generally depressed below 100, and in the third below 81. This diminution is doubtless very notable, but it is less than that of chlorosis.

The solid materials of the serum vary in phthisis between 64 and 98; the proportion 64 was afforded by a patient in whom the fibrine was only 2. The water is more abundant the more advanced the period of the disease. It varied from 784 to 845.

Second Class. *Diseases in which the fibrine is in its normal or in a diminished quantity, while the proportion of the globules is normal or augmented.* This class is divided into two orders: 1, the pyrexiæ or fevers; 2, many congestions and hemorrhages.

In *continued fevers*, the fibrine is never augmented and often diminished, sometimes even to a thousandth. The globules are never diminished before bloodletting, and frequently they are augmented to a degree exceeding 140. When simple continued fevers exist without appreciable inflammatory action, the modifications are those just stated. In one case, where all the symptoms characterizing inflammatory or *angéioténic* fever were present, the globules reached the enormous proportion of 185, and yet the fibrine maintained its normal proportion.

The authors apply the term *typhoid fever* to continued fevers, exhibiting at first an exanthematous affection and afterwards ulceration of the intestinal follicles. In consequence of the inflammatory condition of the intestinal alteration which forms the anatomical character of typhoid fever, it might be naturally supposed that the blood in this disease would, more or less, retain the qualities of the blood in the inflammations; but this is not the case, for whatever may be the intensity of the intestinal inflammation, the blood never takes these characters. In typhoid fever, at whatever period the blood is examined, and this examination has been made from the fourth to the twenty-first day, the fibrine is never found elevated above its normal proportion. It often preserves this point, but is frequently depressed below it, thus affording results directly opposite to those observed in the inflammations. Again, in inflammation the fibrine is augmented in a direct ratio with the intensity of the disease, but in the typhoid lesion, on the contrary, the fibrine diminishes in a direct ratio with the severity of the fever, and it may fall to below a thousandth. Of all diseases the typhoid fever is that in which the proportion of fibrine has been found to descend the lowest. As to the globules, whilst in inflammation they frequently have a slightly elevated proportion in the commencement of the disease, in typhoid fevers the tendency is reversed. Thus towards the eighth day, it is not rare to find the proportion of globules from 140 to 150, whilst in acute rheumatism and pneumonia at the same period, they are seldom elevated above 130. Now, at a similar period from the commencement of typhoid fever, notwithstanding bloodlettings and diet, we frequently find the globules maintain a proportion much above 130. And it is very remarkable that this elevated proportion has never been wanting or ceased to exist, that the fever has not been

less severe or its progress more slow. We see, then, that typhoid fever is a morbid state infinitely more complex than an inflammation.

In the *eruptive fevers* (variola, varicella, rubeola, scarlatina,) the fibrine descends to 1 and never exceeded 4, and this maximum was only once observed. It is singular that in a disease, as variola, where the skin becomes the seat of an abundant suppuration, the blood, according to the law of inflammation, should not offer an augmentation of fibrine. But the cutaneous inflammation of variola, like the intestinal inflammation of typhoid fever, is nothing more than a simple element of a general predominant affection, the character of which is taken by the blood. The globules are considerably augmented in many cases of rubeola and scarlatina, to the proportion for instance of 146; on the contrary they are not augmented in a sensible manner in any one case of variola.

Cases of *intermittent fever* were rarely met with, and only negative results obtained.

In answer to the question whether "In inflammation is it the fever or the inflammation which produces an augmentation of fibrine in the blood?" the authors reply that "it is the inflammation, and that without the occurrence of the local lesion which constitutes this state, fever alone, whatever may be its intensity and duration, has not the effect of augmenting the quantity of fibrine contained in the blood." In the majority of *congestions and cerebral hemorrhages*, but not in all, the fibrine was found below its normal proportion, whilst the globules preserved or exceeded theirs; and this result was the more evident the nearer to the commencement of the disease the blood was examined.

THIRD CLASS. *Diseases in which the globules of the blood are diminished.* The memoir contains a number of facts relative to the morbid states of which this diminution is a constant character, as dropsies, the sequelæ of intermittent fevers, and the cachectic state presented by workers in lead, but we can only give the details on chlorosis. In this disease there is a first degree which is so slightly characterized by external signs, that at the first onset one might take the young women who are attacked for plethoric persons; but this is a false plethora which is in some degree announced by the state of the blood, as on analysis there are found less globules than in the normal state, though the diminution is as yet inconsiderable. In the advanced stages of the disease the globules are diminished to a degree not found in any other disease, unless the whole system has been exhausted by accidental or abundant hemorrhages. In one of these latter cases, cited in the preceding memoir, the proportion of the globules descended to 21; in chlorosis, the mean proportion has been found depressed from 127, its normal standing, to 38, but generally it is about 50.

After iron has been administered to chlorotic patients for a certain time, if we again examine the blood, we find the proportion of globules risen again. Thus in one case under the influence of this remedy, the proportion promptly mounted from 46 to 95. The other elements of the blood (except the water which increases in consequence of the diminution of the globules) remain completely unchanged. Thus the solid matters of the serum, varying from 94 to 75, are maintained in their normal proportion; and the fibrine does not descend with the progress of the disease, nor increase under the action of iron. This, however, applies to simple chlorosis, for if an inflammation complicate this affection, it will be marked by an increase in the proportions of fibrine.

FOURTH CLASS. *Diseases in which the albumen of the serum is diminished.* When the secretion of the kidney is so modified that the urine is mixed with a certain quantity of albumen, this principle is found in a less proportion in the blood. The authors found that the serum contained from 50 to 60 of albumen, instead of 72, the normal medium. In the different cases included in the fourth class, the other constituents of the blood presented accidental modifications in connexion with accidental causes. Thus in one of these cases, an acute inflammation supervening upon the principal disease greatly augmented the quantity of the fibrine; in another case, the prolonged privation of aliments greatly diminished the quantity of globules.

Archives Générales de Médecine. Août et Septembre. 1840

Acute Ovaritis induced by Uterine Injections. By M. Leroy-D'Etiolles.

"This disease," says M. Leroy, "has been as yet very little studied. Having had occasion to observe two examples supervening after uterine injections, I have detected some characters which authors have not assigned to it. Most generally it follows labour, but in the two cases which I have observed the cause was very different. The most prominent symptoms were sudden pain and rapid development of a tumour on one side. The pain comes on in fits, it is very severe, and compared by the females to the pains of delivery; but, notwithstanding its great intensity, it is not augmented by pressure, differing in this respect from the pain of peritonitis. In no other disease have I observed tympanitis so considerable which contributes still further to the sufferings of the patients. It shows itself on the second or third day, and is combated by powerful purgatives."

In the two cases reported by M. Leroy, the injections were passed with moderate force into the uterus, by the aid of a gum elastic sound. In one the quantity of marsh-mallow water was ten drachms. In both cases the liquid had scarcely arrived in the uterine cavity, when the patients complained of an acute pain in one side. In one case the disease was regarded as a metro-peritonitis occasioned by the escape of the liquid into the peritoneum; but M. Leroy thought it was inflammation of the tube and ovary on the side corresponding to the pain. The symptoms he observed in the first case enabled him to recognize it in the latter. Neither of the patients died, but in both the sinking down of the ovarian tumours, which could be felt by the finger in the vagina, was accompanied by an abundant discharge by the anus.

Gazette Médicale de Paris. Septembre 5, 1840.

SURGERY.

On Itch and its Treatment. By Dr. de la Harpe, Chief Physician of the Hospital of Lausanne.

The author gives the following formula for an ointment which he has employed in upwards of 400 patients. It does not appear to irritate the skin, and is said to be better than the liniment of Valentin or the alkaline sulphur ointment of Alibert. The mean duration of treatment was eighteen days in 1836, fifteen in 1837, eleven in 1838, and ten in 1839. The formula is as follows:

Flowers of sulphur - - -	16 parts.
Sulphate of zinc - - - -	2 ,,
Powder of white hellebore -	4 ,,
Soft soap - - - - - - -	31 ,,
Lard - - - - - - - -	62 ,,

Gazette Médicale. Juillet, 1840.

On the Use of Mercurial Plaster ("Emplâtre de Vigo") in preventing Deformity after Smallpox. By M. Chomel.

A young girl is under M. Chomel, in the Hôtel Dieu, who has been vaccinated, but she says without effect. The variolous eruption, which is demiconfluent, was preceded by the ordinary symptoms. A mask, made with the plaster of Vigo, was applied to the face on the second day of the eruption. The patient tore it off within twenty-four hours, but, notwithstanding this, the result is very remarkable. On the neck, chest, and all the rest of the body, the variolous pustules were developed with all their distinguishing characters: opaque, umbilicated, and surrounded by a bright red areola. But on the face the course has been very different; instead of pustules we see acuminated vesicles, or solid papulæ. On some points where the plaster had not adhered, small pustules are

perceived; where it had, we search for them in vain. It appears impossible, on the simple inspection of this patient, to state that mercury has not exercised a local action, specific and advantageous; for the patient will not be scarred, and will be speedily convalescent.

In another ward is a patient already convalescent, who has been submitted to the same mercurial treatment, and the desquamation has followed its ordinary course, except on the face, where scales have not formed. In another bed in the same ward is a woman five months and a half advanced in pregnancy, in whom the plaster has entirely averted the eruption from the face: small whitish papulæ supplying the place of the pustules. Desquamation has not taken place, except on the lips and eyelids, where some pustules appeared; but the mask of plaster did not extend to these parts. The treatment is well worthy of further trial.

Bulletin Général de Thérapeutique. 15 *et* 30 *Août*, 1840.

On the Employment of large Doses of Tartarised Antimony in the Treatment of Articular Dropsies. By M. GIMELLE.

M. GIMELLE, who some time ago published a series of facts, illustrative of the utility of tartar emetic in hydarthrosis, has continued his observations, and finds that when an articulation is the seat of synovial effusion, the same treatment is the most effectual for procuring speedy resorption. In twenty-eight cases the emetic tartar was given in increasing doses, commencing with four grains in the twenty-four hours, and increasing two grains daily till the dose was sixteeen, eighteen, or twenty grains per diem, with the invariable effect of determining the resorption of the liquid in a space of time, varying from eight to sixteen days. Of twenty-eight cases of effusion of synovia into articulations, twenty-two had their seat in the knee-joint; three were double, and two were in the shoulder-joint; one was in the elbow, and one in the ancle. All the patients took the emetic tartar in infusion of linden tree with syrup of poppies. In eighteen the tolerance was established on the first day, in two on the second, and in two on the third. No accident occurred to any of the patients after the tolerance was established. The dose of twenty grains was never exceeded, and in all the cases the effusion was absorbed in the space of eight, ten, or sixteen days—longest period during which the remedy was administered. In twenty-five cases the pain and stiffness felt in the affected articulations diminished at the same time and degree with the effusion, and when the latter had disappeared, the patients could walk as easily as before they were attacked by the disease. In two cases, however, though the liquid disappeared in the ordinary time, the pain remained in one case for a month and in the other for forty days. In one case the remedy was carried to twelve grains without benefit, and as it was one of very old standing, it was thought proper to relinquish the medicine.

We give the following cases, as they are very interesting proofs of the value of the treatment employed.

CASE I. A man, aged seventy-three, was affected by a very large hydarthrosis of the left knee, which extended into the hollow of the ham, where it formed a tumour of the size of the fist, which disappeared on strong pressure, and became visible on each side of the patella. M. Pasquier prescribed the emetic after the form of M. Gimelle. The dose was successively carried to 16 grains; the patient experienced no inconvenience, his appetite continued good, and on the sixteenth day all the signs of effusion into the articulation had disappeared. Two days afterwards the patient was dismissed entirely cured.

CASE II. On the 10th of September M. Gimelle was called to a student, aged twenty-one years, with an hydarthrosis of the right knee, which had been treated without success during six weeks by leeches, blisters, compression, frictions, and embrocations. M. Gimelle prescribed the tartar emetic. The patient's appetite continued good, and he had no inconvenience. The dose of twelve grains was not exceeded, and fourteen days after this treatment was commenced there

was no trace of synovial effusion, the patient feeling merely a feebleness of the limb.

Case iii. A healthy female, aged twenty-three, in a journey from Tours to Paris, caught cold during the night, and, on her arrival in the capital, experienced pains in the right knee. M. Gimelle considered this case one of commencing hydarthrosis, and prescribed without delay a gummy potion with four grains of tartar emetic and an ounce of syrup of poppies. Five or six vomitings, and afterwards alvine dejections followed, but the pain was relieved in the night and the patient could move the limb. On the second day the catamenia appeared, and the emetic was suspended for five days, during which the pain reappeared and the effusion increased, and the articulation became very red and hot. The emetic was then resumed, four grains being given on the first and second day; it produced three or four vomitings, and as many stools. Tolerance was established on the second day, there was a diminution of the synovial tension, and the patient could bear slight movements of the limb without much pain. On the third day six grains were given, eight on the fourth, ten on the fifth, twelve on the sixth, and fourteen on the seventh; the tolerance continued, and the amelioration was progressive. On the eighth, ninth, and tenth days, sixteen grains were given, and on the tenth day the synovial effusion had completely disappeared.

In none of these cases did M. Gimelle precede the employment of the tartar emetic by local or general bloodletting. Nevertheless, he thinks that if the fever be intense, and the articulation present great heat and redness, or if there be great irritation of the digestive organs, it would be proper to combat these symptoms before administering the emetic tartar. By this preliminary treatment we should diminish the chances of gastric irritation, probably facilitate the tolerance, and consequently the action of the remedy.

The most constant effects of the tartar emetic were diminution in the force and rapidity of the pulse, enfeebling of the voice, fatigue and coloration of the eyelids (known by the name of *yeux cérnes*), and abundant perspirations during the night. Five patients had vomitings, two during one day, one two days, and two during three days. Eight had very abundant alvine dejections, lasting from one to three days: in three the vomiting and purging coexisted. Sixteen experienced neither vomiting nor purging. In the majority the appetite was unaltered; and in those cases where it was disturbed, it was reestablished with the tolerance. The quantity of urine was diminished, which M. Gimelle attributed to the abundant perspirations. All the other functions were performed as in the healthy state. The quantity of food the patients had taken when in health was not diminished, and often increased. Lastly, M. Gimelle saw all the patients some months after treatment, and many of them after some years, and in none did any accident occur.

Bulletin Général de Thérapeutique. 15 *et* 30 *Juillet*, 1840.

Case of immense Cæcal Fistula in the Lumbar Region.
By Dr. Meding, of Meissen.

A strong man, aged thirty-six, suffered for some days from griping sensations in the abdomen; on the sixth day I found him with the most complete symptoms of a severe psoitis of the right side, which I believed was the result of either rheumatism from cold, or of a sprain from over-exertion. General and local abstractions of blood, several times repeated, mitigated the pain, but produced no essential change in the state of the patient; a constant restlessness prevented him from sleeping; his pulse was accelerated, but full; his breathing anxious; his tongue moist and furred; he was thirsty and had lost his appetite; his skin was alternately cold and hot; his bowels very loose; his urine depositing an abundant red sediment.

The severe pains in the back from the right lumbar region to the shoulder, and forwards to the testis and the thigh, as well as the great tenderness strictly

limited to the right iliac and inguinal region, suggested an affection of the cæcum. But although as the disease went on, there was every reason to believe that effusion or suppuration must have taken place, yet no part could be found at which the matter might be evacuated. Only on the back, the common belly of the sacro-lumbalis and longissimus dorsi appeared somewhat prominent, though without any alteration in the colour or elasticity of the skin. At this part however the author determined to make an exploratory incision, and on the 24th day of the disease he cut along the outer edge of the common belly of the muscles just mentioned for four inches upwards from the crista ilii, through the skin and a thick layer of fat down to the external oblique. All these tissues were perfectly healthy; but on introducing a lancet an inch deep into the fibres of the abdominal muscles, a little pure pus gushed out with the blood, and was soon followed by a stream of blackish-gray ichor of a most fetid macerated-bone-like smell. For several days the discharge continued to be very profuse, and the patient became extremely weak. Every day there came away with the acrid irritating ichor masses of cellular tissue, partly from the interstices of the outer muscles of the back, and partly from the fat and cellular substance surrounding the kidney and psoas muscle. The finger could now be easily passed over the transverse processes to the anterior surface of the bodies of the lumbar vertebræ, and the psoas and iliacus muscles could be distinctly felt quite cleared of cellular tissue. Such examinations always excited the severest cramps of the chest, and twitchings of the right lower extremity. With a long probe it was found that on the inner side the whole lumbar region of the abdomen behind the peritoneum from the kidney into the cavity of the pelvis, and on the outer side the interspaces of the dorsal muscles from the inferior angle of the right scapula to the middle of the outer surface of the ilium, formed one cavity, the source of the putrid fluid.

For six weeks after the operation the condition of the patient seemed hopeless; he became weaker and more emaciated, had severe spasms of the pharynx and rectum, and was in a state of utter prostration. At the end of this time, however, and quite unexpectedly, the sloughing away of the cellular tissue gradually ceased, the wound assumed a more healthy aspect, the matter discharged became thicker and more purulent, acquired a stercorous odour, and soon after was found to contain small portions of fæces. At the same time the general condition of the patient improved.

The whole of the cavity above described was filled twice a-day with dry charpie pushed into it as far as possible with a long and thick piece of whalebone. On passing the latter as a probe forwards and downwards over the crista ilii, one came in the neighbourhood of the fifth lumbar transverse process, immediately at the outer edge of the quadratus lumborum, to an aperture as big as a large quill, through which the whalebone passed to a depth of six or eight inches, till it was stopped by a soft yielding substance. There could be no doubt that this hole led into the cæcum; and it was found that by closing it accurately with charpie, and keeping the patient on his left side, with a careful diet and open bowels, the evacuation of fecal matter through the wound was entirely prevented. With some unimportant interruptions, the gradual closure of the cavity went on steadily after the eighth week from the operation. The quantity of charpie necessary to fill it up (which had at first been very considerable) decreased with the diminution of the discharge; the thigh which had been fixed in the flexed posture became more and more moveable; and all the functions of the body were gradually restored to their natural state. In rather more than ten months from the day on which the cavity was opened, the external wound was completely healed; no relapse took place; the muscles which had been implicated in the disease gradually gained power, and in rather less than two years from the commencement of his malady, the patient was restored to his former robust health.

Von Ammon's Monatsschrift für Medicin, u. a. Juli, 1840.

Subcutaneous Section of Forty-two Muscles, Tendons, or Ligaments, practised the same day, on the same person, to remedy a general articular deformity. By M. Jules Guerin.

In a letter addressed to the Academy of Sciences on the 31st of August, 1840, M. Guérin states that on the 25th of that month he performed the section of forty-two tendons, muscles, or ligaments, to remedy a series of articular deformities of the trunk and limbs, caused by the active retraction of these muscles and ligaments. There were twenty-eight openings of the skin required. The following were the parts divided:

Trunk	Pectoralis major	1
The arms	On each side biceps cubiti	2
	,, pronator teres	2
	,, extensor carpi radialis	2
	,, flexor communis sublimis	2
	,, palmaris brevis	2
The forearms	Tendons of extensor carpi ulnaris on each side	2
	,, palmaris longus and brevis	4
	,, abductor pollicis	2
The legs	The sartorius on each side	2
	The biceps cruris	2
	The semi-membranosus	2
	The semi-tendinosus	2
	The rectus femoris	2
	Fascia lata	1
	External lateral ligaments of knee	2
The feet	The tendo Achillis on each side	2
	The tibialis anticus	2
	The extensor communis	2
	The extensor proprius pollicis	2
	The peronei antici	2
		42

The patieut only experienced moderate pain or fatigue, and did not complain during the operations, which lasted an hour. An hour afterwards he was in a sound sleep. He was very tranquil the following night and day. No inflammatory accident supervened, and on the third day, the twenty-eight wounds were completely cicatrized. The sections were made in the presence of many distinguished French and foreign surgeons. M Guérin holds that he is not open to the charge of rashness, as he first established the absolute innocuousness of subcutaneous wounds by numerous experiments on animals, and then verified the same principle in man by a series of operations from the section of one muscle to that of a great number. He purposes to lay an account of his method of operating with its definite results before the Academy in a future memoir.

Gazette Médicale de Paris. Septembre 5, 1840.

On a New Species of Torticollis. By M. Bouvier.

This species of torticollis differs altogether ftom the acute or chronic muscular form, and from the spontaneous luxation of the atlas and axis, three states with which it has been confounded. It has its seat in the articulations of the first cervical vertebræ, and may perhaps be designated by the term articular torticollis. The lateral articulations, right or left, of the first cervical vertebræ, and especially of the atlas with the axis, are, so to speak, the focus of articular torticollis, which consists in a peculiar form of inflammation of the synovial capsule and fibrous tissues of these articulations. The essential characters are: 1. A position of the head and neck, similar to that produced by

contraction of one of the sterno-cleido-mastoid muscles, namely, lateral flexion to the right or left, and rotation of the face to the opposite side. 2. A pain felt towards the top and sides of the nape of the neck, sometimes on the lateral and posterior parts of the cranium; arising spontaneously, or in the movements of the neck. 3. Complete relaxation of the sterno-cleido-mastoid of the affected side, and an equal tension of the muscles of the neck both left and right. 4. Stiffness and pain in all or any of its movements, notwithstanding the preservation of the articular connexions of the cervical vertebræ, and the integrity of the muscles. 5. Absence of swelling, and of sensibility on pressure. 6. Atrophy, or arrest of development of the side of the face, which answers to the inclination of the head, when this has lasted a certain time.

This disease presents two successive states or periods, the acute and chronic. It is often produced by cold, sometimes by sudden distension of the ligaments. M. Bouvier thinks it very important to detect this disease, as the section of the muscle would be useless and rash, but reserves to another occasion the proceedings which he has found useful.

L'Expérience. 17 Septembre, 1840.

Wound of the Urinary Bladder. By Dr. SCHÜTTE, of Mullheim.

A healthy man, thirty-seven years old, fell perpendicularly from a height of about eight feet, on an upright wooden stake several feet long and fully an inch thick. Its end passed into the inner surface of the left thigh, about three inches from the rectum, and ran into the lower part of the urinary bladder above the sphincter vesicæ. The urine flowed continually and insensibly through the wound; but neither blood nor urine passed through the urethra. A catheter was placed in the urethra, and several leeches and lotions of cold water were applied externally; and when the danger of severe inflammation had passed by, bread and water poultices were put over the wound. The patient was allowed only mild food, and in about three weeks the wound had healed without any ill consequences.

Medicinische Zeitung. October 7, 1840.

On the Treatment of Fissures of the Anus by Rhatany. By M. TROUSSEAU

A mode of treatment successful in these troublesome cases without the necessity of the knife or cauterization has long been desired. We therefore publish the results M. Trousseau has obtained by the employment of rhatany. M. Bretonneau first introduced this treatment, and the numerous cures effected by him induced M. Trousseau to employ it, with the effect of saving the patient from a very painful if not dangerous operation. This means is one which appears scarcely rational, as it is well known that spasmodic constrictions of the sphincter play an important part in this disease, and rhatany injected into the rectum is *a priori* one of the most likely means to augment this constriction. To this M. Trousseau replies, that it matters little whether the remedy be irrational or not, provided it cure the patient. During the month of January, 1839, five patients were thus treated, of whom four were cured. MM. Marjolin, Bérard, and Desquibses, have also each cured a patient by the rhatany. Some of the cases are given at length, but we do not think it necessary to detail them.

With regard to the modus operandi of the remedy, M. Trousseau enquires, Do the tannin and gallic acid, so abundant in extract of rhatany, the action of which is so powerfully astringent, force away the blood which accumulates towards the irritated part, and by thus dissipating the inflammatory congestion favour rapid cicatrization? Or does the additional tonicity imparted by this medicine to the sphincter muscles, to the mucous membrane, and the submucous cellular tissue, enable the tissues more effectually to resist the distension caused by the passage of the mass of excrement; so that the wound, no longer enlarged each day, will naturally tend to cicatrize? However this be, M. Trousseau

thinks that there is some special property in rhatany which cures fissures of the anus as quinine cures fever, and mercury and iodine syphilis. He believes that all vegetable substances approaching rhatany in their chemical composition would have the same therapeutic effects, and this belief is strengthened by the success MM. Payer and Manec have obtained by the topical application of "monesia," a remedy recently imported into France, which among other ingredients contains a remarkable quantity of tannin.

The mode of employing the rhatany appears very simple, and the following is the method recommended by M. Trousseau, with slight alterations to suit individual cases. He gives the patient an enema, either of olive or almond oil, or of mucilage, and half an hour afterwards another containing five ounces of water, a drachm to a drachm and a half of extract of rhatany, and half a drachm of alcohol. The patient must retain this, and have a similar one at night. When the acute pain is allayed he is only to have one lavement a day, and when the cure appears to be completed one every two days for a fortnight. He has tried without advantage suppositories composed of a drachm and a half of cocoa-nut oil, and from eighteen to thirty-six grains of rhatany; but it would appear that "mèches" covered with an ointment composed of one part of rhatany and six or eight of lard or cerate, are useful in some cases.

Bulletin Général de Thérapeutique. 15 *et* 30 *Août*, 1840.

On the Use of Boiling Water in the Treatment of Callous Fistulæ. By Dr. Ruppius, of Freiburg.

The author was induced to adopt this method of treatment from what he had seen it effect in the hands of Rust of Vienna, and from the observation that the granulations which grow from scalded parts of the skin are peculiarly florid, and prone to unite firmly; a consideration which, we may add, long ago induced French surgeons to adopt the actual cautery for the same means, and is the foundation of the very skilful operations of M. Lallemand for vaginal fistulæ. Two cases are related, one of recto-vaginal fistula from abscess after a severe labour; the other, of an incomplete fistula in ano, extending four inches up the side of the rectum. The treatment consists in introducing the pipe of a syringe filled with boiling hot water down to the further end of the fistula, (which, if necessary, must be closed there with a finger of the other hand,) and slowly injecting a part of the contents. At the first operation only so much of the water should be forced in as is sufficient to stimulate the end of the fistula, so that it may commence healing at its deepest part, and, after the repeated injections, may make gradual progress in healing towards the surface. In both the cases that are related this procedure was strikingly successful.

Fricke und Oppenheim's Zeitschrift. Juli, 1840.

On a New Method of Operating for the radical Cure of Hernia. By M. Velpeau.

The author of this memoir, after examining at length the various means hitherto employed for the cure of hernia, states that he finds none of them at all efficacious; he therefore determined to try another method. Convinced from repeated experiments that injections with tincture of iodine produced in the serous membranes an adhesive inflammation, slight and scarcely dangerous, he attempted the radical cure of hernia by these injections. Three patients were operated on by him in the years 1836 and 1837, but the difficulties of a first operation, and the vagueness of the results, still left him in doubt, and he afterwards employed a method in two cases consisting of three distinct steps: subcutaneous puncture, scarification of the interior of the sac, and compression. He took the idea of scarifications from the ancients, or rather from what he had himself advanced in the year 1832. Twice that year he had placed great confidence in them, and only discontinued their employment when he perceived

was likely to ensue to the patient from opening the sac. These ned no longer likely, when M. Guérin gave the hint of the possi- ıetrating to the bottom of the cavity, by a simple puncture of the , and expressed in his memoir his desire of applying his subcuta- ons to the radical cure of hernia. M. Velpeau has performed the arifications in this manner. With regard to the compression, he t only to the external abdominal ring, but over the internal ring, uinal canal itself. He employs the celebrated bandages of M. Lempdes, which he states have more than once sufficed for the of hernia.

au successfully operated on a patient in the following manner: oduced the index-finger for some lines in depth into the external ing, he pushed the integuments before it, and passed a sort of he nail of this finger, pushing it obliquely as far as the iliac fossa; wing his finger from the ring, he brought the edge of the instrument liac walls of the abdomen, which he supported with the other hand, rified them in various directions, taking especial care to avoid the ssels. The lancet was then withdrawn at the exact spot where it and the operation was complete, only a few drops of blood having The patient was taken to bed, no accident occurred, the small wound the next day, the bandage of Fournier was applied on the third day, has since walked and conducted himself as if he had never been

ed by this result, the patient demanded a similar operation on nia. It was performed, and at the end of ten days he was conva- e has since continued quite well, and has remained as a cook in the now the radical cure to the profession.

Bulletin Général de Thérapeutique. 15 *et* 30 *Août*, 1840.

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ion and Absorption of Provisional Callus during Typhus Fever.
By Dr. SCHILLING.

ryman received a fracture of the left femur on the 1st of September, ful management the ends of the bone were so firmly united in the ovember, that he could bear some weight on the foot. Symptoms dominalis, however, then set in, and ten days afterwards callus could felt, and the bones moved as freely and as easily upon one another st examination after the reception of the injury. In six days more died. The examination exhibited no trace of callus; the broken e bloody, like those in a recent fracture, and were surrounded by a mbrane, which contained some black bloody fluid.

Medicinische Zeitung. September 16, 1840.

MIDWIFERY.

of Fracture of the Skull occurring during difficult Parturition.
By Dr. SCHULTZEN, of Insterburg.

, healthy, and corpulent woman, about forty years old, had already dren. All of them had been large, and all the births difficult, though d without artificial assistance; two of the children were born dead. egnancy was passed through well, and parturition came on at the The pains were severe, and the membranes ruptured, but the not make progress. After twenty-four hours the author found the e, and that much water had passed away, but that no blood had The uterus felt hard, and was contracted strongly round the child. ad a transverse position; the head lay above at the left side of the

uterus, the feet below and to the right, the right hip presenting. The child was alive. The os uteri was completely open, the vagina very much dilated, and the turning of the feet was easily accomplished. The pelvis was, during this operation, ascertained to be of normal dimensions. After the turning, the body of the child was quickly forced down to the aperture of the pelvis by the continued strong pains, but here the head stopped with both arms lying close beside it, and with the face directed backwards and to the right, till at last with a further increase of the pains, and with the assistance of traction of the feet and shoulders, the child was quickly expelled. It gave no signs of life, and notwithstanding long-continued endeavours were made, it could not be revived. It exhibited nothing whatever abnormal externally, except a sugillation on the right parietal bone as large as a half-florin. It weighed a pound and three-quarters; its length was twenty-two inches and a half, the transverse diameter of the head three inches and three quarters, the long diameter four inches and three quarters, the breadth of the shoulders five inches. The bones of the head moved with great difficulty upon each other, and the anterior fontanelle was proportionally very small.

On examining the head, after its external coverings were removed, there appeared an extravasation of blood on the right parietal bone, amounting to about half a drachm. Directly below this the parietal bone was pressed in at its middle, and fractured in the form of a star. The indentation was of a round form, about an inch in diameter, three lines deep, and completely retained its shape when the bones were separated from one another. When held against the light the bones seemed very vascular, and it was evident that the fracture extended through both the tables. On the dura mater there lay a coagulum of blood of the size of a sixpence, and below it one rather larger. Another of the same size was found below the cerebellum, and the whole of the brain and its membranes were unusually vascular.

There could be no doubt that the death of the child was the consequence of this injury of the head; and it was equally certain that the injury was the result of the difficult parturition, though no instrumental means were employed.

Casper's Wochenschrift. October 10, 1840.

CHEMISTRY, PHARMACY, &c.

Preparation of Mercurial Ointment in Six Hours. By M. Faucher.

Among the various methods that are proposed to facilitate the extinction of mercury in fat, the author proposes the following; by which, in the space of six hours, the globules are rendered invisible, either by the naked eye or with the assistance of a lens. It consists of taking

Pork-lard	6 ounces
White wax, or (which is better) spermaceti .	2 ,,

They are melted by a gentle heat, and the liquid mass is poured into a cast-iron mortar over two pounds of pure mercury; they are agitated with a wooden pestle till the globules of mercury disappear, and then the mortar is placed on the hot ashes, or over an oil-lamp, to keep the mixture of the consistence of molasses. In this condition it is to be constantly stirred for the number of hours mentioned.

Giornale delle Scienze Med. di Torino. 1839.

Cholestearine in Morbid Fluids. By Professor Nasse, of Marburg.

In three cases that have lately occurred to him, the author has found plates of cholestearine in the fluid products of disease.

The first case was that of a man with a large bronchocele, of thirty years' standing, which having produced considerable dyspnœa was punctured. A large quantity of fluid was evacuated, and in a portion of it sent to the author,

iscovered at once fine particles of cholestearine. Under the microscope y were found to be perfectly transparent, and very thin quadrilateral tablets an average of one fiftieth of a line in length, and with well-defined nearly t angles. Besides the cholestearine the fluid contained dark, granular, irre-arly-rounded globules, which probably consisted of stearine, some oil-glo-s, and lymph and blood-globules.

he second case was that of a woman, fifty years old, with a very large rian dropsy. The fluid that was removed by tapping was brownish, and, in ition to the usual salts, contained a great deal of albumen. With the micro-e, there were found in it a number of globules of oil, blood and lymph, and merous tablets of cholestearine, which measured for the most part one eth of a line in length. There was no fibrine present in the fluid of either or the preceding case. Half a year afterwards fluid having again accumu-d was again drawn off. It now contained fibrine, irregularly-formed pus-ules, and scattered fat-globules, but no cholestearine, nor were any traces of latter found in the fluid removed by a still later operation.

n the third case, the fluid was taken from an abscess that had formed in con-ence of the irritation of an anchylosed shoulder-joint. It was found to con-n imperfectly-formed pus-globules, globules of coagulable lymph, minute ps of oil, flocculi of fibrine, and large tablets of cholestearine exactly like e already described. Ten days after none such could be found in the fluid t continued to be sparingly discharged from the wound.

Müller's Archiv, 1840. *Heft* iii., p. 267.

I. THE AMERICAN AND COLONIAL JOURNALS.

Coagulation of the Blood fifteen hours after Death. By R. DUNGLISON, M.D., Philadelphia.

FROM a late number of the Bulletin of the "Proceedings of the American ilosophical Society," (No. 12, for May, June, and July, 18mo, pp. 216,) we tract the following notice:

"Dr. Dunglison gave the particulars of a case in which blood that flowed, dissection, from the arteries of the brain, coagulated fifteen hours after the ath of the individual.

"The patient died after a severe agony, and after an illness of some duration, r which mercury had been administered so as to affect the system freely. n opening the head the arteries of the brain were found turgid with blood, d on removing the brain the blood flowed from them and coagulated.

"Dr. Dunglison made some remarks on the singularity of this phenomenon, d its relations to physiology and medical jurisprudence, and stated that it mpletely overthrew the views of those who believe that the blood is either ssessed of a vital influence or receives some influence from the living vessels t contain it, which maintains its fluidity, and that so soon as it is removed m these influences it coagulates or dies. In this case the blood remained id, and coagulation took place fifteen hours after the total cessation of re-iration and circulation, and after the blood had become cold; circumstances owing that the phenomena is wholly physical in its nature."

Of this case we had not an opportunity of seeing anything until after death; r was an accurate history of it attainable. The patient had been delivered out a month previously, and had suffered under symptoms, as it was believed, f peritonitis, for which she was bled generally and locally, and had taken small ses of calomel, which produced severe salivation with considerable ulceration f the gums. Two or three days before her dissolution she was affected with

diarrhœa, with heat and dryness of skin, quick and feeble pulse, and consi rable stupor, passing her urine and fæces involuntarily. Under these syı toms she gradually sank.

Although the fact that blood may remain fluid in the vessels for a consi rable time after death, and coagulate when removed from them, has been ticed before, it does not appear to have given rise to any physiological deducti of moment, yet it is replete with interest to the physiological enquirer.

It is interesting to remark, that in all these cases, we believe, as well a the one which we observed, mercury had been largely administered, and it r be a topic for farther investigation, whether this peculiarity be in any man connected with the free use of that or other agents.

American Med. Library and Intelligencer. August 1, 18

[THE two following papers, extracted from our most excellent contempor of Philadelphia, are very valuable, and deserve the notice of surgeons. I wish we possessed similar records from our own great hospitals. We are sor to be under the necessity of omitting the tabular views of the whole series cases, having room for the results only.]

I. *Statistical Account of the Cases of Amputation performed at the Pennsylvan Hospital from January* 1, 1838, *to January* 1, 1840. By G. W. NORRIS, M.D one of the Surgeons to the Institution.

All those amputations in which the operation was performed within twent four hours after admission, are included under the head of immediate, tl patient in such cases having been brought to the house soon after the receipt ı his injury. With one exception, the common circular operation was pei formed, and the stumps were all dressed so as to procure union by the fir intention. The ordinary mode of dressing is first to bring the flaps togeth by means of three or four long strips of adhesive plaster, and after coveri the lips of the wound with lint spread with cerate, to apply a small cushion ı charpie over the extremity of the stump, and to secure the whole with a banda moderately tight. The first dressing was generally made on the third or four day, and repeated daily afterwards till cicatrization was complete.

Of eighty amputations on seventy-nine patients, performed during a term ı ten years at the Pennsylvania Hospital, thirty-five were primary, of which twent four were cured and eleven died, four of the deaths occurring within the twent four hours immediately following it.

Twenty were secondary, of which thirteen were cured and seven died.

Twenty-five were for the cure of chronic affections, of which twenty wei cured and four died.

Thirty-two of the amputations were of the upper extremity, of which twent seven were cured and five died.

Forty-seven were of the lower extremity, of which thirty-one were cured an sixteen died.

Seven were amputations at the joints, of which four were cured and three die

13 of the 79 operated on,	were under 20 years of age, of whom	12 were cured and	1 die
26	between 20 and 30,	19	7
22	between 30 and 40,	15	7
16	between 40 and 50,	9	7
2	upwards of 50,	2 were cured.	
—		—	—
79		57	22

The conclusions to be drawn from an analysis of the two tables which I hav now published are,

1. That amputation with us is to be regarded as an operation attende with much danger to the life of the individual, the mortality after it bein 1 in $3\frac{7}{11}$.

2. That the chances of success after it are much greater in persons who have been for some time suffering from chronic diseases, than in those who have it done while enjoying robust health, the mortality in the former class of cases being 1 in $6\frac{1}{4}$, while in the latter it is 1 in $3\frac{2}{11}$.

3. That immediate amputations after injuries are less fatal than secondary operations, the mortality after the former being 1 in $3\frac{2}{11}$, while in the latter it is 1 in $2\frac{5}{7}$.

4. That amputation of the lower extremity is much more fatal than that of the superior member, the mortality after the former being 1 in $2\frac{15}{16}$, while in the last-mentioned class of cases it is only 1 in $6\frac{2}{3}$.

5. That the danger increases with the age of the individual operated on.

II. *Statistics of the Amputations of Large Limbs that have been performed at the Massachusetts General Hospital; with Remarks.* By GEO. HAYWARD, M.D., one of the Surgeons to the Hospital.

IN a large proportion of the following cases, the amputation was done by the circular incision; the flap operation was adopted occasionally, whenever there was reason to believe that a better stump could be made by it than by the other method. The dressings were always of a light and simple kind, consisting of two or three strips of adhesive plaster and a small compress and roller; and yet there are some surgeons of the present day, who would perhaps regard these as more cumbersome than was necessary. If the bleeding was slight, the dressings were applied before the patient left the operating room; but if there was anything more than an oozing from the veins, it was deferred till a few hours after. Secondary hemorrhage was not frequent, though it sometimes occurred; pressure was generally sufficient to arrest it, but occasionally it was found necessary to open the stump, and tie one or more vessels. In one case where hemorrhage occurred twelve days after the operation, from a diseased state of the posterior tibial artery. the femoral artery was tied. No one who had secondary hemorrhage died, and though it sometimes debilitated the patient, in no case was there any permanently injurious effect from it. In all the cases it was attempted to heal the wound by the first intention, and in a few instances it was completely successful, but in by far the greater number it was only partially so. It has not been the usual practice at the Massachusetts Hospital to administer an opiate before an operation, though in a few instances it has been done. In one case where amputation was performed on a patient with delirium tremens, twelve grains of opium were given shortly before the operation; he became drowsy soon after and recovered.

From this table, it appears that there were seventy operations on sixty-seven patients, three patients having two limbs removed. In one of these three cases, one operation was above and the other below the knee, and in the other two, both operations were below; the first patient died, and the other two did well.

Of the whole number operated on, fifteen died and the remainder recovered, at least so far as to be able to leave the hospital, though it is probable that in some instances the disease may have returned.

There were thirty-four patients who had the thigh amputated, and one of these had the other leg taken off at the same time below the knee; of this number, nine died. Of twenty-three patients whose legs were amputated below the knee, two having both legs removed, five died; and of the ten who had an arm amputated, six below and four above the elbow, one died.

This goes to confirm the prevailing opinion among surgeons, that amputations of the lower extremities is more often followed by fatal consequences than that of the upper, and that death takes place more frequently after amputation of the thigh, than after that of the leg. More than a quarter of those whose thighs were amputated died, while there was but little more than one death in five

among those whose legs were removed below the knee, and only one of the ten whose arms were amputated. This patient too died of delirium tremens. The operation to be sure did not arrest the disease, but apparently contributed nothing to the fatal result.

This table tends also to support the opinion, that patients who undergo amputation for chronic diseases are much more likely to recover than those in whom it is performed in consequence of recent accidents. Of the first class, there were forty-five patients afflicted with various diseases, and of this number all recovered but six; and of the remaining twenty-two, whose limbs were removed on account of recent injuries, no less than ten died, being nearly half of the latter and less than one in seven in the former.

This fact certainly gives support to the opinion, that a state of high health is not favorable to surgical operations; and it also tends to show that death after amputation is not by any means attributable in all cases to the operation alone; for if it were, the proportion of deaths should be as large among one class of patients as among the other. There can be no doubt, I think, that the result is influenced very much, not only by the age and constitution of the patient, and the disease or injury for which the operation is performed, but also by the period at which it is done. I have before said that I thought that amputation was "often performed when it might have been avoided." But this remark applies principally to cases of recent injury. In those of chronic diseases of the limbs, the error is more apt to be of the opposite character; the operation is either not performed, or if done at all, frequently not till it is too late. It cannot be denied, I think, that there is a disposition at the present day to defer amputation too long in cases of diseased limbs; there is an unwillingness to admit that the morbid affection is beyond the reach of remedies, and the operation is too often postponed till other parts become affected, or the system is worn down by continued irritation. At length the limb is removed; but the patient, already exhausted by disease and long suffering, is hurried to his end by the very means that might have saved him, if they had been earlier employed.

If amputation is frequently too long delayed in chronic diseases of the limbs, it is, I fear, very often resorted to in recent injuries earlier than it should be. Many limbs that have been removed, might probably have been saved; but where this cannot be done, it is rare that much inconvenience would follow from a little delay.

In most cases of accident sufficiently severe to justify amputation, the whole system has suffered a great shock, and an operation at this time, before reaction is fairly established, is very likely to cut off what little chance the patient might otherwise have of recovery. While the extremities are cold and the action of the heart is feeble, the local injury is hardly, if at all, perceived, and adds nothing to the patient's sufferings. An operation cannot be required then; and yet how often it is done at that period, the better judgment of the surgical attendant sometimes being overruled by the importunate interference of the bystanders.

If the injury be not so serious as to cause almost immediate death, reaction usually comes on with proper management in a few hours, and then, if an operation be necessary, it can be done with a much greater prospect of success.

With regard to the ages of the patients operated on, it appears that there were

		Under 20	years of age	13,	of this number	1	died.
Over 20	and not exceeding	30	,,	31,	,,	8	,,
,, 30	,,	40	,,	9,	,,	3	,,
,, 40	,,	50	,,	10,	,,	2	,,
,, 50	,,	60	,,	3,	,,	1	,,
		Over 70	,,	1,	,,	0	,,

Whole number, 67, No. of deaths, 15.

American Journal of Medical Sciences. May, 1840.

On the Effects of Temperance Societies in diminishing Sickness and Mortality among the Troops in India. By WM. BELL, Surgeon of the Cameronians, Calcutta.

THE Temperance Society of the Cameronians was established about two years ago, and though the numbers have fluctuated considerably, yet upon the whole it has been well supported, and there can be no doubt that its influence has been most favorable both on the health and on the morals of the regiment. In December, 1837, the average monthly strength had been 121; and in December, 1838, it remained 159, having fluctuated in the mean time from 208 to 108. These alterations are to be accounted for, in some degree, by the arrival of detachments of invalids and recruits, which generally interrupt for a time the steadiness of the corps The effect in health during 1837, was that of the society the per centage of sick amounted to 3 ¼, and of the rest of the regiment to 101¾. During 1838, the average daily sick of the society has been 6½ per cent., and of the remainder of the regiment 9 per cent.

These results, however gratifying, do not convey an adequate idea of the benefits of the society; for a number of men whose constitutions had been ruined by dissipation became members, and several such remained in hospital nearly the whole year, until they were invalided. The admissions of the last year have been of the society 1 in 25, and of the remainder of the regiment 1 in 11.

This is the first instance I believe of any regiment stationed in Fort William, having established a regular Temperance Society, and it therefore becomes a duty to point out such favorable circumstances in the state of the corps, as may fairly be attributed, more or less, to its influence, in order that others may be induced to seek for the same results by the adoption of similar institutions.

1st. The deaths in the regimental hospital have been in 1837, 26, and in 1838, 22, whereas the average mortality in Fort William, for a period of fourteen years previously, had been seventy-two nearly.

2d. The spirits drunk in the canteen have been for 1837, 9673 gallons less, and for 1838, 8242 gallons less than the regiment was entitled to draw.

3d. During the above two years, the beer sold in the canteen amounts to 156 hogsheads, 46½ gallons, and the wine to 326 dozen.

This consumption of beer and wine is not quoted as very creditable or praiseworthy, but as a proof that the use of spirits, in a great degree, was abandoned, even by those who did not choose to be particularly abstemious with regard to their beverage.

4th. The remittances by the men to their friends at home, and to the Edinburgh Savings' Bank, were for 1837, £587 18*s*. 9*d*., and for 1838, £763 4*s*. 6*d*.

This is not quoted as a remarkably large sum, being smaller than those of previous years, but as a proof that even on half batta stations, and where the temptations to spend money are numerous, the soldier who can avoid dissipation has both the means and the inclination to provide, to a certain degree, for the future comfort of himself or his friends.

5th. Since the arrival of the regiment in India, it appears that the consumption of spirits has diminished from the enormous quantity of 10,000, 12,000, and 14,000 gallons to 2516.

What is the cause of this remarkable change?

It may depend on more causes than one; but there can be no doubt that the chief is the establishment of a Temperance Society, and its principles.

6th. There are other circumstances which deserve to be mentioned, such as the decrease of liver complaints from 111, 140, and 135, as in the years 1832-33-34, to 82, and to 50, as during the last year; but this may be partly owing to other causes, besides the disuse of ardent spirits, as to change of climate, from the upper to the lower provinces, &c.; yet a comparison of the tables will show the great superiority in regard to health in the Cameronian over any other corps that has ever been stationed in Fort William.

7th. As Temperance Societies have been formed in most of the Queen's corps serving in Bengal, the following abstract is here added, showing the result of the whole from the 1st January to 30th June, 1838, as drawn up by the Inspector-General.

ABSTRACT OF THE COMPARATIVE STATE OF HEALTH OF THE TEMPERANCE SOCIETIES OF HER MAJESTY'S TROOPS SERVING IN BENGAL, FROM JANUARY 1 TO JUNE 30, 1838.

Months.	Strength of the Temperance Society.	Strength of the remainder of the Regiment.	Relative proportions admitted to Strength.		Average number of men daily in Hospital of the Society.	Per centage of the Society.	Average number of men daily in Hospital not of the Society.	Per centage not of the Society.
			Temperance Society.	Remainder of Regiment.				
Jan.	1053	2560	1 in 18·77-104	1 in 9·220-261	49·24-31	2, 64	206·27-31	8, 15
Feb.	1840	2649	1 in 20 10-110	1 in 9·245-266	41·20-28	2, 27	218·11-29	8, 27
March	1542	2879	1 in 11 44·107	1 in 7·149-390	45·12-31	2, 94	248·19-34	8, 66
April	1359	3081	1 in 10·9-135	1 in 5·261-564	74·12-30	5, 47	316·22-30	10, 28
May	1262	3161	1 in 18·44-69	1 in 6·353-468	67·6-31	5, 24	338·30-31	10, 66
June	1364	3065	1 in 19·53-69	1 in 6·371-349	62·3-30	4, 55	317·6-30	10, 35
Total	9340	17354	1 in 16·47-143	1 in 7·284-1199	341·40-84	3, 65	1771·731-930	10, 20

[The preceding paper comprises only a small part of the evidence communicated to the public on this vital subject, and all having the same bearing. If *total abstinence* from intoxicating drinks generally should be substituted in India for the abstinence from spirituous liquors only, we have no doubt but the results would be greatly more striking still.]

India Journal of Medical Science. Nov. 1839.

II. THE BRITISH JOURNALS.

(FOR THE QUARTER ENDING NOVEMBER 30, 1840.)

Case of dangerous Uterine Hemorrhage in which Transfusion was successfully employed, with some observations on the more frequent expediency of that operation. By RICHARD OLIVER, M.D., Carlisle.

ON the 26th of June 1837, I was called to attend the wife of John Cook, a weaver, living at Eden Place, about a mile from my house. She had been attended by a midwife, and had given birth to a child at its full time in the course of the previous night. The patient was about forty-two years of age, and this was her seventh child. I found her at six A.M. in an exceedingly exhausted condition. Blanched by a profuse hemorrhage, which no adequate means had been employed to suppress, but which had now ceased, she was lying on her back in a state of imperfect consciousness, with the pulse at the wrist barely perceptible, now and then moaning lowly, and casting about her arms. About half a glass of rum with a little water was immediately given to her, and this, with a few spoonfuls of beef tea, was repeated two or three times at intervals of about twenty or twenty-five minutes. After each dose she appeared to be a little refreshed, but upon the whole the symptoms of collapse was gaining ground. About half-past seven o'clock brandy was substituted for the rum, and the dose was increased to an ounce and a half, with the addition of a drachm of aromatic spirit of ammonia and a few drops of tincture of opium to every second or third portion. The same deceitful promises of reaction were suc-

ceeded by the same progressive indications of sinking, until at length, about one P.M., she became quite unable to swallow. The pulse at the wrist and in the carotids had not been perceptible for more than two hours and a half, and the coma was now complete. Under these very unpropitious circumstances I determined upon transfusion, with little hope of success, and with no small compunction for having thus afforded the operation so little fair play. At half-past one P.M. I was provided with the apparatus necessary for performing it; and having obtained a willing supply of blood from three of the patient's kind-hearted neighbours, I opened a vein at the bend of her arm, and with the assistance of two of my professional friends, Mr. Bowman, surgeon of this place, and Dr. Henry Lonsdale, now Demonstrator of Anatomy in Queen's College, Edinburgh, I proceeded cautiously and steadily to introduce it.

I had first taken care to see that the instrument was in proper order, and particularly that I should have the syringe and its tubes free from air. After one or two gentle strokes with the piston, made with a view to ascertain this point, I found that the cup attached to the apparatus was so small that it could not be safely used. Unless the piston was elevated very slowly, and the blood was supplied very steadily to the cup, there was great risk of introducing air into the cylinder. But finding that, by taking a common basin to receive the blood, and by drawing it up thence through the bottom of the syringe, I could obviate this danger, I laid the cup aside altogether. With this simpler arrangement I passed syringeful after syringeful into her exhausted veins, pausing from time to time to mark the effects, and anxiously watching for some glimmering promise of the return of energy to her heart. On a moderate computation we had already transfused twelve ounces of blood, and she still lay pulseless and perfectly insensible. The respiration, however, although faint and low, was distinct and regular; so that, however small the amount of blood in her system might be, there was still some undergoing aeration in the lungs; and in gradually augmenting its quantity, we might possibly contribute to raise her vital powers, by enabling a larger portion of it to reach the nervous centres. We could not discern the heart's pulsations, but we might be quite certain that it did beat, and that the general circulation, although thus imperceptible, was still actually carried on. We had yet obtained no assurance of improvement, but it was pretty evident that, by proceeding cautiously, we neither had done nor could do any harm. Without this expedient the poor woman's death was inevitable, and but too probable we then thought even with it; so, disregarding the cautions given upon this point, we determined to go on. Steadily and slowly the blood was introduced as before, until at length we imagined that the pulse became faintly perceptible in the arm; and slight as it was, this intimation of the heart's increased power gave us no small encouragement. After persevering for a few minutes longer, we had the very perfect gratification of witnessing not only the complete restoration of the circulating power, but the return of consciousness, and of the ability to speak. It is unnecessary to advert to the subsequent details of the treatment and of the recovery, farther than to mention that she went on very favorably, and in a few weeks was moving about in her family as usual. She remained for some time longer rather weak and delicate, but beyond an occasional slight headach, and a tendency to constipation and flatulence, she suffered from none of the more prominent and distressing symptoms which ordinarily ensue after serious losses of blood, and she has long been, and still remains, in very good health. With respect to the quantity of blood introduced in this case, I am not able to speak with absolute accuracy; but I feel quite certain that I am below the mark in mentioning twenty-two ounces. From each of the three individuals who supplied the blood, we took at the least an average of twelve ounces; and although we did not attempt to measure the amount of it, I am perfectly satisfied that, at all events, not more than one-third of the whole was lost by coagulation, and by being thrown upon the ground in adjusting and preparing the instrument.

Edinburgh Medical and Surgical Journal. Oct. 1840. *No.* 145.

Observations on the Diagnosis and Pathology of Fractures of the Neck of the Femur. By Robert William Smith, A.M., M.R.I.A.

[This is a very valuable Memoir, well deserving attentive perusal. We r gret that we can only find room for the tabular view of the cases (each of whi is detailed in the paper and illustrated by a woodcut), and the conclusions d duced therefrom by the author.]

No.	Name.	Age.	Shortening.	Position of the Foot.	Period of Surviv after the Receip of the Injury.
	Intracapsular Fractures of the Neck of the Femur.				
1	Laurence Maguire .	40	½ inch	Eversion	14 days
2	William Collins . .	36	¾ ,,	,,	17 ,,
3	Thomas Maguire .	84	½ ,,	,,	14 ,,
4	Dorah Campbell .	75	1 ,,	,,	2 months.
5	Mary Gill . . .	80	½ ,,	,,	Not known.
6	Esther Christie . .	60	1½ ,,	,,	,,
7	Mary Lamb . .	80	¾ ,,	,,	1 year.
8	Margaret Bourke .	90	½ ,,	,,	14 days.
9	Margaret Myler . .	78	¼ ,,	,,	2 months.
10	A Female . . .	65	1 ,,	,,	Not known.
11	Patrick Doolan .	60	2 ,,	,,	7 years.
12	Michael Curry . .	40	1¼ ,,	,,	1 month.
13	Matthew Reilly .	46	½ ,,	,,	4 months.
14	A Female . .	55	1 ,,	,,	Not known.
15	Sarah Ashton . .	65	1¼ ,,	,,	9 years.
16	Elizabeth Casey .	50	¾ ,,	Inversion	Not known.
17	Robert Robinson .	50	2 ,,	Eversion	Several years.
18	Ellen Walker . .	70	½ ,,	,,	7 days.
19	Laurence Reilly .	56	2 ,,	,,	Several years.
20	Joseph Seaton .	90	1¼ ,,	,,	7 years.
21	A Female . .	65	2½ ,,	,,	Several years.
22	Thomas Connolly .	50	¾ ,,	,,	10 days.
23	Bridget Misset .	72	1 ,,	,,	10 weeks.
	Extracapsular Fracture of the Neck of the Femur.				
24	Patrick Murphy . .	80	2 inch	Inversion	14 days.
25	Alicia Harris . .	70	1½ ,,	Eversion	5 ,,
26	James Stanford .	67	2 ,,	,,	8 days.
27	A. B. a man . .	50	2 ,,	,,	14 ,,
28	Mary Kelly . .	56	1¼ ,,	,,	11 ,,
29	Ellen Bryan . .	65	1½ ,,	,,	5 weeks.
30	Patrick Grant . .	70	1½ ,,	,,	5 days.
31	Margaret Connolly .	89	1½ ,,	,,	12 ,,
32	Thomas Murphy .	41	Not known	,,	A few weeks.
	Impacted Fractures of the Neck of the Femur, external to the Capsule.				
33	John Summers .	74	1½ inch	Eversion	2 months.
34	Mary M'Kenna .	52	¾ ,,	,,	4 days.
35	Catherine Egan .	60	1¼ ,,	Inversion	1 month.
36	Sarah Denny . .	70	1 ,,	Eversion	1 ,,
37	Alicia Sherlock .	64	½ ,,	,,	15 weeks.
38	James Power .	54	1½ ,,	,,	5 months.
39	A. B. a female .	80	Not known		Not known.
40	Not known . .	...	...		... ...
41	Bryan Dunn . .	60	¼ ,,	,,	13 days.
	Impacted Fracture of the Neck of the Femur, internal to the Capsule.				
42	Owen Curran . .	70	½ inch	Eversion.	1 year & 10 mo.

ıat has been stated in the preceding pages, and from the evidence r the *post-mortem* examination of fifty specimens of fractures of the e femur, forty-two of which have been detailed, I think I am justified g the following conclusions:

;ht degree of shortening, removable by the extension of the limb, inracture within the capsular ligament. 2. The degree of shortening, fracture is within the capsular ligament, varies from a quarter of an e inch, or one inch and a half. 3. The degree of shortening, when re is within the capsule, varies chiefly according to the extent of la' the fibro-synovial folds which invest the neck of the femur. 4. In ı of intracapsular fracture the injury is not immediately followed by of the limb. 5. This absence of shortening is generally owing to ty of the fibro-synovial folds. 6. In such cases the retraction of the occur suddenly, many weeks after the receipt of the injury. 7. This raction of the limb, which indicates a fracture within the capsule, is, to be ascribed to the accidental laceration of the fibro-synovial folds. gree of shortening, when the fracture is external to the capsule and ;ed, varies from one inch or one inch and a half, to two inches or two a half. 9. When a great degree of shortening occurs immediately eceipt of the injury, we usually find a comminuted fracture external psule. 10. The extracapsular facture is generally accompanied e with displacement of one or both trochanters. 11. The extra*impacted* fracture is generally accompanied by fracture without :nt of one or both trochanters. 12. In such cases the fracture of ıses unites more readily than that of the cervix. 13. The deırtening, when the fracture is impacted, varies from a quarter of an ; inch and a half. 14. The exuberant growths of bone met with in have been by many erroneously considered to be merely for the purpporting the acetabulum and the neck of the femur. 15. The difficertaining crepitus, and of restoring the limb to its natural length ief diagnostic signs of the impacted fracture. 16. The position of as much influenced by the obliquity of the fracture and the relative ' the fragments, as by the action of the muscles. 17. Inversion of ay occur in the intracapsular, extracapsular, or impacted fracture of f the femur. 18. When in the intracapsular fracture the lower fragıced in front of the upper, the foot is usually inverted. 19. When 'acapsular fracture with impaction, the superior is driven into the gment, so as to leave the greater portion of the latter in front of the e foot is generally inverted. 20. In cases of comminuted extracapıre without impaction, but with separation and displacement of the ı, the foot may be turned either inwards or outwards, and will gene in in whatever position it has been accidentally placed. 21. The on by bone of the intracapsular fracture is most likely to occur, fracture is also impacted. 22. Severe contusion of the hip-joint, ralysis of the muscles which surround the articulation, is liable to be l with fracture of the neck of the femur. 23. The presence of :umatic arthritis may not only lead us to suppose that a fracture exıe bone is entire, but also when there is no doubt as to the existence :, may render diagnosis difficult as to the seat of the injury with re; capsule. 24. Severe contusion of the hip-joint, previously the seat rheumatic arthritis, and the impacted fracture of the neck of the ; the two cases most liable to be confounded with each other. ıarticular symptom of fracture of the neck of the femur, separately must be looked upon as equivocal: the union of all can alone lead liagnosis.

Dublin Journal of Medical Science. Sept. 1840.

On Nervous or Spasmodic Asthma. By ROBERT J. GRAVES, M.D., of Dublin.

[THE following is only a part of the very interesting section on Asthma, the article "On the Treatment of various Diseases," in the last number of t Dublin Journal, by this eminent physician.]

It is evident, that to account for the spasmodic symptoms of asthma, v need not have recourse, with Doctor Clutterbuck, to the diaphragm or inte costal muscles, but to the muscles of the trachea and bronchial tubes themselve on the whole, therefore, we may conclude, that those who have returned to tl opinions professed by our predecessors, are not so much mistaken as their o ponents pretend. Even when the paroxysm is intense in degree and duratio where the patient is obliged to sit up half the night; where any attempt to l down produces symptoms of asphyxia; where hours are spent in extreme di tress with lividity of face and lips, gasping, loud wheezing, and great fulness (the vessels of the head and neck; even under all these circumstances, the attac may be nothing but a fit of pure spasmodic asthma. A person thus affect may spend a whole night in the way I have described, and yet, towards mor ing, he may sleep a few hours, and awake refreshed and comparatively free fro dyspnœa, and in the course of the day may be able to go up stairs quickly, ru ride, even hunt without difficulty. I have in my recollection, the cases (several young men subject to severe paroxysms of asthma for five or six nigh in succession, and who, immediately after the paroxysm disappeared, could u any active exercise as well as the most vigorous and healthy of their companion

These facts establish the existence of a disease deserving the name of spa modic asthma, and show that very violent paroxysms of difficult breathing ma occur in persons free from organic affection of the heart or lung. When, how ever, any permanent change in the structure of the respiratory or circulatin apparatus exists, then such changes become the exciting causes of paroxysn of dyspnœa, often closely resembling true spasmodic asthma, but readily di tinguishable from it, if due attention be paid to the history of the patient sufferings and his state between the fits. I have now met with so many cases (young persons in whom no trace of any organic complication existed, that seems to me more than probable, that spasmodic asthma is not so rare a disea as is imagined. In a little boy, some particulars of whose case I formerly pul lished, the attacks were frequent, violent, and to all appearance, purely spa modic; he got a very severe paroxysm of gout (hereditary from both his fath and mother) in his foot, and has never since had asthma, though four years ha now elapsed, and he has been subject to all the excitement and violent exercis of a public school. Mr. Fleming, now of the Isle of Man, and Sir Phil Crampton, attended with me a young gentleman, aged about twelve, who wa subject to violent dyspnœa, increased by even the most gentle exercise; inde for many months he could not walk even quietly in his room, without incurrin the risk of suffocation for want of breath, attended with palpitation, wheezin and all the symptoms of approaching asphyxia; every remedy we could devi was tried most perseveringly for a year, without the slightest benefit, when l got typhus fever, from which he narrowly escaped, but since his recovery, l has never had even the least vestige of his former complaint. These two case exemplify, in a remarkable manner, the influence which the general state of tl constitution often exerts on local affections.

Asthma, like all other nervous diseases, is subject to the most unaccountab variations, and is most uncertain as to the effects which our remedies, or tl influence of physical agencies, produce. The following is an example. I December, 1839, I attended two gentlemen residing in the same street, ar each about forty-five years old; neither was liable to any other disease, and the were both short and stout; on a very cold morning I found one of them very i indeed; he had not slept at all during the night, and had every moment been o the point of smothering from asthmatic dyspnœa. The extreme violence (the paroxysm he attributed to the fact, that his bed-room chimney had smoke

occasionally during the night, and the weather was so cold, that he was afraid to open the window to let out the smoke. I ordered him to change his room, and I then proceeded to visit his neighbour, and found him sitting in a room full of smoke; he apologized to me for introducing me into so disagreeable an atmosphere, and explained, that when the fit of asthma became very bad, the only sure means of obtaining relief, which he knew of, was to get a good coal fire lighted in the grate, which being done, he made his servant occasionally obstruct the progress of the smoke up the chimney, and thus maintain a certain density of smoke in the room; this never failed, he assured me, to bring relief. This gentleman was of very active habits, was agent to several large properties, and consequently obliged to travel much about the country; experience had proved to him, that he could derive no benefit from turf smoke, and therefore he never stopped at any inn where they had no other fuel but turf, as he felt himself insecure unless he could procure coal smoke in case of an asthmatic attack. Such *idiosyncrasies* will ever baffle the researches of the mere morbid anatomists, but afford a useful lesson to the practical physician.

The phenomena of this disease are calculated to throw much light on the nature of what has been termed wheezing. A person subject to asthma, who has been breathing tranquilly the whole evening, may be attacked towards midnight with difficulty of respiration, and a wheezing so loud as to be heard on the stairs; this will continue for several hours, and then terminate, in some with a copious discharge of sputa, *in others without any expectoration whatever.* When we apply the stethoscope to the chest of a person so affected, we hear a great number of bronchitic rales, showing that the larger and smaller tubes are both engorged; this is a matter of frequent occurrence in cases of dry asthma, where there is no expectoration, and where the fit terminates in a few hours, without leaving behind the slightest trace of pulmonary derangement. Hence we are led to the conclusion, that sounds of various characters and remarkable intensity may be produced without any inflammation whatever, and in fact without any remarkable alteration in the secreting functions of the bronchial mucous membrane, and that these sounds may wholly disappear where there has been no expectoration, and consequently where the bronchial tubes have not been cleared out. This is a fact worthy of being held in memory. Stethoscopists, when they hear bronchial rales, are apt to attribute them to the existence of bronchial inflammation; but here, with distinct proofs of the absence of inflammation, you may have a *maximum* of bronchial rales, and in the space of a few hours, you may not have a single sound at the very points where so many were audible before. It is obvious, therefore, that some of the received doctrines on the subject of bronchial rales, are still open to discussion. The practical inference, however, to be drawn from this fact is, that we should study such rales with great attention, and in connexion with other signs and symptoms, lest we be induced to treat antiphlogistically a case in which depletion might be uncalled for or injurious, an error by no means unfrequent among those who rely too exclusively on physical signs.

As to the treatment of spasmodic asthma, I have nothing to add to what is generally known, except that it is often serviceable to stupe the whole chest during the fit with flannel wrung out of water, as hot as can be borne; and that, in some, much advantage is derived from small but very frequently repeated doses of ipecacuanha wine, mixed with an equal portion of good tincture of castor.

Dublin Journal of Medical Science. Nov. 1840.

On the Use of Tincture of the Muriate of Iron in Diabetes Mellitus.
By CHARLES CLAY, Esq., Surgeon, Manchester.

THE following three cases yielded so decidedly to the use of the tinct. ferr. mur. P. L., after many other remedies had been tried, that I trust that placing them before the public will be the means of testing its merits still further. It

is quite necessary, in order to succeed, to give it in large doses, as I have repeatedly tried the same remedy in small doses without any effect. The cases I am about to give were of sufficient standing as to time and obstinacy (and could only be considered bad cases), and of such a character that the trial of any new remedy was perfectly justifiable.

Case i. James Newton, of Ashton-under-Lyne, February, 1836, aged 75 years, had been for two years suffering from diabetic flows of urine, which for nine months had considerably increased; he had been under the care of different persons, and a variety of remedies were tried, but no abatement of the symptoms was observable. When he applied to me the quantity of urine discharged was nine pounds and a half by weight in twenty-four hours, fully charged with saccharine matter; his appearance was emaciated, anxious countenance, and a dry, furred tongue. After trying various plans, without any apparent benefit (with the exception of temporary relief, for a few days, by the exhibition of nitrous acid), at last, without any particular hope of benefit, I ordered the following mixture:

> Tincture of opium, ʒjss;
> Tincture of muriate of iron, ʒij;
> Sulphate of quinine, grs. viij;
> Distilled water, ℥vj. An ounce to be taken three times a day.

After continuing this formula for three days, I was agreeably surprised by a sensible abatement of the quantity of urine, but still as fully charged with saccharine matter; in five days more (that is, eight from the commencement), the abatement continued; the countenance less anxious, tongue clean, and evidently improving in constitution. On the eighteenth day barely four pounds of urine were discharged in twenty-four hours, in which little saccharine matter could be detected. In four weeks, with a continuation of the medicine, he appeared in perfect health, and, at the end of six weeks, ceased taking medicine entirely, and since has had no return of the complaint.

Case ii. W. Grundy, aged 30, of Hurst, came under my care in April, 1838, after being treated by different persons without any apparent benefit. From the decided success of the tinct. fer. mur. in the case of Newton, I began immediately with the same dose, as above stated. The quantity of urine was, at the commencement, eight pounds in twenty-four hours, and full of saccharine matter. For five days no improvement in either the quality or quantity of the discharge was observable; after that time, however, the abatement began to show itself, but without any diminution of the saccharine principle. On the fourteenth day, the diminution of the discharge was remarkable, not more than three pounds and a half in twenty-four hours, and the character of the urine much less sweet. On the twenty-fourth day the discharge was natural in quality as well as quantity, and before the expiration of five weeks he left off taking medicine.

Case iii. Mary Wild, aged 56, of Ashton, had been subject to a diabetic discharge for eight months; her general health had for some time been very precarious, from the cessation of the menstrual discharge: about seven pounds and a half of urine in twenty-four hours. In this case the saccharine matter was not so abundant as in the former cases. I gave the tinct. fer. mur. mixt. for six days, when a slight abatement was observable; but on the twelfth day the quantity was more than at the commencement. On the fifteenth day the abatement again showed itself: and, from this time to the end of four weeks, kept continually decreasing. At this time pleuritic symptoms called for a cessation of these remedies and the substitution of others, during which time a slight increase of urine came on; but on going on with the old medicine the improvement returned. She finally ceased taking medicine at the end of eight weeks, feeling her health quite restored, and has had no return since. The date of this case was March, 1840.

Lancet. Oct. 10, 1840.

PART FIFTH.

Medical Intelligence.

TABLE OF MORTALITY FOR LONDON,

For the Third Quarter of 1840:

Showing the number of Deaths from all causes registered in thirteen weeks, from the 4th July to the 26th September, 1840.

Causes of Death.	July. Week ending				August. Week ending					September. Week ending				Total.
	4th	11th	18th	25th	1st	8th	15th	22d	29th	5th	12th	19th	26th	
Class I.														
Smallpox	16	11	13	21	17	18	20	12	25	23	22	23	18	239
Measles	19	20	22	29	34	24	18	28	21	21	13	23	27	299
Scarlatina	53	40	41	32	38	35	37	44	35	48	40	35	42	520
Hooping-cough	17	16	19	16	17	19	16	16	8	16	9	12	17	198
Croup	8	7	6	2	9	12	10	6	6	6	6	3	5	86
Thrush	9	7	2	9	10	8	15	15	20	12	10	6	8	133
Diarrhœa	10	9	11	11	14	14	31	20	22	20	40	29	26	257
Dysentery	0	0	3	2	3	2	9	4	3	3	1	2	1	26
Cholera	0	2	0	3	2	4	6	6	1	6	10	7	1	48
Influenza	3	1	1	1	2	3	1	1	1	0	1	0	0	15
Typhus	24	19	27	21	14	17	26	27	23	35	23	19	33	311
Erysipelas	4	6	12	10	8	8	[illegible]	3	1	8	1	4	6	74
Syphilis	0	0	0	1	0	0	0	3	0	1	0	0	1	1
Hydrophobia	0	0	0	0	0	0	0	0	0	0	0	0	0	8
Total Epidemic, &c.	163	138	167	158	168	164	185	185	166	199	176	156	185	2128
Class II.														
Cephalitis	19	8	16	9	9	13	9	17	22	12	15	8	6	163
Hydrocephalus	30	27	44	36	27	38	37	36	36	41	43	40	32	467
Apoplexy	17	12	15	14	15	9	20	17	11	18	14	16	20	196
Paralysis	12	29	16	15	14	13	10	10	19	13	21	16	12	200
Convulsions	69	61	74	66	76	73	66	68	81	66	59	62	53	894
Epilepsy	8	4	5	4	2	6	3	0	4	0	5	2	2	45
Insanity	3	0	1	3	0	1	0	0	0	0	1	1	0	10
Delirium tremens	3	1	2	0	2	1	4	1	1	1	0	2	2	19
Diseases of the brain, &c.	8	7	6	11	5	9	11	8	7	8	14	4	11	109
Total Dis. of Brain, &c.	157	139	179	147	150	163	160	157	181	151	172	151	136	2045
Class III.														
Quinsey	4	1	2	1	1	0	2	4	0	1	2	1	3	22
Bronchitis	7	2	10	11	7	3	5	7	7	1	4	3	1	68
Pleurisy	2	1	1	4	1	3	0	0	1	3	2	1	2	21
Pneumonia	44	62	45	49	51	51	43	39	39	44	38	40	52	590
Hydrothorax	0	6	9	6	3	1	0	1	7	5	5	2	3	48
Asthma	9	10	15	9	9	5	10	8	4	12	4	12	16	123
Consumption	156	144	142	135	146	144	146	142	131	128	137	130	118	1805
Diseases of the lungs, &c.	5	14	4	11	6	13	10	11	8	12	8	6	12	120
Total Dis. of Lungs, &c.	227	240	228	226	224	220	216	212	190	206	200	201	207	2797
Class IV.														
Pericarditis	2	0	1	2	0	1	1	1	1	0	1	0	2	12
Aneurism	2	0	1	1	1	1	0	2	1	0	0	0	3	12
Diseases of the heart, &c.	14	18	20	20	16	12	18	9	10	14	25	15	9	200
Total Dis. of Heart, &c.	18	18	22	23	17	14	19	12	12	14	26	15	14	224

Causes of Death.	July. Week ending 4th	11th	18th	25th	August. Week ending 1st	8th	15th	22d	29th	September. Week ending 5th	12th	19th	26th
Class v.													
Teething	22	20	24	29	26	23	23	24	25	27	31	35	27
Gastritis, enteritis	19	17	25	25	19	28	47	39	25	40	38	31	34
Peritonitis	2	0	1	0	3	0	2	2	3	2	0	4	0
Tabes mesenterica	5	3	5	6	8	6	9	5	5	9	11	6	2
Ascites	0	1	1	1	2	0	2	0	0	1	1	0	1
Ulceration	2	0	2	3	1	2	0	2	6	1	0	1	0
Hernia	0	3	0	2	2	0	1	0	1	1	1	0	1
Colic or ileus	1	1	2	1	0	2	3	1	3	2	2	0	0
Diseases of the stomach, &c.	5	11	8	5	15	5	11	11	6	7	3	13	6
Hepatitis	1	3	1	0	0	2	1	1	0	1	3	2	0
Jaundice	1	0	3	4	1	5	3	2	2	1	2	3	3
Disease of the liver, &c.	6	8	8	9	7	16	11	6	14	4	9	15	12
Total Dis. of Stomach, &c.	84	78	80	85	84	88	115	92	90	96	101	110	80
Class vi.													
Nephritis	0	0	1	0	1	1	0	1	1	2	0	0	0
Diabetes	0	0	0	0	0	0	1	0	0	0	0	0	0
Stone	0	0	0	1	1	0	1	1	1	0	0	2	1
Stricture	0	0	0	0	0	1	0	1	1	0	0	0	0
Diseases of the kidneys, &c.	4	7	2	2	3	6	2	4	4	1	2	1	4
Total Dis. of Kidneys, &c.	4	7	3	3	5	8	4	7	7	3	2	3	5
Class vii.													
Childbed	5	9	5	7	5	8	2	9	2	3	6	6	4
Ovarian dropsy	2	0	0	2	0	0	0	0	0	0	0	0	1
Diseases of uterus, &c.	2	3	2	4	3	0	1	3	1	1	1	2	4
Total Dis. of Uterus, &c.	9	12	7	13	8	8	3	12	3	4	7	8	9
Class viii.													
Rheumatism	2	1	4	2	3	1	1	0	2	1	3	4	1
Diseases of joints, &c.	5	5	2	4	3	0	1	3	4	3	1	3	4
Total Dis. of Joints, &c.	7	6	6	6	6	1	2	3	6	4	4	7	5
Class ix.													
Ulcer	2	2	0	0	1	1	0	0	0	0	1	1	0
Fistula	0	0	0	0	0	1	0	0	1	1	0	0	0
Diseases of skin, &c.	0	1	0	0	0	3	0	1	0	0	0	0	1
Total Dis. of Skin, &c.	2	3	0	0	1	5	0	1	1	1	1	1	1
Class x.													
Inflammation	6	4	7	5	9	4	9	4	6	8	5	3	6
Hemorrhage	1	12	3	1	4	2	3	2	3	1	1	2	2
Dropsy	36	27	32	29	37	30	31	35	28	26	34	36	43
Abscess	3	2	4	2	1	6	6	6	4	4	3	1	7
Mortification	2	5	7	5	6	2	7	6	3	4	4	4	7
Scrofula	0	4	5	1	3	2	2	1	4	3	1	2	3
Carcinoma	8	8	7	10	9	4	6	8	13	9	8	6	8
Tumour	1	9	2	4	2	1	1	1	2	1	1	4	2
Gout	0	0	1	1	0	0	1	2	0	1	2	0	1
Atrophy	5	3	4	9	7	11	9	7	7	11	9	12	6
Debility	16	23	11	23	26	20	25	27	20	19	19	14	18
Malformations	0	1	0	0	2	0	2	2	1	0	1	1	2
Sudden deaths	23	16	11	12	4	5	6	11	6	12	11	[illegible]	15
Total Dis. of Uncertain Seat.	104	113	94	109	110	99	108	112	97	93	99	98	120
Class xi.													
Old age or natural decay	44	61	64	61	42	57	46	56	46	49	42	63	52
Class xii.													
Intemperance	2	0	0	0	2	0	0	0	0	0	2	1	0
Privation	2	1	0	0	0	0	1	0	0	0	0	0	0
Violent deaths	22	24	25	27	11	18	28	25	20	34	20	16	21
Total by Violence, &c.	26	25	25	27	13	[illegible]	29	25	20	34	22	17	21
Class xiii.													
Causes not specified	6	2	2	0	1	3	1	1	1	3	1	1	1
Total Deaths from all causes	831	840	867	851	829	848	868	875	890	857	853	821	[illegible]

ER OF DEATHS IN THE DIFFERENT DISTRICTS.

Districts.	Estimated Population in 1840.	Deaths during the Quarter.
s	308,921	1602
ts	414,458	2087
cts	369,722	2121
....	411,634	2441
ts	450,265	2757
al (Males 5714, Females 5294) ..	1,955,000	11,008

DR. CARSWELL.

guished pathologist has recently been appointed to the honorable office of physician to the King of the Belgians. We have much plea- a place in our pages to the following highly complimentary ad- d to him on resigning his chair in University College. We entirely sentiments contained in it, as we believe that the profession does more honest or more successful cultivator of pathological science, orable man, than Dr. Carswell.

ARSWELL, M.D., *Professor of Pathological Anatomy in University London, and Physician to University College Hospital, &c. &c.*

tudents of the faculty of medicine of University College have uch regret that they are about to lose the advantages which they ved from your services as professor of pathological anatomy, and clinical instructors. The painful feeling with which they con- r separation from them is, however, accompanied with all the admits of, by their being enabled to indulge the hope and the be- hange you are about to make is calculated to prove advantageous ore especially in the prospect it affords to you of a degree of hey have observed with sorrow to be apparently inconsistent with of the laborious and responsible duties with which you have been

t now permit you to depart without offering you their most nce how highly they appreciate the honour of having had for ne who has contributed so much to the advancement of scientific ey look back with gratitude and pleasure to the valuable infor- ve derived from your labours, and to the rare and high example ted love for science and devotion to its interests, which your con- led them. Of your private virtues they forbear to say much; le sentiments and conduct, your unsullied integrity, the urbanity er, and your eminently disinterested kindness are familiar to all the least acquaintance with you; and they desire to assure you, unostentatious and unobtrusive have been the acts by which these been manifested, they have nevertheless not passed unobserved, e unremembered.

offer you their warm congratulations on the evidence afforded by ppointment, that your merits have been appreciated by a sove- distinguished by his moral and intellectual endowments as by his ances and high station; and they now take leave of you, with the their sincere desire that the change you are about to make may of all the advantages you expect from it, and, especially, that it he perfect restoration of your health, and the enjoyments insepa-

rable from health, which they know you have sacrificed in their service. Wherever you are, be assured you will be attended by their grateful remembrances, and by their deeply-felt wishes for your future prosperity and happiness.

October 20, 1840. JOHN TAYLOR, *Chairman.*

(REPLY.)

To the Students of the Medical Faculty of University College.

Gentlemen,—It gives me the most unfeigned pleasure to receive from you such a public testimony of your kindness and esteem; and to have it thus made known to me, that I part from you with the assurance that I carry with me your sincere wishes for my health, my future prosperity, and happiness.

The honour, gentlemen, has been mine—that of having been your teacher; and, if you look back with gratitude and pleasure to the valuable information you say you have received from my labours, and which, in your opinion, have contributed so much to the advancement of scientific medicine, the more do I feel myself bound to express my gratitude to you, and to avow that your approbation of my humble merits is, indeed, no small reward for whatever sacrifice I may have made to the attainment of such gratifying and important results.

Be assured, gentlemen, that your opinion of my character in my social relations and intercourse with you, for a number of years, is the more pleasing to me that it marks a prominent feature in your own character, by which you are distinguished among all others of the same class as yourselves, and to which I feel justified in attributing much of the success and distinctions you have acquired by your professional industry and talent.

Accept, gentlemen, the expression of the same feelings and sentiments for your prosperity and happiness, which you have so warmly manifested for mine; and be assured that I shall always feel as deep an interest in your welfare as in the success and advancement of that institution under whose liberal and enlightened influence you have commenced your useful and honorable career, and in which I have had the proud honour of being enrolled among its professors.

ROBERT CARSWELL.

THE CLAIMS OF ITALY TO THE INVENTION OF LITHOTRITY VINDICATED BY RAMBELLI.

Is lithotrity or lithotripsy a modern invention, belonging entirely to France? Let us examine history. In 1533 Alessandro Benedetti da Legnano described certain methods for the trituration of the stone in the bladder. In 1550 Alfonso Ferri, a Neapolitan, invented an instrument, which he called after himself Alfonsino, for the extraction of bullets from gun-shot wounds, and which gave the idea of that which was in the sequel invented to extract and break the stone. In 1614 Baronio da Cremona first adopted the injection into the bladder of liquids to dissolve calculi, such as lemon-juice, &c., and thus far preceded Hales, Langrish, Bater, Rutherford, Fourcroy, Vauquelin, and others. In 1626 Santorio invented a canula, which contained within it an instrument with three branches, which opened for the extraction of foreign bodies in the bladder or the urethra; and within this instrument he passed a stiletto for the purpose of perforating the calculus when he could not extract it entire. In 1679 Antonio Filippo Ciucci, a surgeon of Arezzo, published his *Promptuarium chirurgicum*, from which it is quite evident that he preceded all others in the invention of lithotripsy.

Ciucci flourished in 1670, and practised surgery in Macerato with reputation, after having passed two years of practice at Rome under Dr. Giovanni Trullo. Besides the Promptuarium, he wrote a treatise on the circulation of the blood, with an appendix on the plague, and dedicated it to Francisco Redi. In the second part of his Promptuarium, he asserts that among the many remedies invented for urinary calculi, there is not any to compare with the *tenacula tricuspis*; which instrument, he says, is not altogether different "a pede gryphi

im quo molæ ab utero educuntur," but is unlike it, "quia istud non habet ·umina mucronata, sed retusa." He subjoins that "debet chirurgus et tena-ıla frustula diligenter quærere;" and that for the remaining heat and pain possumus injicere modicum lactis .... et sic absque incisione ad pristinam nitatem languentem conducemus .... cito, tuto, et jucunde." Ciucci had ch confidence in his method, that he himself submitted to it three times. He res a representation of his tenacula, which, as already said, is very similar to e ball-extractor of Ferri.

Lastly, Santarelli, a surgeon of Rome, in his researches to facilitate cathe-·ism (Vienna, 1795), proposes the straight catheter. Professor Cittadini, of ·ezzo, compares the instrument of Ciucci, invented in 1679, with that of viale in 1823; he recognizes a perfect similarity between the two, except at that of Ciucci is solid and full, and that of Civiale is hollowed for the pas-ge of the lithotritor, which acts by boring and destroying the calculus.

From these circumstances it may be concluded that this most important in-ntion does not belong either to Civiale or Gruithuisen (1813), or Egerton 319), or Leroy d'Etiolles (1821), or Amussat (1822). All of these dispute the nour of having preceded Civiale in the invention and description of the ope-tion of lithotrity; an operation which, it is evident, was imagined and exe-ted by two illustrious Italians, Santorio and Ciucci.

Il Filiatre-Sebezio. Marzo, 1840. (From the *Annali Medico-Chirurgici.*)

THE QUEEN ADELAIDE FUND OF THE HANWELL LUNATIC ASYLUM.

This is a charity attached to the great Middlesex County Asylum, and was in-ituted for the relief of destitute patients at the time of their recovery. Its nature ıd object, and the great benefits it is calculated to afford to the unhappy subjects 'it, are set forth so admirably in the following letter, that we are gratified in eing able to give it extensive circulation in our pages; and are not without hopes ıat some of our readers may be induced by its perusal to contribute to the charity · to induce others to do so. We the more readily republish the letter that it con-.ins some striking though brief illustrations of the causes and miserable conse-ences of insanity among the poorer classes:

"Dear Sir, *Hanwell; February* 3, 1840.

"In the Report which I had lately the honour to lay before the visiting magis-ates of the asylum, it was observed that the extreme poverty of some of the pa-ents, and the certainty that on being discharged from the asylum they would re-ırn to a miserable home, and be exposed to many causes most likely to produce relapse, sometimes occasioned hesitation respecting the propriety of sending them way, after they were sufficiently restored to reason to make their restoration to e ordinary habits of life desirable. It was added, that the benefit arising, in many instances, from the Adelaide fund was so great and so evident as to make s limited extent deeply to be regretted.

"Having almost daily opportunities of seeing the good done by this excellent nd, and, I am sorry to add, of seeing cases to which its limited assistance cannot e extended, and in which such charitable help is greatly required, I trust you will ermit me to lay before you a few particulars illustrative of these circumstances.

"It very often happens that insanity makes its first advances slowly; manifest-ng itself by eccentric and irregular conduct, fits of illness, of dissipation, or of xtravagance long before it is clearly recognized. These first symptoms of the isorder, inconvenient in every rank of life, are ruinous to a family dependent on he daily labour of a husband and a father.

"By their frequent recurrence or by their long continuance, every comfort is uccessively sacrificed, and every article of furniture and dress that can possibly be pared becomes pledged for a little money to meet the daily necessities of the pa-tient, of the helpless children, and of his almost broken-hearted wife; who is long unable to account for the demoralization which is bringing ruin upon the whole family. At length the malady becomes too plain to be mistaken, and the poor

lunatic, after being delayed a short time in the workhouse, and a longer time in some cheap private lunatic house, is brought to the asylum; and his wife and children are taken care of by the parish. In a few months, perhaps, the poor man recovers. He then begins daily to represent to us the deserted state of his family and his anxiety to help them; and becoming at length quite well he is discharged. When he takes off the asylum dress, he clothes himself in the ragged worn out garments which have been kept for him at the workhouse; often fancying that his best clothes have been stolen, forgetting how he parted from them; and going away in some degree of irritation on this account. He then either goes to the workhouse or into the poorest lodging in the lowest retreats of destitution. He finds more difficulty in obtaining work than he expected. His having been insane operates against the success of his efforts to be employed; he is pressed with daily wants which were not felt or thought of in the asylum; and in short, exposed, immediately after his recovery, to every probable cause of relapse.

"In many instances the patient's malady has been of longer duration. When he leaves the asylum he finds that his friends are dead or have almost forgotten him; and he learns all at once the troubles with which those for whom he feels affection have long been struggling. Many of these patients first become insane after long contention with all these evils; and were worn and harassed by various wretchedness until they lost their reason. When we turn any of these unfortunate persons out of the gate pennyless, we at once expose them to a repetition of the causes of their first attack of madness or of melancholy.

"The instances are numerous in which poor widows are admitted, distracted by the failure of some humble business, to the carrying on of which they were unequal after their husband's death. Affliction and sorrow in these cases commonly produce the most marked examples of a profound and speechless melancholy, from which the recovery is slowly effected if effected at all. No means of assisting or of confirming recovery are so likely to be efficacious as being able to hold out the promise of a little aid toward the re-establishment of some business by which these patients, when restored to some degree of cheerfulness, may look forward to being enabled to live honestly. I believe the benevolent persons who have superintended the formation and distribution of the Adelaide Fund have witnessed not a few most affecting cases of this kind; in which, also, the aid derived from the fund became the blessed instrument of regained prosperity.

"Of 244 female patients, of whom the station or occupation are mentioned in a table appended to the Michaelmas Report of the Asylum, 125 were domestic servants. These poor women, when recovered, are of course seldom able at once to find places. Their affliction has seldom been concealed from those who know them or with whom they formerly lived. Unless they can go to their parents the workhouse is their only resource. It often happens that their parents are extremely poor, and ill able to support any additional burthen. A small donation in these cases gives the destitute girl a kind of welcome to her home, and enables her to go to it with confidence and cheerfulness, and she exerts herself and does well. Without such help and her mind still weak, her condition would often have become very lamentable.

"We have also, at all times, among our patients, some in whom, after the severer symptoms of their malady have disappeared, a slight disorder or impairment of the mind remains, or a certain eccentricity of manner, or a disposition to excitement when contradicted or not judiciously managed. These patients under the guardianship of the various officers and servants of the asylum who are familiar with their character, are most industrious labourers, or at least most serviceable assistants, and, whilst pleased and engaged in different employments of more or less consequence, are placed in circumstances extremely favorable to permanent cure. But these patients, conscious that they are useful, and sometimes even overrating their services, would often become discontented and refuse to leave their wards, or to be in any way active without the encouraging hope of assistance when discharged. In these cases, the prospect of pecuniary aid, although they know it can only be trifling, becomes auxiliary to the perfect cure of the patient, promotes

imulates activity, and at the same time contributes to restore the y and of mind.

ten placed in a peculiarly painful situation as respects country pa- ve no parish to return to. The miserable fate that inevitably awaits causes us, indeed, occasionally to keep them week after week be- r that they have, whilst with us, needful food and clothing and shelter, ible when they leave us to furnish them either with clothes or with nnot bear to see them turned out of the asylum in rags, and without without a friend. In these circumstances we cannot always refrain g a little help to them, for which we should be glad to have fuller thority. A suit of clothes, costing about twelve or fifteen shillings, money not exceeding one or two sovereigns, when the recovered pa- g journey before him, are absolutely necessary to prevent their being agrants or even perishing on the road. I am convinced, both by communicated to me and by my own observation, that assistance to , in some cases, perhaps in many, saved a poor man or woman from nent and despair, has been the means of restoring them to their s, and of securing their subsequent well-doing.

occupied in the treatment of a class of maladies, the history of so much physical and moral weakness, and circumstances of such d terrible distress, we feel that we have done but a part of our duty merely contributed to restore the sanity of the mind, and anxiously mpletion of our efforts to any possible means of restoring the victim position in society, promising some tranquillity of heart and a con- of moderate prosperity. Except the opportunities afforded to us by Fund, to the benevolent founders of which we can never feel suffi- l, we have no means of effecting these most desirable ends. The ients in the asylum when that fund was established did not exceed ounts to 834, and preparation has lately been made for the recep- re. If, therefore, the fund could be so brought before the attention of Middlesex, and of benevolent persons in other parts of the king- w them how important its benefits are to the largest lunatic esta- e kingdom, built solely for the relief of paupers, it might possibly l as to enable the magistrates and the officers of the asylum to ex- ible succour to many for whom, at present, there is unfortunately no y kind, no hope, and no resource.

"I remain, very sincerely, dear Sir,

"Your obedient servant,

"J. CONOLLY.

A. Tulk, Esq., Chairman of the
gistrates of the County Lunatic Asylum."

ESTATION OF COWS. BY THE RIGHT HON. EARL SPENCER.

ring observations are highly valuable, and deserving the attention gist and statistician. Their important analogical bearing on the f human gestation is obvious; as they supply an element, on a hich can rarely be obtained in the other case, viz., the exact egnation.]

ose partly of curiosity and partly because I thought the notions pecting the ordinary period of gestation of cows incorrect, I several an to take notes, whenever a cow calved, of the length of time regnant; and, having now the periods of gestation of 764 cows ay, I think a sufficient number of cases has been collected to draw general conclusions from the observations which I have

by inserting a Table which will show how many cows producing e gone each of the different periods therein mentioned. The first the number of days of gestation; the second the number of ve gone each period; the third and fourth columns show whether

the produce was a cow-calf or a bull-calf; the fifth, if it was twin cow-calv the sixth, if it was twin bull-calves; and the seventh, if it was twins of differ sexes. For instance, if 279 is taken, it will appear that 32 cows went 279 da that 16 of them produced cow-calves, 11 of them produced bull-calves, 3 of th produced twin cow-calves, none of them produced twin bull-calves, and 2 of th produced twins of different sexes.

No. of days of Gestation.	Cows.	Cow Calves.	Bull Calves.	Twin Cow Calves.	Twin Bull Calves.	Twin Cow and Bull Calves.	No. of days of Gestation.	Cows.	Cow Calves.	Bull Calves.	Twin Cow Calves.	Twin Bull Calves.	Twin Cow and
220	1	..	1				276	15	7	6	..	1	
226	1	1					277	14	10	2	1	..	
233	1	..	1				278	16	11	4	1	..	[illegible]
234	1	..	1				279	32	16	11	3	..	[illegible]
235	1	1					280	35	15	20			
239	1	1					281	39	20	18	..	..	
242	1	..	1				282	47	26	20	1		
245	2	2					283	54	30	24			
246	2	..	2				284	66	33	33			
248	1	1					285	74	29	43	..	..	[illegible]
250	1	1					286	60	22	38			
252	2	..	2				287	52	25	27			
253	1	..	1				288	42	13	28	..	1	
254	1	1					289	45	20	25			
255	2	..	2				290	23	10	13			
257	2	1	1				291	31	9	22			
258	3	1	2				292	16	5	11			
260	1	..	1				293	10	1	9			
262	1	..	1				294	[illegible]	1	7			
263	2	..	2				295	7	3	4			
266	1	..	..	..	1		296	6	2	4			
268	2	2					297	2	1	1			
269	2	..	1	..	..	1	299	1	..	1			
270	5	2	1	1	..	1	304	1	1				
271	6	5	1				305	1	1				
272	3	1	1	..	1		306	3	3				
273	3	2	1				307	1	1				
274	5	..	5				313	1	1				
275	6	2	2	..	1								

From the inspection of this table it will be seen that the shortest period gestation, when a live calf was produced, was 220 days, and the longest 313 da but I have not been able to rear any calf produced at an earlier period than 2 days. Any calf produced at an earlier period than 260 days must be conside decidedly premature; and any period of gestation exceeding 300 days must a be considered irregular; but in this latter case the health of the produce is affected. It will also be seen that 314 cows calved before the 284th day, a 310 calved after the 285th; so that the probable period of gestation ought to considered 284 or 285 days, and not 270 as stated in the book upon Cattle, pu lished under the superintendence of the Society for the Diffusion of Use Knowledge.*

* In another work, however, entitled "British Husbandry," published under superintendence of the Society for the Diffusion of Useful Knowledge, the experim of M. Teissier, of Paris, on the gestation of cows, are recorded to have given following results:

21	calved between the	240th and 270th day,	the mean term being	259¼
544	ditto	270th „ 299th „	„	282
10	ditto	299th „ 321st „	„	303

"In most cases, therefore, between 9 and 10 months may be assumed as the u period; though with a bull-calf, she has been generally observed to go about 41 we and a few days less with a female." (*Vol.* ii., p. 438.)—F. Burr.

ears also that the number of breeding females is less considerably than ber of males; and to the number of males ought generally to be added, ls that will not breed, the females who are twins with males. I have d believe that in some cases a cow-calf, twin with a bull, will breed; instance in which I have bred twins of different sexes has the female reeding heifer. The number of breeding heifers from these 764 cows the number of bull-calves 422; and the number of heifers twin with ually called fremartins, 11.

is a prevalent belief among farming men, and I believe farmers, that, time of gestation of a cow is longer than usual, the produce is genenale calf. I must confess that I did not believe this to be the case, but shows that there is some foundation for the opinion. In order fairly s, the cows which calved before the 260th day, and those which calved 300th, ought to be omitted as being anomalous cases, as well as the which twins were produced: and it will then appear that, from the cows eriod of gestation did not exceed 286 days, the number of cow-calves was 233, and the number of bull-calves 234; while, from those whose xceeded 286 days, the number of cow-calves was only 90, while the of bull-calves was 152.

Journal of the English Agricultural Society. Part ii., 1839.

BOOKS RECEIVED FOR REVIEW.

ENGLISH BOOKS.

arks on the Surgical Practice of llustrated by cases. Being a which a gold medal was asthe Senatus Academicus of the University, at the graduation of W. O. Markham, M.D.—London, o, pp. 114. 5s.

ingements, primary and reflex, gans of Digestion. By Robert D. — Edinburgh, 1840. 8vo, 7s. 6d.

ractical Treatise on the Cure of s or Squint, by operation and by atment; with some new views of ny and physiology of the muscles nan eye. By P. Bennett Lucas, Illustrated by Plates.—London, o, pp. 91. 6s.

actical Treatise on the Diseases o Women, illustrated by Cases m hospital and private practice. well, M.D., Obstetric Physician rer to Guy's Hospital. Part I. , 1840. 8vo, pp. 208. 7s.

's Hospital Reports, No. XI. 8vo, Plates. 6s.

the Nature and Treatment of and Urinary Diseases: being an to the Connexion of Diabetes, and other affections of the Kidney er, with Indigestion. By Wm. D., F.R.S. 8vo, pp. 484. 20s.

Cyclopædia of Practical Surgery. W. B. Costello, M.D. Part VII. , Chondritis. 5s.

Invalid's Guide to Madeira, with tion of Teneriffe, Lisbon, &c. By W. W. Cooper, M.R.C.S. — London, 1840. 12mo, pp. 116. 4s.

9. The Elements of Materia Medica; comprehending the Natural History, preparation, properties, composition, effects, and uses of medicines. Part II. containing the Vegetable and Animal Materia Medica. By Jon. Pereira, F.R.S.—London, 1840. 8vo, pp. 879. 24s.

10. A Treatise on the Structure, Functions, and Diseases of the Foot and Leg of the Horse. By W. C. Spooner, M.R.V.C. —London, 1840. 8vo, pp. 337. 7s. 6d.

11. A Practical Treatise on the Bilious remittent Fever; its causes and effects: with illustrative Tables and Cases on the temperature of the system in the diseases of Jamaica. By W. Arnold, M.D.—London, 1840. 8vo, pp. 320. 10s.

12. Human Physiology. By John Elliotson, M.D. Cantab. F.R.S., with which is incorporated much of the elementary part of the Institutiones Physiologicæ, of J. F. Blumenbach. M.D., F.R.S. Illustrated with numerous woodcuts. Fifth edition. —London, 1840. 8vo, pp. 1194. £2 2s.

13. Second Annual Report of the Registrar-General of Births, Deaths, and Marriages in England. — London, 1840. 8vo, pp. 247.

14. Elements of Chemistry. By the late E. Turner, M.D., F.R.S., &c. Seventh edition. Edited by Justus Liebig, M.D., and W. Gregory, M.D. — London, 1840. 8vo, pp. 988. 21s.

15. Vital Statistics of Scarborough. By John Dunn, Surgeon. — London, 1840, 4to, pp. 11.

16. A Discourse on the Phenomena of Sensation, as connected with the mental, physical, and instinctive faculties of man. By James Johnstone, M.D., Physician to the Birmingham General Hospital, &c. London, 1841. 8vo, pp. 264. 8s.

17. A Treatise on the Nervous Diseases of Women; comprising an enquiry into the nature, causes, and treatment of spinal and hysterical disorders. By Thomas Laycock, M.D. — London, 1840. 8vo, pp. 356. 10s. 6d.

18. Practical Observations on the Treatment of Stricture of the Urethra, with Cases. By Robert Wade, Surgeon to the Westminster General Dispensary, &c. —London, 1841. 8vo, pp. 149. 5s.

19. The Pathology and Diagnosis of Diseases of the Chest, comprising a rational exposition of their signs. By Charles J. B. Williams, M.D., Professor of Medicine, University College, &c. — London, 1840. 8vo, pp. 331. 10s. 6d.

20. Medical Notes and Reflections. By Henry Holland, M.D., F.R.S., &c. Second Edition.—London, 1840. 8vo, pp. 638. 18s.

21. A Lecture on Toxarthus or Club Foot. By J. D. Mütter, M.D.—Philadelphia, 1839. 8vo, pp. 104.

22. The Elements of Geology, for Popular Use. By Charles A. Lee, M.D., &c. —New York, 1840. 12mo, pp. 375.

23. Thesis on the Nature and History of Plague, as observed in the North Western Provinces of India, for which a gold medal was awarded by the University of Edinburgh. By Frederick Forbes, A.M., M.D., of the Bombay Army.—Edinburgh, 1840. 8vo, pp. 102.

24. The Anatomy of the Arteries of the Human Body; with its applications to Pathology and operative Surgery. In lithographic drawings, with practical commentaries. By Richard Quain, Professor of Anatomy in University College. With delineations by Joseph Maclise, Esq, Surgeon. Parts I, II, and III. 15 Plates, super royal folio. Letter press, 8vo, pp. 112. —London, 1840. 12s. each Part.

25. Willis's Illustrations of Cutaneous Disease. Parts XX, XXI. 5s. each Part.

26. Practical Remarks on the New Operation for the cure of Strabismus or Squinting. Illustrated with lithographic Engravings. By G. W. Duffin, M.D. &c. —London, 1840. 8vo, pp. 147. 6s.

27. An Address to the Medical Practitioners of Ireland on the subject of Vaccination. Second Edition. By S. B. Labatt, M.D., &c.—Dublin, 1840. 8vo, pp. 202. 6s.

28. The Sciences accessory to Medicine essential to its successful Cultivation, the relations of these to one another, and the delights that attend the zealous study of each; a discourse delivered at t[…] gate School of Medicine, as an in[…] to the Session of 1840-41. By […] M.D., &c. London, 1840. 8vo.

29. Practical Remarks on t[…] mination and Appearances of […] Disease, &c. By John Howshi[…] to Charing Cross Hospital. Lon[…] 8vo, pp. 420. 10s. 6d.

30. Demonstrations of Anato[…] a Guide to the Dissection of th[…] Body. By G. V. Ellis, one of th[…] strators of Anatomy in Universit[…] London, 1840. 8vo, pp. 620.

31. An Enquiry into the E[…] Digitalis, in the Treatment of […] Epilepsy. By E. Sharkey, A.[…] London, 1841. 8vo. pp. 80. 4[…]

32. Outlines of a Course of […] on Medical Jurisprudence. By T[…] M.D., Professor of Medical Jur[…] in the University of Edinburgh[…] Edition.—Edin. 1840. 8vo, pp.

33. Dr. Ramsbotham's Atlas […] illustrative of Obstetric Medici[…] X, XI, XII. 1s. 6d. each Part.

34. Spinal Diseases: with a[…] plan of Cure. Including what […] monly called Nervous Compla[…] J. H. Robertson, M.D.—Glasg[…] 8vo, pp. 160.

35. Fifth Report of the Inspec[…] Prisons of Great Britain. III. […] and Western District. By Bisset[…] M.D.—London, 1840. Fol. pp. 1[…]

36. An Introductory Lecture o[…] By James Miller, one of the S[…] the Royal Infirmary, Edinbur[…] 1840. 8vo, pp. 23.

37. On Scientific Medicine, […] lations to claims upon Society […] an address read before the Nort[…] land Medical Association at C[…] the 15th Sept. 1840. By Willi[…] M.D.—Carlisle, 1840. 8vo, pp.

38. A Treatise on the Sympa[…] lation between the Stomach and […] and, throughout, between the […] and the Nervous Systems, in the […] and cure of Diseases. By Chas. […] M.D.—London, 1840. 8vo, pp. 1[…]

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41. Practical Surgery; wit[…] gravings on wood. By Robert […] Edition.—London, 1840. 8vo, p[…]

APPENDIX.

THACKERAY PRIZE ESSAY.

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on the Sources and Mode of Propagation of the Continued Fevers Great Britain and Ireland. By WILLIAM DAVIDSON, M.D., Senior ysician to the Glasgow Royal Infirmary, Lecturer on Materia Medica, mber of the Faculty of Physicians and Surgeons of Glasgow, &c.

CHAP. I. ON THE SOURCES OF CONTINUED FEVERS.

ERROUS kinds of continued fevers have been described by authors; but of these have been found, on investigation, only particular varieties, in of being distinct species. This has been particularly the case with ty- the most prevalent kind of continued fever in this country; and it may ounted for, from its numerous and diversified complications giving various and multiform symptoms. The pathology of typhus, er, of late years has been considerably advanced; and it is now shed, that this disease may be either simple or complicated, with c affections of one or all of the different cavities of the body.

shall not, at present, enter into any discussion respecting the va- kinds of continued fever that are to be met with in the United om; but as perspicuity in arrangement requires some classification, ll adopt the following, reserving the illustrations upon which this ication is founded for a future part of the essay:

1. Typhus.
2. Febricula or Simple Fever.
3. Gastric or Intestinal Fever.

se affections seem to be distinct species of disease, differing in their oms, causes, and laws; and are generally treated in private practice hospitals as continued fevers. To this list may, perhaps, be added itis which, although an inflammatory affection, is more frequently nded with them than any other disease.

SECT. I. SOURCES OF TYPHUS.

ppears to us quite unnecessary here to describe what is understood hus fever; as we assent to the general correctness of our standard s upon this subject; and as some of their descriptions will be d in another part of this essay. At the same time it may be re- ed that typhus possesses an advantage, over the other forms of con- d fever, in having a distinctive characteristic, viz., the eruption, is present in none of the others, and which is now almost univer-

sally acknowledged as decisive of its existence. It must, however, b admitted that typhus can and does occur, without its characteristic eru tion; but it is equally certain that the large majority of patients wl have decidedly the general typhoid symptoms, are more or less spott with this efflorescence. It is therefore the sources of typhus as gen rally so characterized, which we mean to trace in this section.

There is considerable diversity of opinion amongst British physiciai respecting the causes of continued fevers; but certainly the majority authors have adopted the belief that typhus is propagated by contagio The opinion of the majority appears to be supported by the facts co nected with the progression of the disease; it shall therefore be our o ject to establish this point. It is not intended, however, to enter in any speculations respecting the primordial source of the contagion typhus; for the sources from which it, as well as that of other contagiou fevers, originated, is involved in absolute obscurity; and though we coul trace them to the most remote era in antiquity, the same difficulty would be encountered. Some authors, apparently to get rid of this difficulty and to account for the occurrence of typhus where no contagion could be traced, have adopted the opinion that it may be generated by common causes, such as impure air, filth, &c., and be afterwards capable of propagation by contagion.* The argument of analogy is directly opposed to this belief; for if, in nature, there be no exception to the law, that two causes are never required to produce precisely the same effect, it will follow, that whatever cause can be best reconciled with the phenomena of typhus, must be considered the true source of the disease. But, in order to apply this principle more immediately to the subject, it may be necessary to appeal to the various morbid poisons, the laws of which are known and generally admitted. The first we shall notice are those which are admitted by all writers to be propagated by one cause only; viz. matter, whether ponderable, as the pus contained in a variolous pustule, or imponderable, as the effluvia issuing from a patient labouring under smallpox. Measles, scarlet fever, hooping-cough, are propagated only by the *effluvia* which are generated by the patient; and though the material body, by which it is effected, cannot be collected into vessels, like the various gases, still the proofs, upon which their contagious qualities are based, are as unquestionable as those of smallpox. Almost all the contagious diseases of the skin, such as syphilis, scabies, the yaws, sivvens, &c., furnish examples of propagation by only one cause, viz. contagion. There are no doubt authors who maintain that some cutaneous diseases are generated by filth, &c., such as some of the infectious species of porrigo; but of this there is no proof; and in all probability it is equally unfounded as was a similar hypothesis respecting the origin of scabies. These diseases always retain the same characteristics; the one is not convertible into the other; and no known combination of them can generate a new contagion capable of perpetuating a new disease. A specious objection might be brought forward against the introduction of an analogy from chronic contagious diseases, which are only propa-

* The terms contagion and infection are used synonymously, as indicating the ponderable or imponderable matter, which is generated in a diseased living body, and which is capable of producing the same disease when applied to another.

gated by contact, according to the general belief; and besides are regulated in other respects by different laws, whereas typhus is propagated only by effluvia. It is quite evident that a class of diseases may be recognized by one leading and undeviating law, while they differ in many of their subordinate characters; and yet this peculiar law of similarity between them may be as certain and definite as if they had been united into one family by all their habits. The effluvium which issues from a smallpox patient must be as essentially matter as is the fluid of a variolous pustule; for though the first cannot be collected in a separate form, it must possess one or other of the properties of smallpox matter, else it could not induce the disease; the only difference between them consisting in this, that the contagious matter is fluid in the one case and effluvial in the other. It is necessary that the foregoing observations respecting contagious diseases be kept in view; for upon the analogy between them and typhus we mean to establish an argument, that the latter disease can only be propagated by one cause. If it be true then, that all the contagious fevers known can be propagated only by contagious matter, and by no other cause, however much their contagious qualities, their prevalence, and their fatality may be increased by other causes, it must follow from the law of analogy that if typhus can be proved to be contagious, it must also be propagated only by one cause, viz. contagion. We shall, therefore, endeavour to prove this point; and, in the outset, it may be stated, that we do not mean to fatigue the reader by stories about fomites and persons who have carried the contagion about them for months or years, nor to hunt out a particular individual who has conveyed it from one town to another. In place of accumulating evidence of this kind, which although sometimes very conclusive, is in other cases somewhat questionable; we shall select a few facts from the history of our British and Irish hospitals, which, we trust, will be sufficient for our purpose; for if it can be established from these documents that the disease was contagious in all the large hospitals of Britain and Ireland, then it must be more or less so in every other place. In selecting the facts, we shall adduce the most conclusive instances, such as where the whole or almost the whole of the attendants of the patients affected with typhus were infected; for were the whole body of evidence existing on this subject accumulated in this essay, the argument would be encumbered, and the proofs perhaps rendered less convincing. Those who deny the existence of typhous contagion may assert that this is unfair, and that those hospitals also should be brought forward, where the medical and other attendants were rarely affected. As we shall, however, notice this and other objections elsewhere, it need not be farther alluded to here. Drs. Barker and Cheyne in their admirable report of the fever which prevailed in Ireland during the years 1817-18-19, state that "in the hospitals of the house of Industry of Dublin, no clinical clerk or apothecary escaped an attack of the disease; and on the 20th January, 1819, it was reported to Government that five of the medical attendants of the house of Industry were at that time lying ill of the disease. In the city of Cork nine physicians in attendance either on dispensaries or fever hospitals were attacked; every medical attendant at the South Fever Asylum in that city suffered. At the hospitals of the House of Industry, 170 per-

sons were employed in different offices of attendance on fever patien and from this part of the establishment were recorded 198 cases of feve In Dr. Crampton's medical report of the department of Steevens' H pital it is observed "that, with the exception of Dr. Harvey and hims all those concerned in attendance on the patients caught the disea none of the nurses, none of the porters, barbers, or those occupied i handling, washing, or tending on the sick escaped, and many of them had relapses and recurrences of fever."*

Dr. Bracken in his report of the Fever Hospital of Waterford for 1818, states that "there were twenty-seven attacks and relapses of fever among the nurses, servants, and porters, whose number fluctuated according to the demand for them, but who, on an average, may have been about twenty-two during the year." He farther states that "the present year 1819 bears a close resemblance to the last, in respect to the nurses and servants being attacked with fever, eighteen of the former having suffered under the disease; seven of them once, three twice, and one three times." The apothecary, who had not been long in the hospital, caught fever and relapsed twice. During his illness, a young man, who performed part of his duties, was attacked after a short attendance. A temporary apothecary was then engaged for a few weeks; but he had not been many days in his new employment when he also contracted a fever."†

Drs. Barker and Cheyne remark that clergymen who visited typhus patients in Ireland during the epidemic were also observed to suffer in a very remarkable degree; and they quote the following passage from Dr. Stokes' Essay on Contagion, which was published at a time when the fever had made little progress in Dublin: "The deaths from fever recorded in Saunders's News-letter, from August 1st to December 12th following, are sixty-four, and of these nineteen are of clergymen of some of the different persuasions, or of medical men of different descriptions, which appear greater than the proportion which these two classes bear to the whole of those whose deaths we may suppose were mentioned in that manner."‡ Dr. Tweedie in his Clinical Illustrations of Fever for 1828-29, observes that "the London Fever Hospital is placed in an open space, situated in the vicinity of the metropolis, close to the Smallpox Hospital. Both these establishments stand in the centre of a large field, where the production of malaria is extremely improbable. I can state from the most authentic sources, that every physician who has been connected with it, with one exception (the late Dr. Bateman), has been attacked with fever during his attendance, and that three out of eight physicians have died. The resident medical officers, matrons, porters, laundresses, and domestic servants not connected with the wards, and every female who has ever performed the duties of a nurse, have one and all invariably been the subjects of fever; and to show that the disease may be engendered by fomites in clothing, the laundresses, whose duty it is to wash the patients' clothes, are so invariably and frequently attacked with fever, that few women will undertake this loathsome and

* Barker and Cheyne's Report of the Fever in Ireland, vol. i. p. 135.
† Ibid., vol. i. p. 276.
‡ Ibid., vol. i. p. 138.

ntly disgusting duty. Last summer a most convincing illustration ntagion occurred. The present resident medical officer was at- l with fever, and it was necessary, in consequence, to appoint some perform his duties during his illness. The first person who offi- for him resided constantly in the house during the day, but took ecaution of sleeping at home. He was, of course, very much ex- in the wards in the performance of his duties. These, however, oon interrupted by an attack of fever, which confined him for a erable time. The duties were then undertaken by a medical pu- o had completed his education, and entered the hospital in the robust health. He had been taught, and did implicitly believe, non-contagious nature of fever, and ridiculed the idea of any al danger from residing in the hospital. He performed the duty se-surgeon for ten days only, when symptoms of a severe fever red."* Dr. Tweedie also adduces some important facts connected he fever which prevailed in Edinburgh during the year 1817, are the following: Owing to the prevalence of fever at urgh in 1817, it was necessary to apply to government to Queensbury House to be employed as a fever hospital: "In mediate neighbourhood of this extensive building fever was dly less prevalent than in any other quarter of the town. All however, who resided in the hospital, including the resident house- n, clerks, apothecary, and nurses, were successively attacked." ollowing is Professor Alison's report on this subject. "When sbury House was formerly occupied by fever patients, every resi- lerk and every nurse in the house were successively affected with the e; and since it was reopened in December last (1826), the resident ian, two of the clerks (who have not been resident, but have been l hours in the day in the house), the apothecary, several servants, l the nurses except two, in all above forty individuals, who had arily close intercourse with the sick there, have had fever. If this effect of a malaria, it must be a very virulent and effective one, is reasonable to expect that some record of similar visitations in rmer history of the building would be found. But Queensbury has existed for about a century; it was long occupied as a private g-house by the noble family of that name; afterwards it was oc- by a number of families, and afterwards as a soldiers' barrack; t no record can be found of its having been, during these changes, t of an epidemic fever. If a malaria has existed, therefore, in ouse, it must, on both occasions, have sprung up exclusively at es when fever patients were removed thither and lasted only du- eir stay. During the present epidemic (1827-28, as well as that of 19), many of the clerks and nurses employed in the Royal Infir- ave taken fever. Since November last, six of the clerks employed clinical wards only, four of those employed in the ordinary wards, enty-five nurses or servants have taken fever. All these persons ecessarily frequent and close intercourse with the fever patients in ouse, having been employed more or less constantly in the fever

* Tweedie's Clinical Illustrations of Fever, p. 87.

wards, excepting only four of the servants. Of these four, two had been employed in the laundry, where the linen from the fever wards was washed. One was a porter employed at the gate, who would, of course, have communication with the fever patients at their entrance and dismissal, as well as with their relations coming to visit them; and one was a nurse employed in the servants' ward, but who was in the habit of visiting the fever wards." He adds further: "No one of the nurses, whose duty has confined them to the medical or surgical wards, where no fever patients were admitted, has taken fever, with the single exception of the woman in the servants' ward above mentioned; and of the numerous patients in these ordinary wards, the only one who has taken fever, within my knowledge, during the present year, was a patient in the men's general clinical ward, who lay in the bed next the door that communicates with the clinical ward."* Dr. West, in his account of the cases of typhus exanthematicus that occurred in St. Bartholomew's Hospital in 1837-38, states that "since last summer, eleven gentlemen who were in the habit of frequenting the hospital have been attacked by the fever, to which three have fallen victims; sixteen nurses and twenty-one patients admitted for other affections, have likewise suffered from the disease, which terminated fatally in ten instances, and I do not doubt but that many similar cases occurred which did not come under my notice."† Dr. Roupell, also, gives similar testimony in reference to St. Bartholomew's Hospital, and states that "amongst the nurses, in attendance upon the sick, in that establishment, infection was almost universal."‡ In the Glasgow Fever Hospital, which is capable of containing 220 patients, during the last six or seven years almost every clerk and nurse of that establishment have caught fever while acting in the wards, unless they had previously laboured under the disease. On the other hand, the nurses connected with the medical and surgical wards, in the adjoining building, have almost uniformly escaped. Occasionally a case has appeared in the medical and surgical wards; but this fact ought to be coupled with the statement, that now and then typhus cases are sent, by mistake, into the medical wards, and cases of bed-sores, gangrene of the feet, &c. are transmitted from the fever hospital to the surgical wards. Dr. Cowan states that "All the gentlemen who have acted as clerks in the fever hospital for many years past have been attacked with fever, unless they had it previously to their election. During last year, twenty-seven of the nurses of the establishment were seized with fever, and five of them died, several of the students have been affected. One gentleman who acted as apothecary died in the house; and if I have escaped, it must be attributed either to being past the period of life at which fever usually takes place, or to my being secured by having had two dangerous attacks at an earlier period of my career, when acting as physician's clerk in the infirmary, during the epidemic of 1816-17-18."§

* Tweedie's Clinical Illustrations of Fever. Edinb. Med. and Surg. Journal, vol. xxviii. p. 238.

† Ibid., July, 1838, p. 143.

‡ Roupell on Typhus.

§ Cowan's Vital Statistics, p. 26.

Mateer gives a table of 9,588 cases, which were admitted into
elfast Fever Hospital, from 1818 to 1835, showing the number of
ts who had any communication with affected persons, either by
nce in the same house, or by belonging to the same family. He
the following conclusion from the table: "It thus appears that
mber of families where contagion is traceable is 1,856, that the
umber of persons belonging to them is 7,246, making an average
rly four individuals to each family; and that the single cases,
the disease seemed to have arisen from other sources, amount only
2."*

assemblage of facts has been drawn from the large hospitals in
d, Scotland, and Ireland, and the observations have been made
various years and during different epidemics by gentlemen of the
t talents and respectability; their authenticity cannot, therefore,
noment be questioned. The simple relation of these facts would, we
with the majority of men, produce conviction that fever was at
ontagious in these hospitals, provided the mind was not preoccu-
ith an opposite theory; but a few observations will tend to pro-
proper estimation of this testimony. It is quite obvious, that where
h larger proportion of persons is affected with any particular dis-
n any particular place, than occurs amongst the general commu-
or in any particular grade of society, there must be some local
for that increased ratio. This has, manifestly, been the result in
er hospitals that have been enumerated; for in all, a very large
ty of the attendants, and in some the whole of them, were affected
ever. Now, no one will contend, that even amongst the lower
(who generally suffer from fever to the greatest amount), such a
tion has ever been maintained, even in our most severe typhoid
ics; but if the number of hospital clerks be taken and compared
e unaffected number, in the particular grade of society to which
elong, such an attempt would be ridiculous; for the united testi-
of the hospital physicians of England, Scotland, and Ireland
statements we have already quoted) amounts to this, that almost
lerk of a fever hospital has laboured under fever during some
of his attendance upon it. It may be contended, in answer to
gument, that the atmosphere of the hospitals was contaminated
exhalations arising from the number of patients and the want of
ventilation; but the same process of atmospheric deterioration
to take place, in the medical and surgical wards, if they be equally
which is generally the case; and in the latter wards there are
in addition to the ordinary exhalations, the effluvia arising from
s, ulcers, &c., yet typhus rarely, and only in sporadic cases,
s up there.

opponents of contagion, however, endeavour to explain the pre-
e of fever among hospital attendants by the hypothesis, that the
cause that produced it in the filthy, ill-ventilated houses of the
classes is in existence in these institutions; viz., a peculiar ma-

* Dublin Journal of Medical Science, vol. x. p. 35.

laria generated, chiefly, in large towns. If this hypothesis were true, it would follow, as a necessary consequence, that the other parts of the building, being similarly situated, would be subjected to the same malarious effluvia, and hence its inmates would be affected with the same kind of disease; but this has never occurred in any of the large hospitals already alluded to, nor in any other, so far as we are aware, where patients affected with typhus are kept exclusively in one place. Again, it may be asserted by the non-contagionist, when driven to the last extremity, that though malarious effluvium be not generated in an hospital, it may be carried there by the clothes of the patients, and the attendants may be infected by coming into contact with them. The analogy of malarious diseases is in opposition to this belief; for it is not found that a patient labouring under ague infects any person who has not been in the malarious district; neither, according to the general belief, does a patient labouring under yellow fever, when removed from the quarter where he caught the disease, excite contagion in the vicinity of his new residence. But as this supposed typhoid malaria may be assumed to possess something *sui generis*, an argument stronger than analogy can be adduced, viz., the impossibility of carrying any principle of that kind into the wards. It is the practice, in many of the large fever hospitals, to remove the clothes of the patients, to bathe them, shave their heads, and give them clean linen, before they are sent into the wards. This plan is adopted in the Glasgow Fever Hospital, and the following is one of the regulations of the Waterford Hospital.

Dr. Bracken states, that "according to one of the regulations of the hospital, every patient has his hair closely cut at the time of his admission; he is also well washed with warm water and soap, and supplied with linen before he enters the sick ward."*

Sect. II. On the Analogy of Typhus to Exanthematous Fevers.

Having discussed the most important and specious hypotheses which have been brought forward to explain the general and ultra-proportionate prevalence of fever among the attendants of the sick in large hospitals, independent of the operation of contagion, we shall take notice of the general objections to the doctrine of contagion in typhus fever; and these may be comprehended in the statement, that it is not characterized by the laws of other contagious fevers. Before entering upon this part of the subject it may be remarked, that more importance is generally attached to this argument than it merits; for though, in the absence of facts, analogy is the most conclusive process of reasoning that can be employed, yet, undoubtedly, when facts are opposed to the application of this principle in any individual case, the facts have the preponderance over the analogy. And, though we were unable to prove that typhus was analogous, in its leading characteristics, to the contagious exanthemata, yet if it be admitted that there is no theory which can explain satisfactorily the facts regarding the prevalence of the disease in

* Barker and Cheyne on Fever, vol. i. p. 259.

hospital attendants, except that of contagion, the case would be conclusively determined, even in opposition to the analogy, and would be set down either as an exception, or as one of a new series of contagious diseases. It fortunately happens, however, that the law of analogy will be little violated by comparing typhus with the contagious fevers, for in its leading characteristics it resembles them pretty closely; at the same time it ought to be observed, that typhus fever has only, of late years, been examined with sufficient care, as to many points, connected with its history, laws, and pathology, and that it labours under the disadvantage of being frequently confounded with other continued fevers, to which, in its early features, it bears an intimate resemblance; so that the same certainty of analogical conclusion cannot be expected, as exists among the other exanthematous fevers. We shall endeavour, however, to show, by the facts which shall be quoted, that typhus comes distinctly within the range of their analogy, and that though it is not so regular in its progress, nor so certain in its eruption as smallpox or measles, yet that it differs as little from scarlet fever, in these respects, as the latter differs from smallpox.

The principal laws of the contagious exanthemata are the following:

1. The contagion can be traced in families, hospitals, schools, &c., and those exposed to it are very enerally infected.

2. They only affect persons once during their lives.

3. They are characterized by an eruption, which has a rise, progress, and decline, and the disease cannot be checked in limine.

1. *The contagion of typhus is traceable in hospitals, schools, families, &c.* In determining the contagious nature of any disease, it is not necessary that we should be able to trace every case, or even the majority of a particular amount of cases, to a communication with an infected person, or to exposure to a particular fomites; for this would imply that we could, like an American Indian, discover the trail of a patient and trace him through all the windings of a large city, and, besides, should investigate the history of every individual whom he has passed or rubbed shoulders with in every narrow and dirty alley. Even in smallpox, measles, and scarlet fever, any attempt of this kind to trace the contagion regularly would be fruitless, and for the very same reason. Smallpox was, at one period, believed by some authors to originate in filth, because it was found impossible, in numerous cases, to account for its existence in certain localities upon the principles of contagion. Dr. Adams remarks, that "many children born in London live for several years without receiving the smallpox. In the same neighbourhood a person arrives from the country, and without any apparent intercourse with an infected person is attacked by the disease."*

Independent of the many exposures to infection, which are perfectly unknown and undiscoverable by the patient, it is very difficult to ascertain the facts connected with the ordinary movements of a patient, which, in many cases, can only be elicited by tedious cross-examinations; so that this method of determining the point is liable to many objections, and greatly inferior in conclusiveness to the evidence derived from the

* Adams on Morbid Poisons.

spread of a disease in any large school or hospital; but, certainly, it tends to prove the doctrine of contagion in typhus as much, if not more, than it does in smallpox, &c., as will appear from the following table. The whole of the eruptive cases of typhus, in which this point was investigated, and that were admitted into the Glasgow Fever Hospital from 1st May to 1st November, 1839, are included only in this table, the males and females being classed together.

	Exposed to Contagion.	Uncertain.	Cold.	Total No. of Cases.
Eruptive typhus . . .	201	169	53	423
Febricula	10	28	22	60
Smallpox	7	19	1	27
Scarlet fever	...	4	...	4
Measles	...	2	1	3

The number of eruptive cases of typhus admitted into the fever hospital, both in the period included in the above table and also during the previous six months, who have been exposed to contagion, we have always found greater than in those affections not characterized by the exanthema; and it is remarkable that, notwithstanding the most careful enquiries, only 7 cases of smallpox could be traced to contagion out of 27.

Dr. Cowan states, that " of the patients admitted into the fever hospital last year, 472 males and 589 females, forming 47 per cent. of the whole, either ascribed the origin of their disease to contagion, or had been exposed to its influence."*

The propagation of the typhoid contagion is also intimately connected with filth and deficient ventilation; and there are few medical facts better ascertained than the close connexion of pestilence with these circumstances. Dr. Hancock remarks, that " the connexion of plague with filth and impure air, and crowded ill-constructed cities, and with certain seasons and climates and states of the atmosphere, calculated to engender mischief, though not accurately defined, has been so repeatedly observed in different countries as to stand on a far more solid foundation."† Dr. Bateman, in his Historical Survey of the Diseases of London, states that " Dr. Heberden has collected the most ample and satisfactory evidence of the connexion of the plague, and of the malignant contagious fever which generally precedes and accompanies it (if, indeed, they be not modifications of the same disease) with the filth of crowded, ill-ventilated large cities, in all ages and countries." He then quotes Dr. Heberden's remarks: " It has always originated and maintained its head-quarters in the filthiest parts of those cities; as in St. Giles's, in London, in 1665, and in Whitechapel in 1626 and 1636; and in those cities of Europe, which, from natural or political causes, have been backward in adopting the improvements of modern times: the picture of former manners is not exhibited in more lively colours than tha

* Cowan's Vital Statistics of Glasgow, p. 26.
† Hancock on Pestilence, p. 224.

diseases. The plague visited Denmark in 1764, it raged at ı 1771, and at Cracow still later. The last-mentioned town, ːall says, was not wholly paved till within the last two years, ng can be so execrable as the present paving, which scarcely he name. There is not a single lamp in the place; no precau- ısed to cleanse the streets, which, of course, become infectious r and almost impassable in winter." The following is Erasmus's n of the habits of the English, about two centuries ago: "The commonly of clay, strewed with rushes, which are occasionally but underneath lies unmolested an ancient collection of beer, ıgments of fish, spittle, the excrement of dogs and cats, and ; that is nasty."* Dr. Hancock observes, that most writers on : have remarked the exemption of Persia from this disease, and the following passage from the City Remembrancer: "The though their country is every year surrounded by the plague, ıffer anything by it themselves; they are the most cleanly the world, many of them making it a great part of their reli- move filthiness and nuisances of every kind from all places r cities and dwellings."† Drs. Barker and Cheyne, in their of the circumstances which either preceded or attended the fever in Ireland during the years 1816 and 1817, make the fol- ɒarks, which may be assumed as conclusions drawn from the physicians practising in the various provinces, and from the n of more than 100,000 cases in general hospitals: "When nenced in a poor family, or was introduced by a stranger or generally extended to all its members. The poor were the rers, in consequence of their neglect of cleanliness, particularly ct to their clothing, and the smallness and crowded state of tments; evils, at the time, much increased by the extreme hich weighed them down. On the other hand the superior hose circumstances were different, their clothing more fre- hanged, their persons more cleanly, their apartments less nd better ventilated, and among whom seclusion from the sick sed, in proportion to their enjoyment of these advantages, escaped the disease."‡

ːeman, after describing the methods to be adopted for pro- eanliness and sufficient ventilation, remarks: "If these sim- res be steadily pursued, no confinement or accumulation of ßuvia can take place under any state of fever; and the air rtment may be breathed, and the bed and person of the pa- ɔached and touched with perfect impunity. If this were not indeed, physicians and nurses, especially those employed in itals, would have little security for their lives. During the ears, in the course of which I have almost daily been in con- persons labouring under contagious fever, not only myself but rses have thus been preserved from infection, with one excep-

* Bateman on the Diseases of London, p. 18.
† Hancock on Pestilence, p. 287.
‡ Barker and Cheyne on Fever, vol. i. p. 134.

tion, down to the period of the present epidemic." He adds in a note: "It is no disparagement to the system above described that some of the nurses and the matron of the House of Recovery have been infected during the present epidemic, which has kept the wards constantly full. The impossibility of maintaining a free ventilation night and day, during the cold weather, their perpetual exposure, in close contact, to the breath and discharges of the patients, while feeding, moving, or washing them, changing their beds and linen, and even stripping off their infected clothes on admission, might be sufficient to counteract the solitary operation of any general system, however efficacious. But the truth is, that the ventilation of the house has been very imperfect, and even at the command of the nurses and patients; and the injurious consequences of this imperfection have become so manifest, that the subject is now under the consideration of the committee, while this work is in the press."*

Dr. Hancock quotes the following facts, which illustrate very powerfully the influence of ventilation: "In the year 1819, I had occasion to see a very intelligent physician connected with one or two fever hospitals in Dublin, during the epidemic, who assured me he had seen no proof of the existence of contagion in the disease (typhus) as it appeared in those institutions under his care, where very great attention was paid to ventilation, and where the patients were not inconveniently crowded. But soon after this, I saw another physician no less intelligent, who informed me that in the course of about four months, between 200 and and 300 persons were admitted into the Belfast Fever Hospital; and they were frequently so crowded in the wards as nearly to cover the floor with their beds; in which case, although the building is new, airy, and well regulated, the matron, twenty-two nurses, and the apothecary, took the disease; yet it was so mild, that scarcely more than one in fifty died."†

Dr. Prichard relates a striking example of the effects of a good as well as of a deficient ventilation, which occurred in two of the hospitals in Bristol, namely, St. Peter's and the Bristol Infirmary; both of these institutions being under his medical superintendence. "In the former, (St. Peter's,) the medical wards are very small, and it was necessary to place the beds very near to each other, and to put too great a number of patients in a given space. Offensive smells were often perceptible; and it was under these circumstances that the disease was manifestly contagious." In the Bristol Infirmary the wards are lofty and well ventilated. "Here, also, the fever patients were dispersed among invalids of almost every other description. But no instance occurred of the propagation of fever; none of the nurses were attacked, nor were the patients lying in the adjacent beds in any instance infected, though cases of the worst description of typhus gravior were placed promiscuously among the other patients, scarcely two foot of space intervening between the beds."‡

* Bateman on Contagious Fever, p. 154.
† Hancock on Pestilence, p. 339.
‡ Hancock on Pestilence. Prichard's History of the Fever in Bristol, p. 88.

Drs. Barker and Cheyne state that "a remarkable proof was afforded in Sir Patrick Dunn's Hospital of a ward, by the peculiarity of its construction, protecting the attendants upon the sick from the effects of contagion. The ward alluded to is the fever ward for males, which extends the entire breadth of the left wing of the hospital, being sixty-two feet by thirty-eight. It is twenty feet high, and is subdivided by partitions, of the height of nine feet, into six apartments, two of which are thirty-eight feet by sixteen, and the rest are each nine feet square; the latter contains, with great convenience, four beds each, and the former ten; but on occasions of necessity, the square apartments have held five and the oblong twelve beds without inconvenience; the partition walls leave two passages, one leading from the door of the wards across its breadth and another passing in the middle of its length; it is furnished with three large fireplaces, two of which are in the oblong chambers, one on the north and the other in the south side of the ward, and the third opposite the door, at the end of the passage first described; by this door, the fever-ward opens on the staircase which is walled and communicates with the corridors of the basement and underground stories. The greater number of the windows of the ward are sixteen feet from the floor, and in the ceiling are placed two louvres, one toward either end, by means of which and the fire-places a brisk ventilation is kept up. During the late epidemic, when Sir Patrick Dunn's Hospital, by agreement with government, contained 100 patients in fever, the male ward was crowded, containing forty-four patients, yet only one nurse was affected with fever; at the same period, the nurses in attendance on the female patients, who were certainly not so much crowded together, were continually taking the complaint and generally had it with severity."* In addition to the facts which have now been brought forward, it may be stated, without much chance of contradiction, that as there is in almost every large town deficient accommodation for fever patients in an hospital during an epidemic, that over-crowding is an ordinary result, from the anxiety of the directors to relieve the misery of the sick. The Glasgow Fever Hospital is calculated to contain 220 patients; and for nearly two years, namely, during 1836-7, it was generally filled to its maximum, and frequently from ten to twenty additional were accommodated. Now, it is quite obvious, that such a large number of fever patients, all contained in one building, will exhale a prodigious quantity of typhoid effluvium, which must be exceedingly concentrated; and that even the utmost cleanliness and the greatest degree of ventilation consistent with the temperature that ought to be maintained, would scarcely be sufficient for its proper dilution.

Drs. Barker and Cheyne remark, in that portion of their report which has been already quoted, that typhus generally spreads in the families of the lower classes and very rarely in those of the superior ranks. Dr. Cowan states that "the fever was chiefly, nay almost wholly, confined to the labouring classes and to the districts which they inhabited, while among the wealthy and middle classes of society it was comparatively seldom met with, and when it did occur, was not spread by contagion

* Barker and Cheyne on Fever, vol. i. p. 488.

through all the inmates of the family, as was usually the case am(the families of the poor, but was confined to a single individual These results, as stated by the above-mentioned authors, agree, are convinced, with those which have been made in almost every ot place. This remarkable difference, in the two classes of persons refer to, must be owing chiefly to the wide diversity of circumstances in wh they are placed; and approximates very closely to the difference wh exists between a crowded and consequently an ill-ventilated hospi and one which is limited to a small number of patients with thoro ventilation. The lower classes in large cities generally live in dirty ventilated houses, and are often filthy in their persons; while the bet ranks live in more airy situations, have larger houses, and are more tentive to cleanliness in their persons and domestic habits; hence effluvium which issues from a typhus patient, in the first-mentio situations, cannot be carried off so readily, or diluted to the same ext with atmospheric air as in the second. But it may be said by the op nents of typhoid contagion, that smallpox, measles, and scarlet fe more frequently spread in the families of the better ranks than typhı and why is ventilation and dilution not effectual in these cases? answer to this objection, it may be stated, that these three last-mentio diseases are not equally contagious, and that scarlet fever, particula when it is not epidemic, is often confined to one person in a famil whereas smallpox, in the majority of instances, affects the greater nu ber of unprotected persons, adults as well as children. M. Rayer sta that "scarlatina is contagious, but to a less degree than measles. It fects chiefly children and young persons, more rarely adults. Every dividual is not, to the same degree, apt to be affected with scarlati and every condition is not equally proper for its development. It atta females more readily than males; and some individuals, after havi been exposed, in vain, during many days to the contagion of this disea have been seized after the lapse of some time, in consequence of a sim communication with persons who had visited patients affected with t exanthema."†

Dr. Bateman states that adults are not very susceptible of the dise (scarlet fever), and that many medical practitioners who have attenc great numbers of patients affected with it have never experienced effects ‡

Dr. Mason Good observes that "nothing is more common than fo sporadic case of rosalia (scarlatina) to occur in a family without comn nication of itself to the surrounding children, although no pains may ha been taken to keep them separate; while a few months afterwards may possibly be received from a neighbour's house, merely by an ac dental visit for a few minutes. In the one case, there was no predis sition in the habit to receive the complaint; in the other, the alte state of the atmosphere has, perhaps, produced such a predisposition a very high degree, and prepared the way for the disease to become

* Cowan's Vital Statistics of Glasgow, p. 34.

† Rayer des Maladies de la Peau, tom. i. p. 63.

‡ Bateman on Cutaneous Diseases, p. 70.

very general epidemic. What this peculiar state of the atmosphere is has not been very accurately ascertained."* Now, although it be granted that cleanliness and ventilation have somewhat less effect in preventing the spread of smallpox, measles, and scarlet fever, than in checking the progress of typhus; it has been shown by the above quotations that these diseases differ from one another in points equally material. It follows, therefore, that scarlet fever is regulated by a law similar to that of typhus, in being little contagious under certain circumstances; and that though cleanliness and ventilation may not prove an antidote equally efficacious to the contagion of smallpox, measles, and scarlet fever as to that of typhus; yet to exclude the latter from the class of contagious fevers from this circumstance, would involve also the exclusion of scarlatina for an equally strong reason. In reasoning upon this subject, it does not seem difficult to conceive that one species of effluvium may be harmless, if diluted with a certain proportion of atmospheric air, while another may retain its virulency under similar circumstances; or that one species of effluvium may adhere with tenacity to every kind of clothing, while another is absorbed most readily by filthy garments, or by the deposits which are formed on the skin of an uncleanly person. We are in possession of no experiments which tend to prove such an opinion; but there is one analogy which will occur to every medical practitioner in vaccination. It is well known that if too much blood be drawn during the process of vaccination, the effect is very frequently prevented; and this is always explained on the principle, that the vaccine virus is diluted too much with the blood, as the same result follows when it is mixed with water. We have many analogies among the gases, such as carbonic acid, carburetted hydrogen, &c., to prove not only that when diluted to a certain extent with atmospheric air they may be respired with safety, but that each gas has its own peculiar law respecting the requisite proportion of dilution that is required for that purpose.

The majority of French physicians are of opinion that typhoid fever is not contagious, and this belief was almost universal until M. Bretonneau published a contrary opinion. The following quotation from M. Chomel will perhaps account, to a certain extent, for the opinions of the French physicians on this subject: "Another circumstance contributes with us to render the transmission of a contagious malady difficult, particularly the typhoid disease, that in our hospitals everything connected with cleanliness and ventilation is in the most perfect condition, and that the typhoid patients are never united, either in the same establishment or in the same ward, while their number is always very small, when compared with the number of those affected with other diseases; so that none of the conditions are present which favour contagion. It is the same with smallpox, and no one disputes its contagious character. In the wards of our hospitals there are persons frequently affected with smallpox, and there are often individuals who have not been vaccinated, or who not having undergone variola are susceptible of contracting the disease; yet few instances of its transmission are evident. It is also very rare that the transmission of measles or scarlet fever from one subject to another, in

* Good's Study of Medicine by Cooper, vol. iii. p. 19.

the *Hôpital des Enfans de Paris*, can be verified, which even presents i some circumstances the most favorable conditions for the transmissio of these diseases."* At page 98 some tables are given which tend t show the connexion of filthy habits with typhus contagion.

2. *Typhus generally attacks individuals only once during their lives* The second law of contagious fevers is that they only affect persons onc during their lives.

We believe this law to be completely established, and though there are instances of smallpox, measles, and scarlet fever affecting individuals more than once in their lives, yet these may fairly be considered only as exceptions to the general rule.

Before bringing forward the facts upon which the claims of typhus to be comprehended under this law may be founded, it is necessary to state that the evidence is by no means so clear and satisfactory as it is in smallpox, measles, and scarlet fever. These three last-mentioned diseases cannot, in the present day, be readily confounded with any other; for their diagnostic marks are very precise and definite; while the several kinds of continued fever have hitherto not been accurately ascertained, and have sometimes been considered merely as varieties of typhus; hence the difficulty of establishing the application of this law to any one of them. We hope, however, it will appear from the quotations which shall presently be adduced, that the approximation of typhus to this law is so near as to preclude, in all fairness, its exclusion. M. Chomel, after remarking upon the number of persons that are seized more than once with pneumonia, states that "In the typhoid fever, on the contrary, notwithstanding the care with which the patients had always been interrogated on this point, no one, among 130 persons who had been received at the *Clinique* affected with the disease, gave such a statement as could lead to the presumption that he had ever before laboured under it; on the contrary, most of them asserted that it was the first time they had been ill."† He elsewhere adds: "We have already stated that the typhoid fever, in ordinary circumstances, affects the same individual only once. This is the result of all the facts hitherto collected. Since we began to make special researches and conclusions respecting this disease, no authentic example to the contrary has been observed, although the number of cases which are observed be very considerable, and examples of its return ought to be met with, if the malady were susceptible of being reproduced many times in the same person."

In interrogating our patients, we have always taken care to turn their attention from this quarter, but they have never answered in such a manner as to induce us to believe that they had laboured under the same disease; and after all, though some contrary facts should be met with in so frequent a malady, these exceptions which are little numerous are nothing extraordinary, and do not overthrow the species of law which has been announced. Smallpox, scarlet fever, measles, which most generally attack the same individual only once, sometimes return, especially during epidemics of these diseases; it will not be more astonishing i

* Chomel, Leçons de Clinique Médicale, tom. i. p. 321.
† Ibid., p. 309.

examples of a return of the typhoid affection be met with. This nstance is, then, a very important fact, for there are only a small er of diseases that attack the same individual only once, and gst these maladies, there is none which is not evidently contagious; phoid fever will, then, be the only exception to this kind of law if ot contagious like the diseases with which it has this important of agreement. In the mean time we ought to observe that, though diseases which attack the same person only once are contagious, not follow that all those that are transmitted from one individual ther attack only once, many among them, as syphilis and the itch, produced indefinitely."*

Lombard, of Geneva, when describing the difference between the ental and British typhus, states that "in one remarkable point, er, I believe they agree, I mean the fact that no one is known, or t is very rarely known, to have the eruptive typhus twice. With h instances are scarcely if ever met with, and I am informed that ou a person once attacked with typhus, attended with the measles uption, may safely calculate upon immunity from the disease for ture."†

Perry, of Glasgow, states as one of his conclusions respecting ty- ever, that "contagious typhus is an exanthematous disease, and, mallpox, measles, and scarlet fever, during its course produces change on the system, by which the individual having once under- the disease is (as a general rule) secured against a second attack, ay with impunity expose himself to the contagion of typhus, if he ues to reside in the same country in which he previously had the e. In those cases which are exceptions to the general rule, the e appears in a mild and modified form, the crisis taking place on venth, ninth, or eleventh day." The same author states that this sion as well as the others in his paper are "the result of careful ation in upwards of 4000 cases."‡ Drs. Barker and Cheyne, who e most extensive opportunities of ascertaining the history of ty- seem to entertain opinions similar to those already quoted. They that "at the hospital in Cork street, only one physician and the cary had an attack of fever; but then most of the physicians of tablishment had laboured under that disease on some former occa- revious to the appearance of the epidemic."§ Dr. Cowan, as y quoted, states that all the gentlemen who have acted as clerks Fever Hospital for many years past have been attacked with fever they had it previously to their election.

lenbrand's opinion on this subject is of a more modified kind. He that "the miasma of typhus, after having produced the fever, ys almost always for a certain time the susceptibility to a similar gion; nevertheless, it destroys it rarely for the whole of life, as smallpox, measles, &c. It has, however, under this resemblance analogy with the virus of these diseases, whilst on the contrary it

* Chomel, Leçons de Clinique Médicale, tom. i. p. 333.

† Dublin Journal of Medical Science, vol. x. p. 22.

‡ Edinb. Med. and Surgical Journal, vol. xlv. p. 67.

§ Barker and Cheyne on Fever, vol. i. p. 135.

totally differs from the syphilitic virus, which when once introduced i the human body, always favours more and more a similar contagion.

The following table shows the answers to questions which were ca fully put to patients who were admitted into the Glasgow Fever Hospi from November 1st, 1838, to November 1st, 1839. It includes whole of the patients affected with eruptive typhus, from whom answ were obtained relative to any former affection with fever, as evide from decided cases only could be made available in the elucidation this point:

	Males.	Females.	Total.
Not previously affected ...	284	251	535
Previously affected	33	41	74
			609

This table shows that out of 609 eruptive or decided cases of typh there were only 74 persons who stated that they had previously labour under fever. This part of the evidence may be reckoned positive; individuals of all intellectual capacities remember a remarkable circu stance of this kind. On the other hand, the evidence respecting t nature of the former fever or affection is the converse of this; for only a very few cases can it be correctly ascertained; and when we take i account the various diseases which are confounded with typhus (as sh be afterwards shown), such as bronchitis, pneumonia, pleurisy, intesti affections, febriculous or short fevers, and the numerous ailments childhood, this small number can be satisfactorily accounted for.

It appears, therefore, that the evidence which can be produced to b on this point, although not very extensive, decidedly supports the opini that eruptive typhus fever affects individuals as a general rule only o in their lives; and it is to a considerable extent corroborative of t opinion, that almost all the clerks and nurses of the Glasgow Fe Hospital for the last six or seven years have had typhus characterized the eruption, and not one of them, as far as we have been able to lea have ever had it since; while almost all of them consider themselves n perfectly secure against a second attack, although constantly exposed the effluvia arising from fever patients.

3. *Typhus is characterized by an exanthematous eruption.* The th characteristic of the contagious exanthemata is an eruption which ha regular rise, progress, and decline. The exanthematous eruption or r which is peculiar to typhus fever has only been accurately attended within these few years as a diagnostic symptom. It was, however, ticed by Rogers, in the fever which prevailed in Ireland during the y 1731, and one of the characteristic symptoms is described as "a u versal efflorescence of petechiæ;"† also by Huxham in 1734-5, John Pringle in 1750, &c.

No particular conclusion can be drawn from these authors' accoun it; but when taken along with their general description of the dise

* Hildenbrand, de Typhus contagieux, par J. C. Gasc, p. 118.
† Barker and Cheyne on Fever, vol. i. p. 4.

pinion is corroborated that it was the very same affection that is so cterized at the present day. Hildenbrand described it in 1806 more ularly than any previous author. He states that it makes its ap- ice about the fourth day of the disease on the breast, loins, back, , and arms, as being more warm, but sometimes on the face; that it nuch more abundant as the eyes are red. He also remarks that iæ may exist with or without the eruption, are not indispensable nena, and are only developed in certain conditions. He farther es that the *exantheme* is sometimes not present in those cases of which are irregular in their progress.* The typhoid eruption was very general characteristic of the epidemic fever which prevailed and during the years 1817-18-19. Dr. Bracken makes the fol- statement: "Of about 250 cases which fell under my care in iber and December of that year, the majority had eruptions of of various appearance as to size, shape, and colour. They were lly of a diffused appearance, gradually shading off, and insensibly earing, and of the size of a grain of hemp-seed, but sometimes larger or much smaller. The distinct, well-defined petechiæ were ntly seen of a bright brown or purple colour. The shoulders l to be more frequently affected by these eruptions, but the whole : of the body was often covered by them."† Drs. Barker and e give the following account of the eruption, as deduced from received from several parts of Munster: "As the disease ad- it was observed in most or all parts of the province that erup- f different kinds, either closely allied to or varieties of those termed ial, very generally accompanied it. In some instances the erup- is papular, or a motley appearance of the skin, or a rash somewhat ling measles showed itself. At Cork, Dr. M. Barry remarked the species of fever which he terms synochus, petechiæ seldom ed earlier than the fourth or fifth day; but his observation, if it ot express it directly, at least implies that their occurrence was nt. They were generally of a bright red colour, sometimes small, r times large. He did not consider them dangerous, nor find it ary to abstain from those measures of depletion which were useful high excitement prevailed. In a communication from Clonmell, tzgerald states that petechiæ occurred in four cases out of five. owel, petechiæ was so common that Dr. O'Connell did not see es of fever unattended by a petechial eruption, which often ap- early in the disease." In the account of Connaught, the same s state that "an early eruption of petechiæ, which were often to be ed on the third or fourth day or even earlier, and were visible for five days, was a general symptom of the disease; when petechiæ ed thus early, they were not indicative of any malignancy." In port for Ulster, it is stated that petechial eruptions were very com- and that they occurred early. For Leinster, the same reporters hat one physician observed the petechiæ in seven cases out of ten, hought them more general than they had been on any former occa- and others represented them as universal. They appeared on the

* Hildenbrand, de Typhus contagieux, par J. C. Gasc, p. 53-4.
† Barker and Cheyne on Fever, vol. ii. p. 231.

third, fourth, or fifth days, continued visible for four or five days, and were often remarked in the mildest cases.*

The typhoid eruption, however, excited very little attention among the authors who wrote upon the epidemic that prevailed in Britain about the same period as in Ireland; and even up to a much later period it is only noticed in a cursory manner in our treatises on fever, and not as a diagnostic mark of great value. Dr. Alison in 1827 described it as a very frequent symptom of the epidemic which prevailed in Edinburgh about that period, occurring in a majority of the cases, and remarked that these eruptive fevers formed the connecting link between continued fever and the contagious exanthemata.†

M. Louis, who published his admirable work on gastro-enteritis or typhoid fever in 1829, states that "he has observed this eruption in twenty-six out of thirty-five cases, where it has been searched after, without saying that it was not present in some others; many of the persons in whom it was present had come to the hospital after the twenty-fourth day of the disease, at a period when the spots had perhaps disappeared."‡ M. Chomel gives the following excellent description of the typhoid eruption: "Usually from the seventh to the ninth day the eruption peculiar to typhoid fever makes its appearance, which consists in minute rose-red spots, disappearing on pressure from half a line to two lines in diameter, of a circular form, without elevation or scarcely raised above the level of the skin, dispersed over the abdomen, sometimes on the chest, less frequently ou the thighs, the arms, and forearms. These little spots are so much the more distinct as the skin is white; in persons who have brown skins they are sometimes distinguished with difficulty. Their number cannot be determined because they are not all equally apparent; but in order to furnish a characteristic of the typhoid affection they ought at least to be from fifteen to twenty. When there are only two or three, no value can be attached to their presence. The eruption does not make its appearance on all points at once; often, after having noticed for three or four days some rose-coloured spots upon the abdomen, but in too small number to be considered as important, they are found all at once very numerous upon the chest and belly, sometimes upon the thighs, the arms, the back, and even the face, though very rarely. Its duration is not always the same; in some cases, after two or three days, there is no vestige of it; at other times it persists during twelve or fifteen; but in the latter case it consists of many successive eruptions; for each rose-coloured spot is usually visible for three or four days only and sometimes less; and at the end of this time it disappears altogether, after having attained a colour less vivid. These spots present, at most, a slight elevation on the surface of the skin, but they never have a conical form or vesicles at their apex. They rarely appear before the eighth day after the invasion of the disease. The following are the results of observations collected in our wards during the years 1830-1-2. Among seventy cases of typhoid fever, where the presence or absence of rose-coloured lenticular spots was carefully established, in sixteen cases, at no period of the

* Barker and Cheyne on Fever, vol. i. pp. 426, 454, 465, and 483.

† Edinb. Med. and Surgical Journal, vol. xxviii.

‡ Louis, de Gastro-Enterite, tom. ii. p. 231.

disease could traces of this eruption be found; from which it may be inferred that in about one fourth of the persons seized with the typhoid affection this eruption is wanting."*

Chomel found that among fifty-four cases none presented the eruption before the sixth day, and in two it appeared after the thirty-sixth. This, he states, is confirmed by the observations of M. Louis, which were made on a much larger number of patients. He attaches great value to the eruption, as a diagnostic mark of typhoid fever, as it is as rare in other acute diseases as it is common in this.

Dr. Roupell states that in St. Bartholomew's Hospital, London, the eruption in typhus occurs in seventy out of every 100 cases.† Dr. West, in his account of the typhus exanthematicus as observed in St. Bartholomew's Hospital, states that "forty-two cases presented the peculiar measle-like eruption described by so many authors, which in all those cases in which I have been able accurately to note the date of its appearance, first showed itself from the sixth to the eighth day, generally on the former. It appeared in one instance on the fourth and another time on the fifth day; but I never saw it make its first appearance after the eighth day, though it was still visible on several patients admitted on the fourteenth, and on three who came to the hospital on the twenty-first day of the affection. Of the eighteen cases in which no eruption was observed, five only were admitted before the eighth day of the disease; it is, therefore, very probable that the eruption had existed in some of these patients but had disappeared before their admission."‡ Dr. Cowan has investigated the frequency of the eruption in the Glasgow Fever Hospital on upwards of 2000 cases, during the year 1835-6; and his results are the following: "At the close of the year, in 76·16 per cent. of the males, and 71·77 of the females, the typhoid eruption had occurred, giving as an average of the whole cases 73·99 out of every 100 admitted."§

Dr. Craigie found the typhoid eruption only in seventy-nine among 169 cases in the Edinburgh Royal Infirmary;‖ while Dr. Henderson discovered it in 108 cases out of 130 in the same insitution at a subsequent period.¶

In the Glasgow Fever Hospital, from May 1st to Nov. 1st, 1839, during which time the presence or absence of eruption was carefully noted, the proportion was as follows:

	Males.	Females.	Total.
Cases with Eruption	224	217	441
Cases without Eruption or doubtful	130	120	250
			691

This table includes every case, with the exception of smallpox, measles, hooping-cough, and scarlet fever.

* Chomel, Leçons de Clinique Médicale, tom. i. p. 18.
† Roupell on Typhus, p. 35, 1838.
‡ Edinburgh Medical and Surgical Journal, July, 1838, p. 140.
§ Cowan's Vital Statistics of Glasgow, p. 26.
‖ Edinburgh Medical and Surgical Journal, vol. xxvii. p. 301.
¶ Ibid. October, 1839, p. 437.

Dr. Peebles, in 1835, gave a very minute and excellent account of exanthematous typhus, and states, as the result of a minute enquiry into the subject in Great Britain and on the continent, that "he has found the eruption as constant as any exanthema of other eruptive diseases."*

It appears, therefore, that the eruption is present generally in from 70 to 75 out of every 100 patients that are admitted into fever hospitals in this country as well as in France. It must be well known, however, to every hospital physician, that cases are frequently admitted as continued fever, that are found, on examination, to be other diseases, and which are usually included in the total enumeration; but this point shall be further illustrated in another part of the essay.

It is also well known that many cases of fever are admitted at a very late stage of the disease, as may be proved by statistical tables and also from the great number of deaths that occur on the first, second, and third days after admission; hence it is extremely probable that the eruption has disappeared in a certain proportion of those who have the other decided symptoms of typhus. There is one fact, however, which powerfully supports the opinion that contagious typhus, in the great majority of cases, particularly in adults, is attended with the eruption, namely, that almost all the instances of fever which have occurred during the last six or seven years among the physicians, clerks, nurses, &c. of the Glasgow Fever Hospital, have been accompanied with this exanthema. We have made careful enquiries respecting this point, and have only heard of one or two exceptions amongst at least 100 cases. We do not, however, mean to maintain that typhus fever cannot exist without the presence of this eruption; on the contrary, we have repeatedly attended families where the majority only of those affected were so characterized, and we have remarked this two or three times, when several of a family were sent into the hospital.

The want of regularity in the appearance of the eruption, and its persistency in regard to time, has been considered opposed in analogy to that of smallpox, measles, and scarlet fever; and certainly, though these three diseases are more regular in their characteristic eruption than typhus, yet in scarlatina anginosa the eruption is frequently irregular or altogether absent. Dr. Tweedie, who must have treated scarlet fever extensively in the London Fever Hospital, makes the following statement: "Indeed, we are inclined, from our own experience, to affirm that the scarlatina simplex, scarlatina anginosa, and the scarlatina or angina maligna, and the sore throat without efflorescence on the skin, are merely varieties of one and the same disease."† He also quotes the results of Dr. Willan's experience during an epidemic scarlatina in the year 1786. "Of 251 cases, there were 152 of scarlatina anginosa, 42 of sore throats without eruption on the skin, and 39 of scarlatina maligna."‡ Rayer states that "often it does not appear until the third day, and is not dispersed so constantly upon the whole surface of the body. ...... It is sometimes entirely effaced on the day of its appearance, and is developed anew at a period more or less near.........The appearance of

* Edinburgh Medical and Surgical Journal, vol. xliv. p. 373.

† Cyclopædia of Practical Medicine, vol. iii. p. 647. ‡ Ibid., p. 653.

xantheme is tardy; its tint is feeble and livid; it is interspersed with chiæ, and its duration is uncertain. It appears and disappears r times."* It appears, therefore, that scarlet fever often differs from pox very materially in the regularity of its eruption; for that of atter disease is extremely regular and almost unvarying in its rise ess, and decline; while in the former it is frequently absent, and ier cases so evanescent as not to be distinctly recognized. The ion characteristic of typhus again differs from that of scarlet fever, ng still less regular than it; but there is not a greater difference, re be not less, in this respect, between scarlet fever and typhus than is between scarlet fever and smallpox.

Typhus cannot be checked in limine. It has also been stated by rs, and it is a prevalent opinion among medical practitioners, that is can be checked in its early stages, and that in this respect its law ally different from the exanthematous fevers. If this opinion were ct, the analogy between these affections and typhus would be greatly ished, though not completely undermined; for it is not contrary to ience to suppose that some agent may be discovered that might be le of modifying or destroying the poisonous principle that is d in the body. Those, however, who believe in the possibility of ing typhus *in limine*, have assumed a false premise, at least one is not admitted, from which they draw their conclusions, namely, every febrile affection which resembles this disease in its early symp- is identical with it. Now it is well known to every medical prac- er that many of those febrile attacks which arise from disturbance digestive functions and from vicissitudes of temperature are at- d with the same symptoms as typhus in its early stages; and yet will subside in a few days under every variety of treatment, and fre- ly without any treatment at all, at least such as could produce any on the system. If typhus fever, which is frequently so prevalent, be checked in its progress by the means which are generally em- d for that purpose, namely, bleeding, purging, sweating, &c., this ine would long ere now have been established with the same cer- as that peritonitis or pneumonia can be checked by a similar sys- of treatment; and yet the disease proceeds onwards in its course, trained by the heroic, but occasionally injudicious attempts to it. If, then, there be febrile affections which subside in a few days every variety of treatment, and often without any possessing a cura- peration, it follows that those who make the assertion that they can typhus *in limine*, should prove that their cases did not belong to briculæ we have referred to; or what would amount to the same , make their experiments upon an unequivocal example of the se, namely, one characterized by the eruption, and demonstrate that can stop its career.

isis of typhus is pretty regular in cases not complicated. A second tion has been brought forward against the inclusion of typhus among thematous fevers, namely, that it has no regular crisis like these last- tioned diseases. This objection does not seem to have much weight

* Rayer, Traité de Maladies de la Peau, tom. i. pp. 59-60.

attached to it; and to give it any degree of importance, it would be necessary to prove that all the other fevers of this order are uniformly characterized by a crisis on a particular day. Now, what are the facts connected with the history of this point in scarlet fever. Dr. Bateman states that the rash in scarlatina anginosa does not always appear on the second day, as in scarlatina simplex, but not unfrequently on the third; nor does it so constantly extend over the whole surface, but comes out in scattered patches, which seldom fail to appear about the elbows. Sometimes, too, it vanishes the day after its appearance, and reappears partially at uncertain times, but without any corresponding changes in the general disorder; the whole duration of the complaint is thus lengthened and the desquamation is less regular..........The same author, after describing the dangers which result from hemorrhage, diarrhœa, &c., in malignant scarlatina, states that "even those who escape through these dangers have often to struggle against many distressing symptoms for a considerable length of time, such as ulcerations spreading from the throat to the contiguous parts, suppuration of the glands, tedious cough and dyspnœa, excoriations about the nates, &c., with hectic fever."* When treating of measles, Rayer remarks that "it is never the exantheme which compromises life. The gravity of the evil depends upon the internal inflammation which accompany or succeed it. —The appearance of the measles before the third day, the sudden disappearance or the leaden redness of the spots, the appearance of petechiæ, much difficulty of breathing are severe symptoms. They are often characteristics of bronchitis and pneumonia, the existence of which is easily ascertained by auscultation and percussion of the chest. ...... When the symptoms of gastro-pulmonary inflammations, which accompany the exantheme of measles, are little intense, and when it travels over its periods *easily and regularly*, the treatment of the disease is very simple."† It is obvious from these quotations, and it is well known to every experienced practitioner, that the crisis of measles and scarlet fever varies considerably in different individuals, being pretty regular and early in the simple cases, and more or less protracted and irregular in those that are complicated with any organic affection. In typhus fever, uncomplicated with any serious organic disease of the head, chest, or abdomen, the crisis occurs very frequently about the same period in persons of a similar age; for young persons, as a general rule, pass through the disease more quickly than those more advanced in life.

Chomel states that the crisis or amelioration of the symptoms in sixty-eight cases, occurred in fifty, or in about three out of four, from the fifteenth to the thirtieth day.‡ Dr. Arthur Thomson states that the average duration of 2630 cases was twenty-seven days; and this calculation was made from cases described and enumerated in the works and papers of Drs. Bateman, Welsh, S. Smith, Latham, and Craigie.§ There is certainly considerable irregularity as to the period when the crisis takes

* Bateman on Cutaneous Diseases, pp. 73 and 85.
† Rayer, Traité des Maladies de la Peau, p. 24.
‡ Chomel, Leçons de Clinique Médicale, vol. i. p. 44.
§ Edinburgh Medical and Surgical Journal, July, 1838, p. 109.

typhus in different individuals; at the same time, it may be re- that a majority of patients begin to ameliorate within a certain and the reports from the various authors already quoted in regard oint do not differ very materially as to the mean duration of iowing, even with imperfect statistics, a near approximation to by which it is regulated. It ought to be observed, however, evidence obtained from public hospitals is still very uncertain; s very careful and repeated enquiries be made to the patient, actory or accurate answer can be obtained, as to the period when ase commenced; for he is often partially incoherent, and in ll cases, more or less confused in his ideas; and even though an opportunity of questioning his friends, more or less of cross- tion is generally required to elicit a correct answer.

gree of uncertainty also arises from not calculating the crisis it the same period. Some, as Chomel, Mills, Stoker, &c., cal- the termination of the disease from the commencement of the scent stage; while others have included the whole period of the residence in the hospital in its duration. It is obvious that screpancy must arise from such a different method of calculation; tient is often a week and sometimes two weeks in an hospital period of convalescence commences. Another uncertainty on it has arisen from not classifying patients according to their dif- ges, such as is employed in calculating the mortality of typhus at rent periods of life; for if the disease be shorter in its duration, tainly is, in young persons than in those more advanced in life, possible to expect uniformity by arranging the whole together. l also contribute to elucidate this point, were the duration of the ed cases, namely, those characterized by the eruption, classified ly; as by this means, the duration of typhus would not be con- with that of other continued fevers, and this method might also available as one of the means of diagnosis.

ery frequent complications of typhus with organic affections in rent cavities of the body is another reason amply sufficient to for a considerable portion of its irregularity as to termination; hese complications occur more frequently in this disease than in x, measles, and scarlet fever, it follows that allowance should be r its greater irregularity on these accounts; as it has already been y quotations from authors that some of the exanthematous fevers rendered irregular and protracted by organic complications.

Arthur Thomson gives the following table of the complications of mpiled from cases related by Drs. Smith, Tweedie, Alison, and and it shows that the complicated varieties are much more nu- than the simple or uncomplicated.

Simple fever		374
Fever with cerebral complications . .		375
thoracic	do. . .	264
abdominal	do. . .	180
mixed	do. . .	308
		1501

5. ***Relapses in typhus do not occur after complete convalescence, unle some local disease be present.*** The occurrence of relapses in fever is al: brought forward as an important difference between it and the contagio exanthemata; and almost every author who has written on this subje mentions them as more or less frequent at different periods and durin different epidemics. They do not seem to occur frequently in the typhoi fever, for Chomel does not make any particular reference to this point; and he merely states that the convalescence is sometimes prolonged, and that troublesome results are sometimes the consequence of satisfying hunger.

In considering this subject it is necessary, however, to observe that relapses, according to most authors, do not mean a return of the fever after complete convalescence, but a return of the symptoms with their former intensity, after a partial recovery, or what in the majority of cases might more properly be called a remission of the disease; for it is perfectly obvious that if the febrile symptoms had only been meliorated, but not removed, that the disease was still present in the system.

That relapses do occur, in a small proportion of the continued fevers of this country, after complete convalescence, we have no manner of doubt; but certainly this occurrence is rare in typhus, without the coexistence of some organic lesion sufficient to account for the return of the febrile symptoms. A considerable number of what are generally called relapses, particularly during the prevalence of an epidemic, consist of cases which have been admitted with fevers not of a typhoid kind, such as febricula, bronchitis, and those arising from gastric or intestinal derangement; and have caught the contagion of typhus during their residence in the hospital. That this occurs, as a general rule, in the fever hospital of this country we are not in possession of facts to prove; but that it has very frequently occurred in the Glasgow Fever Hospital there can be no doubt; and during the late epidemic it was considered very dangerous to retain a patient long in the house after his convalescence from a febriculous affection.

The following table shows the number of relapses or secondary affections that occurred after complete convalescence among 686 cases, admitted into the Glasgow Fever Hospital from May 1st to November 1st, 1839, erysipelas being excepted:

(MALES.) SECONDARY AFFECTIONS.									
	Typhus.	Cynanche Tonsillaris.	Apoplexy.	Peritonitis and Int. Ulcers.	Œdema Glottidis.	Intestinal Fever.	Ulceration of Uvula.	Delirium Tremens.	Total.
Typhus	...	1	...	2	...	...	1	...	4
Febricula	2	...	...	...	...	...	...	1	3
Intestinal Fever ...	1	...	...	..	...	2	...	..	3
Pneumonia	...	...	1	...	1	...	...	...	2
									12

(Females.) Secondary Affections.

	Laryngitis.	Pneumonia & Int. Ulcers.	Synovitis.	Intestinal Fever.	Smallpox.	Roseola.	Pneumonia.	Total.
....	1	1	2	...	1	...	...	5
....								
ever	...	...	...	1	...	...	...	1
....								
....	..	...	...	...	...	1	...	1
....	...	...	...	...	...	...	1	1
								8

Total of Males and Females = 20.

.rs from these tables that among the cases of typhus there was e relapse into the same febrile state, characterized by a new ıd the other distinctive marks of this disease; but on the con- all the secondary affections were well marked local diseases. hown that two cases of febricula and one of intestinal fever ed with typhus during their residence in the hospital; and it : that more of such cases would have been infected had not tion been adopted of dismissing them as early as possible.

uding our remarks upon relapses, we shall make the following 'rom Drs. Barker and Cheyne's work, in order to show that most powerful facts in favour of the doctrine of relapses may :d by the theory we have adopted. These authors state that "as ic advanced and particularly in its latter stages, relapses became on, insomuch that a very large proportion of those who had ked suffered a relapse, and with many this happened several . . . It was remarked at Roscrea that the more early the rred the greater was the probability of relapse. This obser- apply to every part of this province, for as the epidemic fever l to a close, a fever of short duration, continuing for about extremely mild and rarely proving mortal, became very fre- at this time the tendency to relapse was most observable. On 'y, after fever of long continuance, it rarely happened that re- place."* . . . . The same authors in their medical ac- ever in Connaught, state that "relapses were so rare at the ment of the epidemic that Dr. Veitch, Physician to the County in Galway, in his letter of the 6th September, 1817, says that t observed one case of relapse out of some hundred cases of a describing the disease as it occurred among the upper ranks ', they state that "petechiæ were universal, insomuch that case occurred without them."†

erences which may be deduced from these quotations are, hese short or five-day fevers were either not typhus or their ıce was only a remission of the disease; for we are not aware ter on this subject who describes it as terminating so early.

arker and Cheyne on Fever, vol. i. p. 438. † Ibid., p. 455.

2d. Very few of those which were protracted, or which continued to the end of the second or third week, relapsed, which is about the average period for the duration of typhus. 3d. That in Galway, where petechiæ or the typhoid eruption were almost universal, showing the disease to be typhus, not a single case of relapse occurred out of some hundred cases.

CHAP. III. SOURCES OF CONTINUED FEVERS, NOT TYPHOID.

Pneumonia, pleuritis, peritonitis, bronchitis, and modifications of these affections are not unfrequently confounded with continued fever, being admitted to fever hospitals as such; and thus the numerical amount of non-eruptive cases of typhus is often considerably increased by the inclusion of these diseases in the list; independent altogether of the two other affections which we are about to describe, and which are generally considered continued fevers, although different from typhus in their prominent features and laws. The first and most prevalent of these two affections has been called febricula, on account of its mildness and short duration, when compared with typhus. The second is prominently accompanied with derangement of the digestive organs, either in the form of constipation or diarrhœa. Chomel makes the following observations, when treating of the diagnosis of typhoid fever: "In effect, various diseases may present, during the first three or four days, a great resemblance to the typhoid affection. Among the diverse morbid states which may at this period present analogous phenomena, we shall find the early symptoms of many eruptive diseases, as smallpox, scarlet fever, and measles; also some catarrhal affections of little intensity: protracted ephemeral fever may be taken for the typhoid inflammatory fever, bilious derangement for bilious fever, exhaustion for the commencement of an adynamic fever, and especially a latent phlegmasia either visceral or venous. . . . . One of the most important characters of the typhoid affection is the duration of the febrile state. As often as the febrile phenomena which can be attached to any appreciable lesion are prolonged beyond a certain limit, eight or ten days for example, there will be already serious grounds for presuming an alteration of the glands of Peyer; and when a disease terminates at the end of some days, we can always be assured, whatever doubts may have existed as to its nature, that it was different from the typhoid affection; and thus all the morbid states, the duration of which does not extend to the tenth or twelfth day, are distinguished."*

1. *Sources of Febricula.* This affection generally commences, like typhus and several other febrile affections, with a rigor, attended by headach, frequency of pulse, heat of skin, flushed face, thirst, moist tongue, generally more or less coated with a whitish fur, and red at point and edges, more or less constipation of bowels, and in the great majority of cases uncombined with any determinate local affection. It is difficult to distinguish this fever from typhus for the first four or five days; but after that the diagnosis may in most cases be made with tolerable accuracy.

If the typhoid eruption be present, there can be no doubt whatever of the nature of the disease; for in Britain this peculiar efflorescence occurs in no other febrile affection that could be confounded with typhus; but

* Chomel, Leçons de Clinique Médicale, tom. i. p. 400.

:ertain proportion of cases it is not present in the latter disease. In of typhus destitute of the eruption, there are frequently, however, symptoms present, even by the sixth day, which are rarely if at all red in febricula; such as suffused eyes, delirium, or partial stupor, ınd brown tongue, a dark or dusky hue of the skin. The frequency pulse is also a very important symptom in the diagnosis; for in ıla it is rarely above 100, and it generally continues full or of ate strength throughout the whole course of the disease; whereas ıy cases of typhus, the pulse becomes weak, soft, small, or very essible, at an early period of the disease, and in most cases is more above 100 about the sixth or seventh day. Sometimes this fever ates in one or two days, being described by some authors under ıme of ephemera; but more generally symptoms of amendment : about the sixth or seventh day, and complete convalescence is shed, in the large majority of cases, from the sixth to the tenth day. ess and desquamation of the cuticle, both of which are frequent teristics of typhus, are generally absent in this affection. Again, ete convalescence from typhus rarely occurs in adults before the th day, and is in the majority of cases much later. In children, er, the crisis of typhus generally appears earlier than in adults; e febriculous affections to which they are liable are proportionally often only one or two days in duration. The statistical facts con- l with the minimum and maximum duration of typhus have not ery conclusively determined; for, as we formerly remarked, one of authors terminate the disease when the stage of convalescence , while another class do not consider it terminated until the pa- s discharged from the hospital; and this discrepancy is still further sed by not carefully classifying the different febrile affections that mitted into fever institutions and their corresponding duration. Chomel, who seems to have been exceedingly careful in drawing nclusions only from decided cases of typhoid fever, gives the fol- ; statistical account of the duration of the disease, from its com- ment to the beginning of convalescence:

In 1	patient	on the	8th	day after attack.
1	„	„	9th	„
4	„	„	12th	„
3	„	between	12th and 14th	day inclusive.
10	„	„	15th and 16th	„
15	„	„	17th and 20th	„
14	„	„	21st and 25th	„
11	„	„	26th and 30th	„
8	„	„	31st and 40th	„
1	„		on the 45th.	

, however," he adds, " we throw aside the cases in which improve- nas appeared before the fifteenth day, and those in which it has red after the thirtieth, which constitute a small number of excep- there remains fifty cases out of sixty-eight, that is nearly three s, in which this improvement took place, from the fifteenth to the th day."* It appears from this table that there were only two out ty-eight cases that presented symptoms of convalescence at the

* Chomel, Leçons de Clinique Médicale, tom. i. p. 44.

eighth and ninth day, and if we add five or six days for its comp establishment, the disease, even in this fractional proportion of ca could not be considered as terminated before the thirteenth or f teenth day.

This method of calculating the duration of fever, adopted by Cho and many other authors, is greatly inferior in accuracy to that of marl the patient convalescent when he is actually free of the febrile sympto namely, when his pulse is natural, his tongue pretty clean, his s tolerably sound, and his appetite moderately good, but still weak consequently unable to leave the hospital for some days at least. I quite obvious that the positive character of these four symptoms ren them more fixed, more easily ascertained, and not so likely to be mis prehended as their relative improvement during the first stage of con lescence; and therefore that it is preferable in the determination of t question.

It appears necessary, before presenting our table constructed on t principle, to give one which will show the whole diseases that have b admitted within a certain period into the Glasgow Fever Hospit namely, from May 1st to November 1st, 1839; as some of our deducti depend upon a fair and impartial consideration of these cases, and as t various statistical points referred to in this section were noted with ca

		Males.	Females.	Total.
Continued Fevers.	Typhus	270	276	546
	Febricula	32	31	63
	Gastric or Intestinal Fever	8	7	15
	Bronchitis	14	8	22
	Pneumonia	15	7	22
	Smallpox	16	11	27
	Measles	3	1	4
	Scarlet Fever		4	4
	Hooping-cough	1	1	2
	Hydrocephalus	1		1
	Erysipelas		3	3
	Roseola		2	2
	Erythema	1		1
	Hepatitis	1		1
	Apoplexy	1	1	2
	Determination of blood to head	1		1
	Intermittent Fever	1		1
	Cynanche Tonsillaris	1	1	2
	Syphilis	1	1	2
	Delirium Tremens	1		1
	Suppuration of Kidneys		1	1
	Phthisis		2	2
	Dysentery		2	2
	Mania		1	1
		368	360	728

As a considerable number of the cases in the above table were continued fevers, it may be necessary to explain one or two po respecting the admissions into the Glasgow Fever Hospital. The fa lities of admission have of late been very great, in consequence of th being much more accommodation than was required; and every ca

ere there was the slightest suspicion of fever, seems to have been sent this institution, not only from the city, but from its vicinity, to the ex- t of many miles.

t may be supposed that there is a large number classified as bronchitis pneumonia; but it requires to be stated that in all the cases of the -mentioned disease, there were no typhoid symptoms present, and only two or three were arranged under this division, whose conva- ence extended beyond the tenth day, while the greater number of the monic patients were bled from the arm, and the blood found deci- y buffy.

e case marked suppuration of the kidneys was one of peculiar interest. patient had been delivered of a child about a fortnight before her ad- ion, and was at this latter period found quite comatose, but there none of the peculiar symptoms of typhus present. The inspection, ever, cleared up any doubt that existed as to the nature of the affec- , for both kidneys contained numerous small abscesses throughout r whole texture, there was pus in both pelves, in the ureters, and blad- , but no urine in the latter organ.

Ve shall next present a table, showing the maximum of the pulse and period of complete convalescence in 181 cases of eruptive typhus and thirty cases of febricula, that were admitted into the Glasgow Fever spital from May 1st to November 1st, 1839, and it includes the whole t were admitted within that period, except two or three, whose conva- cence and pulse were not noted, and those that were omitted for reasons be presently stated.

Table of the Maximum Frequency of the Pulse in 181 *Cases of Eruptive Typhus.*

Males.		Females.	
Maximum frequency of Pulse.	No. of Cases.	Maximum frequency of Pulse.	No. of Cases.
86	5	96	12
96	20	98	1
100	8	100	3
104	4	104	5
106	3	108	23
108	15	110	1
110	1	112	3
112	4	116	3
116	4	120	17
118	1	124	7
120	18	130	10
124	5	134	2
128	1	140	4
130	1		
	90		91=181

Average maximum of pulse in Males = 107·5.
" " Females = 114·1.
" " Males and Females =110·8.

The five cases in which the pulse is marked 86 were admitted on the seventh, ninth, eleventh, fourteenth, and twenty-first days of the disease,

so that it is probable that partial convalescence had commenced time the pulse was noted.

Table showing the Day of the Disease on which complete Convale was established in 181 *cases of Eruptive Typhus.*

MALES.		FEMALES.	
Day of Disease.	No. of Cases.	Day of Disease.	No. of Cas
12th	1	13th	2
13	4	14	7
14	2	15	11
15	9	16	3
16	9	17	9
17	9	18	10
18	6	19	6
19	7	20	10
20	3	21	3
21	10	22	5
22	8	23	2
23	2	24	3
24	6	25	1
25	2	27	4
26	4	28	1
27	4	29	3
28	1	30	2
29	3	32	1
		34	4
		36	1
		44	1
		54	2
	90		91=18

Average Convalescence in Males = 19·7 days.*
„ „ Females = 21·3 days.
„ „ days in Males and Females = 2(

Every case below twenty years of age has been excluded, becau maximum of the pulse varies more from childhood to adolescence during any other similar period of life; and those who died have als excluded, as the comparison between the pulse and the recovery not be uniform in the two diseases, and as the average maximum pulse of those cases which terminated fatally was greater than t those who recovered.

We have taken the eruptive cases of typhus only by which to illu the law of convalescence and frequency of the pulse; in order to pı any doubt as to the nature of the fever from which the conclus drawn, and because they constitute the large majority of fever pat But it may be said that though the non-eruptive cases constitute a or perhaps only an exceptional proportion of the whole number, the not follow the same law as the majority, but may be milder, and th severity of the cases is in proportion to the amount of eruption.

* Dr. Henderson states that he has seen instances of convalescence on the s and eighth days, in which the eruption had existed; but it is not mentioned a stage of convalescence the calculation was made, and what were the ages of the pı Edin. Med. and Surg. Journal, Oct. 1839, p. 430.

lenderson supports this opinion, which is founded on the examination f about 200 cases in the Edinburgh Infirmary. We can so far support e author of this paper in regard to the general severity of the cases tended with a copious eruption; but certainly there is no uniform pro-rtion between the two, for we have frequently met with mild cases of phus in which there existed a copious eruption, and occasionally with me which terminated fatally when there were only a small number of ots. Indeed, reasoning by analogy from scarlet fever, to which typhus has st resemblance in the irregularity of its eruption, we should be led to fer that the intensity of the symptoms would not probably correspond iformly with the copiousness of the eruption; for cases of scarlet fever ve often been found very malignant during some epidemics, although t characterized by any exanthematous eruption, or by one which was ly extremely indistinct or evanescent.

There seems to be, therefore, no valid reason why the law of typhus specting complete convalescence and the frequency of the pulse should t be deduced from the eruptive cases, as they constitute, at least, about ree fourths of the whole number, and as there is no uniform proportion etween the amount of the eruption and the severity of the symptoms.

Table of the Maximum Frequency of the Pulse in 30 *Cases of Febricula.*

Males.		Females.	
Maximum frequency of Pulse.	No. of Cases.	Maximum frequency of Pulse.	No. of Cases.
68	2	72	1
72	7	74	1
76	1	76	1
82	1	84	3
84	1	88	1
86	1	90	2
92	1	92	2
		96	1
		100	3
		104	1
	14		16=30 tot.
Average maximum of pulse = 82·8 in Males and Females.			

Table, showing the Day of Disease on which complete convalescence was established in 30 *Cases of Febricula.*

Males.		Females.	
Day of Disease.	No. of Cases.	Day of Disease.	No. of Cases.
4th	1	3d	1
7	3	4	1
8	3	5	1
9	2	6	1
10	5	7	2
		8	3
		9	3
		10	4
	14		16=30tot.
Average convalescence = 8 days in Males and Females.			

These tables show that, in 181 cases of eruptive typhus occurring adults, the maximum frequency of the pulse was not below 96, except five cases; that in about three fourths it was 108 and upwards, a that the average maximum of the whole was 110·8. They also show th only one case of typhus was convalescent on the 12th, and six on t 13th day of the disease out of this number; and that the avera convalescence of the whole was 20·5 days. Contrast this with febricul in which out of 30 cases the pulse did not exceed 100, except in one p tient, in whom it was 104; and the average maximum of the pulse for t whole was only 82·8. The convalescence in any of these cases of f bricula did not exceed the 10th day, and their average convalescen was 8 days. Are there, then, reasons for maintaining the opini that these short and mild fevers are specifically different from typhus, opposition to that of Bateman and many eminent authors? We thi there are; for if diseases are to be discriminated by a difference of la and phenomena, there is certainly in these two affections a wide d tinction in their symptoms, and also a distinct line of separation betwe them as regards the period of their duration and the frequency of t pulse.

This view is supported by the fact that febriculous patients have bee frequently affected with typhus during their convalescence in the Glasgo Fever Hospital, which cannot be satisfactorily explained on any othe principle than that these two affections are different in their nature The causes which are generally assigned for febricula also tend to sup port its disjunction from typhus; for although they have not been suf ficiently investigated, yet there is an approximation to something like a proof, that exposure to cold is more frequently an antecedent to this af fection than it is to typhus. The following table shows the causes that were assigned for the following cases of febricula:

Cold.	Uncertain.	Contagion.		Total.
22	28	10	=	60

This result tends to support the popular belief and that of many medical practitioners, that there is a short fever which has sometimes been called "a cold fever," although not necessarily attended by a cough or other pectoral complaint.

It is not probable that this affection is contagious, for though more than one in a family sometimes become affected, this is not generally the case, as in typhus among the lower classes; and it is rare that more than one person from the same house has been admitted for this disease into the Fever Hospital. Besides, the fact formerly stated respecting the almost uniformly typhoid and exanthematous character of the disease in the Glasgow Fever Hospital, when nurses and hospital attendants became affected, has a tendency to support this belief; for cases of febricula are always found associated with typhus in every institution of this kind when there is no particular restriction respecting admission; and if it were contagious, it is probable that some of the attendants would have been affected with it. And though these short and mild fevers are not generally described and classified separately as to their phenomena and laws, there is abundant evidence existing in the writings of our British and Irish authors to prove that they constitute a greater or less proportion

of the fever cases of Great Britain and Ireland. It does not appear to be confined, like typhus when not epidemic, to particular localities, such as large towns, &c., and in all probability it is the most common sporadic fever met with in many country districts. It seems also to be capable of attacking the same individual more than once during his life; and we have in a number of instances attended the same individual within a few years under two different attacks, both having the same characteristics of mildness and shortness. If this view be adopted, it may account to a certain extent for the statements that typhus fever has often been known to affect a person more than once during his life; the one fever being confounded with the other.

We have no facts sufficiently conclusive to bring forward respecting its mortality; but undoubtedly it is very small, unless complicated with a particular local affection: and when a disease which originally has all the characters of febricula becomes protracted, the diagnosis becomes so obscure that any deductions drawn from it are very questionable.

If the analysis of the cases admitted into the Glasgow Fever Hospital during the six months already specified be granted, it will tend to reduce the number of those without eruption very considerably. It is stated at page 21 that there were 250 cases without eruption, and 441 in whom this exantheme was observed. Now among these 250 cases there were 145 other affections than typhus, which, being deducted from 250, leave as those really non-eruptive 105, being above 80 per cent. of eruptive cases. But the number of those cases without eruption might be still farther reduced; for a portion of them were admitted after the tenth day of the disease, when it is presumable that the exantheme might have disappeared, and some of them were verging on convalescence; so that even during the non-epidemic prevalence of typhus, when other febrile affections bear to it a larger proportion than when it is extensively diffused, the number without eruption is not very great; and this fact may account for the opinion which is held by some authors and by many medical practitioners that the exantheme is chiefly characteristic of typhus during the prevalence of an epidemic.

2. *Sources of Gastric or Intestinal Fever.* This febrile affection is very often of an ephemeral kind, lasting only two or three days, and hence it is not frequently met with in hospital practice. Sometimes it results from excesses in eating and drinking which have been repeated in rapid succession; occasionally it is caused by a single indulgence in some aliment difficult of digestion. Persons who have feeble or dyspeptic digestive organs, particularly if the bowels be constipated, are very liable to this affection if their habits be irregular. The person attacked generally feels a kind of malaise for some days previous to the rigor which often ushers in the febrile symptoms; the pulse is sometimes extremely rapid, the skin hot, the tongue is coated with a thick white fur, and there is frequently nausea and an uneasy feeling in the abdomen, which is more or less tumid. The bowels are always either constipated or there is diarrhœa, and when the latter symptom is present, even when the stools are feculent, there is very generally reason to suspect, at least at the commencement of the disease, the existence of solid excrementitious matter in the cells of the colon. This affection is sometimes suddenly terminated by a copious perspiration; but more generally, not

until the bowels have been freely unloaded of their feculent contents; and we have repeatedly met with cases of obstinate constipation, in which the febrile symptoms did not completely subside for six or eight days.

In many cases it may be distinguished from typhus at the commencement by ascertaining the antecedent circumstances of the patient, and by the state of his bowels and abdomen. When the diagnosis is doubtful during the progress of the affection, its short duration in the great majority of cases will necessarily distinguish it from typhus. In some instances, however, particularly when diarrhoea is present, the attack is prolonged for a week or two, and sometimes for two or three weeks. In some of these cases there is a tendency to peritonitis, while in others there is reason to suspect some enlargement or ulceration of the glands of the intestines. We are quite aware that such cases, which are not of frequent occurrence, might be called typhus fever without eruption; and in the present state of our diagnostic means this question cannot be solved in a satisfactory manner; but we hope that future investigators will be able to define a line by which they may be distinguished. That intestinal fevers, even those of a protracted nature, are specifically different from typhus may be deduced from the fact that repeated instances have occurred of such patients being affected with eruptive typhus during their convalescence in the fever hospital, and a case of this kind is mentioned in the table of secondary diseases at pages 26-7. Dr. Lombard, of Geneva, in a recent publication, maintains the opinion that there is a bilious fever which is quite distinct from the typhoid fever; but at the same time acknowledges the extreme difficulty of the diagnosis. He states that "the facts collected justify the inference that there are insensible degrees between a simple 'embarras gastrique' and the most severe typhoid fever; but it does not thence follow that there are no true bilious diseases and no true gastric derangements, because we have cited cases of this kind which have terminated by death without presenting any of the lesions characteristic of typhoid fever; only it appears very difficult to distinguish if a mild case of gastric derangement arises from a simple derangement of the alimentary canal, or if, as in a case related of a suicide, it is accompanied by a development of the glands of Peyer. Perhaps in the lenticular eruption may be found the distinctive sign of the intestinal eruption and of the bilious disease. But farther observations are necessary to determine this in at all a satisfactory manner."*

Various other forms of fever than those we have described have been mentioned by authors; but we have seen no reason to believe, either from the account given of them or from our own experience, that there are any other species.

The typhoid eruption has been found in almost the whole of those that were formerly considered distinct fevers; and has identified into the same species, synochus, typhus mitior and gravior, adynamic, ataxic, putrid, spotted, and jail fevers; while synocha or inflammatory fever is admitted to have scarcely an existence in this country, and it is not very easy to conceive how inflammation could exist without the presence of some local inflammatory action.

3. *Bronchitis.* Bronchitis is a frequent complication of typhus fever;

* Clinical Remarks on Bilious and Typhoid Fevers, p. 16.

but this inflammatory affection is also confounded with it and other continued fevers when there is the strongest evidence for believing that the febrile symptoms are solely dependent upon the bronchial inflammation. It may be distinguished from typhus by the affection of the bronchi being almost uniformly the first symptoms of the disease, as indicated by hoarseness, cough, dyspnœa; whereas the bronchitic symptoms in fever are rarely present to any extent at the very commencement. The febrile symptoms in bronchitis are almost always proportionate to the greater or less severity of the bronchitic inflammation, increase as it increases, and decline when it is diminishing, which latter result is often well indicated by the expectoration of yellowish opaque mucus. The duration of bronchitis is also generally shorter than that of typhus, unless it be complicated with some pneumonic inflammation; and when this occurs there may be some difficulty in determining the case. If, however, there be the distinct stethoscopic signs of pneumonia, if the blood be decidedly buffy, and not simply coated with a whitish or greenish-white pellicle, if the skin be of its natural whiteness, if the febrile symptoms be proportionate to the local affection, if there be no stupor, delirium, or suffusion of the eyes; even although the typhoid eruption be not present, there will be a tolerable certainty that the disease is not typhus fever. The effects of a full bleeding are not to be overlooked; for in pure pneumonia, its influence in reducing the frequency of the pulse and the urgency of the other symptoms is generally very decided, which is by no means the result when typhus is associated with this disease.

I. *Alleged sources of continued fevers from putrid effluvia.* It is a well-established fact that the accidental inoculation of the body with decayed or putrid animal matter has produced morbid symptoms, resembling in some respects those of typhus fever, and many medical men have been so affected, after making necroscopic inspections. There is always, however, in such cases extensive local disease of the member inoculated, or a diffused cellular inflammation. According to the researches and experiments of MM. Gaspard, Majendie and Leuret, and Hamont, putrid animal matter, when injected into the veins of healthy animals, proves speedily fatal,* and putrid vegetable matter acts similarly, though to a less degree; while the symptoms induced have some resemblance to those in typhus fever.

The following were the symptoms which were produced in a dog, into the jugular vein of which M. Gaspard injected a putrid solution of fermented cabbage, on the 14th July, 1821. Some hours after the injection of the liquid, there was great malaise, difficult respiration, vomiting, and great weakness. At the end of nine hours a very copious black and liquid stool. On the 15th, the weakness was more considerable; there was lateral decubitus, small and feeble pulse, ardent thirst, natural and abundant urine, free respiration, strong pulsations of the heart, as in aneurism with hypertrophy of that organ. On the 16th, some improvement, less weakness, no pulsations of the heart, great thirst, disinclination to food, fever, and occasionally vomiting of drinks. 17th, the same symptoms. 18th, symptoms aggravated, extreme feebleness, staggering locomotion, excessive thirst, red inflamed eyes and filled with mucus, tumefied nostrils obstructed with mucus, mucous membrane of mouth

* Christison on Poisons, p. 583.

red and phlogosed, a liquid grayish-white stool with some clots of putri blood, and death at the end of the fifth day of the experiment. On dis section, the lungs were found black and slightly inflamed, but still suf ficiently crepitant. The right ventricle of the heart contained an albu mino-fibrous concretion, which extended into the superior cava and pul monary artery. The mucous membrane of the intestines, especially tha of the duodenum and rectum, and a portion of the small intestines wa violet-red, as if ecchymosed, inflamed chiefly in the form of longitudina wrinkles and by irregular plates, which variegated the exterior of th intestines before their incision. The mucous glands of the rectum wer swollen and very distinct. The mesenteric glands appeared to be en gorged with blood and were completely inflamed, the gall-bladder wa filled with black, thick, and ropy bile.*

In several particulars the symptoms of a malignant case of typhu were exemplified in this experiment upon the dog; the small quick puls the peculiar decubitus indicating great weakness, the black stools, th red colour of the mucous membrane of the mouth and fauces, the in jected eyes, and finally the staggering as indicative of delirium. Th necroscopic inspection also furnishes some points of resemblance, namel the inflammatory patches in the mucous membrane of the intestines, th enlarged glands in the rectum, the swollen and engorged mesenteri glands, the black ropy bile; all of which are pathological appearanc more or less frequently met with in typhus. M. Majendie found th fatal effects were produced by confining dogs over vessels in which an mal matters were undergoing the process of putrefaction; but pigeon rabbits, and Indian hogs were not in the least injured by a residence i the same cage for nearly a month. He repeated many times this exp riment with dogs, and always obtained the same result with one excep tion; but he states that in this case the dog was acclimated, for th injection of a putrid liquid into his veins had little effect upon hin The symptoms, however, are different from those produced by the inje tion of a putrid fluid into the veins; for the animals seem to die onl from extenuation at the end of about ten days; and the post-mortem ap pearances are a total absence of fat, of aliments in the stomach, and chyle in the lacteals; while the mucous membrane of the intestines is in flamed, but less so than when putrid matter is injected into the veins.† appears, however, well authenticated that workmen employed in peculi manufactories, and who are constantly exposed to the effluvia arisin from animal substances in a state of putrefaction, are not subject to an of those morbid effects which result from the injection of putrid matt into the veins, or, according to M. Majendie, to those which result fro exposure to putrid effluvia; there must, therefore, be some other expl nation given of the last-mentioned author's experiments, or some unknow concurring circumstances must be required to bring the poison into op ration. One of the most remarkable and repulsive manufactories rather nuisances of this kind is the Chantiers d'Ecarrissage de la Ville Paris. It is an inclosure of many acres of ground, situated close to th walls of Paris, and has existed for several centuries. Into this receptacl are carried the contents of the necessaries of the city; and the carcas of 40,000 or 50,000 horses, dogs and cats are flayed and cut up the

* Journal de Physiologie, tom. ii. p. 16. † Ibid., tom. iii. p. 85.

annually. Various parts of these animals are separated and manufactured for sale: the intestines into gut for machinery; the fat is melted for blow-pipe lamps; the flesh, blood, &c. are collected for manure; a compost is made to breed maggots for feeding poultry, and the bones are chiefly used as fuel. Hordes of rats live in this bed of filth and extend their ravages extensively in the neighbourhood. The fetor which arises from it is overpowering, and often spreads to a great distance. It is remarkable, however, and contrary to every preconceived notion that could be formed respecting its salubrity, that the workmen of this establishment and their families are healthy, the most of them being stout and long-lived. This fact has been established satisfactorily by Parent-Duchatelet. This author states that they have all the characteristics of the most blooming health, that in this respect they resemble butchers, and that they seem to attain longevity more frequently than other artisans. Even new workmen employed upon extra occasions, although not acclimated, do not appear to be more susceptible, nor do they become affected with any disease. During the time that cholera prevailed in France, not an écarrisseur was affected with the disease, and not one was sick; and the mortality of the village which is in the vicinity of Montfaucon was very small when compared with that of Paris. He also quotes the innocuous influence of the human bodies which are exhumed to the extent of 200 annually from Père la Chaise, and the exhumations from the cemetery des Innocents, amounting to about 20,000 bodies annually, which occupied three years in the execution, and which was also carried on during the greatest heats of summer.* Dissecting rooms are also situations where putrid effluvia are constantly present; and it has been affirmed that those who are much confined to these places do not enjoy good health, and are subject to fevers. MM. D'Arcet and Parent-Duchatelet state that the most frequent indisposition among those who are engaged in dissections is dyspepsia and diarrhœa, but that this latter affection is frequent among the strangers who arrive at Paris. These authors cite an immense number of authorities of the highest respectability, namely, Boyer, Dupuytren, Lallemand, Roux, Jadelot, Breschet, &c. to prove that dissecting rooms are not insalubrious and are not productive of fevers. M. Andral states that gastro-enterite, meningitis, and typhoid fever are common among the young élèves of medicine during the first year of their residence at Paris; but so little does this depend upon their sojourn in the dissecting amphitheatre, that among those who are affected, there is at least as many seized before they commence their dissections as after this period. He adds that the health of the men employed in handling the débris of dead bodies is similar to that of other individuals.† The workmen employed in the manufacture of strings for musical instruments are exposed constantly to the putrid effluvia of animal substances, arising from their long maceration, and they are not more subject to fevers than other tradesmen.

Butchers, who are believed by some authors to be almost exempt from fevers, are exposed in the slaughter-house to the emanations arising from the putrid blood and other animal fluids, which are frequently allowed to stagnate, and which are sufficiently indicated by the fetid and insup-

* Annales d'Hygiène Publique, tom. viii. p. 139. † Ibid., tom., v. p. 301.

portable odour which issues from these places during hot weather. The atmosphere of whale vessels must be constantly impregnated or rather saturated with the effluvium that issues from large and numerous fishes; yet fevers are not prevalent among the seamen. Majendie states that the most deleterious animal poison is the putrid water of fishes: when some drops of this water are injected into the veins, in less than half an hour symptoms very similar to those existing in typhus and yellow fever are produced, and the animal dies in about twenty-four hours.* It appears from these facts that persons may live constantly amidst the most concentrated putrid animal emanations and yet not contract fever of any type; may enjoy health of the most perfect kind; attain longevity in many instances, and be less subject to some epidemic diseases than the inhabitants in their neighbourhood. It may be asked how are the experiments of M. Majendie and others to be explained upon this view? It does not appear from M. Majendie's experiments that the same symptoms or pathological appearances were produced by exposing dogs to putrid animal emanations, as by injection of a putrid fluid into the veins; indeed, he admits this himself; but adheres to the belief that the effluvium was the cause of death in the dogs subjected to experiment, although no injurious effects were produced on several other animals. Many animal poisons, however, operate differently on different organs and tissues; and this is well exemplified in an experiment mentioned by Dr. Christison, namely, that "a pupil of Professor Mangili swallowed at once the whole poison of four vipers without suffering inconvenience;"† but if a small quantity of this be inserted into a wound, poisonous effects are always produced. From a consideration of the whole evidence that might be adduced respecting this point, it may be drawn as a conclusion that although putrid matters, when injected into the veins of animals, cause death under symptoms similar to those of typhus fever, yet that the effluvia arising from similar matters do not under ordinary circumstances produce any deleterious effects on man. That there are exceptions to this general law we doubt not, such as Olivier being affected with diarrhœa after visiting a cellar filled with old bones, and Chevallier being seized with the same disease after exposure to the emanations from dead bodies; but that the effluvia arising from animal substances in a state of putrefaction constitute any regular source of continued fevers, we think there are no grounds for believing.

II. *Alleged sources of continued fevers from the exhalations of the human body.* Another modification of putrid miasmata has been noticed by almost all authors as a cause of fever, namely, the concentrated exhalations from the human body. Sir John Pringle and other army as well as navy physicians have remarked that fever was often produced in crowded hospitals, especially during hot weather, and also in crowded barracks and in transport ships, when filled beyond a due number. Dr. Tweedie makes the following statement: "The late Mr. John Pearson told me that when he was surgeon of the Lock Hospital, he uniformly observed when more than a certain number of patients were placed in any of the wards, fever became prevalent in the establishment; and that

* Journal de Physiologie, tom. iii. p. 83. † Christison on Poisons, 3d Ed., p. 571.

from repeated observation of this fact, he was induced to restrict the number of beds in each ward, and never afterwards witnessed the recurrence of fever in the house."* Dr. Bateman remarks that "if it had not been already demonstrated on the most copious evidence, that the mere accumulation of animal matter in a putrescent state is incapable of generating fever; yet the fact that the closeness of the habitations of the poor, the uncleanliness of their persons, furniture, and apparel, and the accumulating filth in the lanes and alleys which they occupy remain unchanged in all seasons, while epidemic fever appears but rarely and with long intervals of absence, is decisive against the supposition that the latter is engendered from such sources."† It is singular that a writer of such distinguished accuracy, after having drawn so fair a conclusion from the facts connected with the prevalence of fever, should apparently contradict this; for he states in a note that "the morbid and even natural effluvia of the living body, when allowed to accumulate by want of cleanliness and air, are unquestionably common sources of fever, and contribute mainly to its propagations as has been intimated in the preceding note."† The inhabitants of some countries, such as the natives of Kamstchatka, are remarkable for their filth and for living amidst the most foul and putrid effluvia; and yet fever is not known among them. The places they live in are called yourts, which "are sunk seven or eight feet below the surface of the ground, and are covered with a thatched roof in the form of a truncated cone, open at the top; they consist of one small apartment, which usually contains six families, with their utensils and stock of provisions for the winter, the chief part of which is dried fish almost putrefied. . . . . Here they eat, drink, and sleep, crowded promiscuously together, and satisfy all the calls of nature without modesty or restraint, and never complain of the noxious odour that prevails in these habitations."‡

The same mode of living is practised by the inhabitants of the Island of Oonalaska, by the Samoiedes, by the Greenlanders and Esquimaux; and there are no continued fevers among them, although scurvy prevails to a considerable extent. In many parts of Russia the same system of living in filthy and unventilated houses, and in an atmosphere saturated with human effluvia, is practised; yet no febrile disease is the result. Dr. Bancroft quotes the slave-ships as examples, where an atmosphere is more offensively impregnated with human exhalations, excretions, &c. than could probably be found in any other place of confinement, and makes the following statement: "I am fully convinced that fever of any kind rarely occurs on board these vessels, and contagious fever never; though great mortality has frequently happened from other diseases, and more especially from dysentery. . . . . There certainly is nothing in the constitutions of the negroes which exempts them from typhus or contagious fever; on the contrary they have been found as susceptible of it as whites, and considerable numbers of them who were sent from this country and from Nova Scotia to the new colony of Sierra Leone died of it on their passage thither, as will be more fully related in another place."§

* Tweedie's Clinical Illustrations of Fever, p. 83.

† Bateman on Fever, pp. 5, 6, and 7.

‡ Bancroft on Yellow Fever, &c. p. 121. § Ibid. p. 129.

Dr. Bancroft quotes a very remarkable instance of crowding in the Decade frigate during the revolution in France; where 193 persons were crowded to as great a degree as the negroes are in slave-ships, yet not one of them died during a period of ninety-six days.

III. *Alleged sources of jail fever from filth and an impure air.* The breaking out of fever in jails has often been brought forward as a proof of the origin of the disease from filth and an impure atmosphere, being afterwards propagated by contagion; and Sir John Pringle's aphorism is frequently quoted or alluded to by writers on fever, namely, that "the cause seems plainly to arise from a corruption of the air pent up and deprived of its elastic parts by the respiration of a multitude, or more particularly vitiated with the perspirable matter, which, as it is the most volatile part of the humours, is also the most putrescent."

Dr. Bancroft makes the following very pertinent remarks upon this point: "That this fever often exists in them (jails) cannot be denied; but this circumstance can afford no evidence of its having been generated therein any more than the multiplication of vermin in such places could demonstrate the spontaneous generation of these and other insects by the nastiness which favours the deposition and hatching of their eggs.* . . . . Indeed, if it were true that the vegetable or animal matters while decomposing or putrefying could, de novo, generate contagion properly so called, the species or varieties of contagion ought necessarily to have become as numerous and various as the matters so decomposing, and also as various as their relative proportions; every dunghill, every collection of rubbish and filth, ought to be capable of generating the cause of a new disease, and that disease ought to be capable of reproducing itself in other persons."† In estimating the value of the testimony that is generally brought forward to prove the spontaneous origin of fever in jails, and the great improbability of the first person attacked, who may have been resident there for several months, being infected previously to his imprisonment, the remarks which we formerly made respecting the impossibility of tracing the contagion in similar situations, even of diseases universally admitted to arise from that cause alone, will also apply here. And although jails may apparently be the most secure places against the inroads of contagion, from the number of their bars and gates; yet their inmates, from the nature of their offences and their dependent situation, must have a more frequent communication with their friends, either personally or through the medium of clothes, than is generally supposed, and that too frequently with the most filthy and debased of the human race. The question relative to the spontaneous origin of fever in jails seems to be almost solved by the fact that the same cause existing in a variety of other situations produce no disease like continued fever; and it can be proved from the history of other prisons, namely those in Switzerland, Italy, Russia, &c., and similarly circumstanced as to filth, want of ventilation, &c., that no such diseases were known there. Mr. Howard, who has investigated this subject in his work on prisons states: "If it were asked what is the cause of the jail fever, it would,

* Bancroft on Yellow Fever, &c. p. 149.
† Ibid., p. 105.

n general, be readily replied, the want of fresh air and cleanliness; but s I have found in some prisons abroad, cells and dungeons as offensive nd dirty as any I have observed in this country, where, however, this istemper was unknown, I am obliged to look out for some additional ause for its production."*

The following fact is worthy of being quoted, as illustrative of the effi-cy of cleanliness and disinfection of suspected clothes, &c. in pre-nting the introduction of fever into jails, which were formerly so much fected by this disease: "In the jail at Cork, the prisoners remained e from fever when it had spread in every direction among the inhabi-nts of the city. To prevent its introduction, means were employed hich deserve record: jail dresses were provided for the prisoners, whose othes on their admission were removed and heated in a stove, and their rsons washed and cleaned; the bedding was occasionally steeped in ymuriatic acid water and then stoved; patients in whom fever showed self were immediately removed to an hospital; this system was continued uring a year and a half, in the course of which time two prisoners died f dysentery but none of fever; when the medical inspector for Munster ade his visit to the jail, the system had for some time been discontinued n consequence of the expense attending the jail dresses, and then fever egan to show itself among the prisoners, and a few cases were found n the jail at that time."†

From a review of the whole facts connected with filth and deficient entilation, it appears that both in the countries where continued fevers revail and in those where they do not exist, the inhabitants may live onstantly amidst this impurity and yet be entirely exempt from any fe-rile disease of this kind; and that if filth and an impure air were a ommon source of fever in jails, hospitals, &c., without the influence of ontagion, they would produce the same effects in all other countries and ocalities similarly situated and circumstanced. The opinion, there-ore, which is so generally admitted and propagated by many of our first uthorities, that fever may arise from common causes, such as putrid mi-asmata, contaminated air, &c., and yet afterwards be propagated by con-agion, receives no support from this presumed source; for though we re not prepared to assert that febrile affections may not, under peculiar ircumstances, arise from these causes; yet it is undoubtedly deducible rom the evidence that they are not ordinary or even limited though egular sources of the disease in any form.

IV. *Alleged sources of continued fevers from river malaria.* Before oncluding this part of the essay, we shall notice an hypothesis which as lately been somewhat confidently brought forward to account for he prevalence of typhus in some large cities, namely, that a peculiar malaria is generated by the animal and vegetable filth which accumulates along the sides of rivers running through large towns, and that the in-habitants who live in their immediate vicinity become thereby subject to fever. We are quite aware that very disagreeable and sometimes fetid effluvia occasionally arise from such situations, particularly during hot

* Bancroft on Yellow Fever, &c. p. 149.
† Barker and Cheyne on Fever, vol. i. p. 97.

weather; but that it is capable of causing continued fever has not eve been rendered probable by any satisfactory evidence.

We presume that this point may be determined by the locality Glasgow; for the Clyde runs through the town, and has a numerou population inhabiting houses close to its banks. This river is also considerable magnitude; and certainly there is abundance of filth deposited in its bed by the numerous common sewers and public works i Glasgow. We have kept a record of the places of habitation of 934 persons who were admitted into the Glasgow Fever Hospital from January 1s to November 1st, 1839, and have classified the cases in the manner shown in the following table. The town has been divided into seven districts: 1, includes all the streets parallel to the river and close to its banks on both sides; 2, all the streets on both sides of the river, which run at right angles to it and which open into it—these first two divisions are of course excluded from the others; 3, east district of the town, from the cross eastward; 4, west district, from Buchanan street westward, and bounded on the north by Sauchie-hall road; 5, north side, northward of Sauchie-hall road and Rotten row; 6, south side of the river, with the exception of those streets close to its banks or which open into it; 7, centre of the town, from the Cross to Buchanan street, and bounded on the north by Rotten row.

	Males.	Females.	Total.
Streets close and parallel to river . .	14	10	24
Streets at right angles to and opening into river	56	51	107
East district of town	140	136	276
West district of town	44	54	98
North side of town	44	41	85
South side of town	41	22	63
Centre of town	92	106	198
From the country, 2 to 14 miles . .	46	37	83
			934

It is shown by this table that among 934 cases admitted as labouring under continued fever, there were only 24 who inhabited houses close to the river; and when we take into calculation the large population that live upon its banks, this proportion is very small. Again, in those streets which run at right angles to the river, and which open into it, the number is greater; but it must be remembered that most of these streets are long, and that it is only those inhabitants who live at their river termination that are at all exposed to the effluvia. If this be taken into the account, a fractional proportion only of these 107 cases ought to be calculated. The east district of the town, a situation very remote from the river, furnished nearly one third of the whole cases, and it and the centre of the town together more than the half. These facts clearly show that river malaria has no influence in the production of continued fevers in Glasgow, and that it is proportionally as prevalent, if not more so, in other and more central parts of the town.

CHAPTER II.

CUMSTANCES FAVORING THE DIFFUSION OF CONTINUED FEVERS.

DEMIC diseases have prevailed from the most remote erá of the world, ıve, with one exception, hitherto bid comparative defiance to the phi- ıy of medicine, in its attempts to check their progress or diminish ıortality. Almost every country has its own peculiar pestilence, that ; rapidly away its redundant population at periodical seasons; and s fatal operations ceases, partly from the subjects it can attack reduced in number, and partly because its laws rendered it pro- e from one city or country to another, or because the element that ed its operation had been changed or modified in its constitution. diseases, the contagious as well as the non-contagious, possess the ty of becoming epidemic; and smallpox, which is perhaps the nfectious of all febrile affections, is subject to the same law, being y diffusible during some particular seasons, while in others it re- comparatively inactive. The same periodical prevalence of scarlet and measles is observed during particular seasons, as of yellow which is generally believed to be a non-contagious disease, and to : its origin from vegetable malaria generated in a hot climate.

ere are four prominent circumstances which favour the diffusion of gious continued fever:

. A humid state of the atmosphere.

Poverty, famine, or food of bad quality.

An accumulation of persons not previously affected.

ı. Filth and deficient ventilation.

: do not mean, however, to assert that these are the only circum- :s that operate in the diffusion of contagious fever; for certainly nowledge of the constitution of the atmosphere, particularly its me- logical and miasmatic qualities, warrants no such conclusion. But :an be shown that these circumstances generally precede or accom- an epidemic of typhus fever; although they may not account for all henomena connected with its extension, yet may so far elucidate the ct as to facilitate the progress of future observations or experiments.

Humidity of the atmosphere, scarcity of provisions, filth, and defi- ventilation tend to diffuse continued fevers. Almost all authors who written on epidemic diseases have noticed what is called an epidemic itution of the atmosphere; but this has in general been so indefi- stated, far less defined, that no conclusion can be drawn from their iptions as to the peculiar alterations of which it consists. We shall, 'ore, confine our evidence solely to that state of the atmosphere which ıer cognizable by our senses or by instruments, as it is impossible : present state of our knowledge to advance anything but vague hy- sis, upon what authors designate by the term referred to. Typhus is a disease peculiar to cold or temperate regions, and it does not ır that it is capable of propagation to any extent in a hot climate; owerful heat of the sun in such regions appearing to dissipate or oy its contagious properties. Dr. Bancroft observes that "in ges to the East Indies, ships remain for a much longer space of time een the tropics, and being also exposed to a higher temperature, the :r of heat in destroying typhus fever is in them more decisively ma-

nifested, an entire cessation of the disease (however prevalent) commonly taking place before they can reach the Cape of Good Hope. It has indeed never been known, as I am informed, that a single case of this fever had occurred on either side of the Indian peninsula."* The existence of typhus fever, at least its diffusion, seems therefore incompatible with a powerful or tropical heat of the sun, and in that respect it differs very essentially from yellow fever; but the ordinary heat of a temperate climate does not extinguish it or even materially check its progress; for it has often prevailed epidemically during summer as well as winter; though it has been generally observed that the seasons during its prevalence were attended with more than the average quantity of rain. In adducing evidence to prove this point, we shall also include that which establishes the coexistence of scarcity of provisions with its consequences, namely, filth and deficient ventilation, as the descriptions of authors generally comprehend these concomitant circumstances. Drs. Barker and Cheyne state, on the authority of Rogers, that "after the year 1721, there was again an interval of good health in Ireland so complete, that scarcely a case of fever was to be met with; this continued till the year 1728, when, as we learn from Boulter's Letters, there had been three bad harvests in succession. Oatmeal, the chief food of the poor in the north, rose to an extravagant price; in the south, the scarcity was so severely felt, that on the 26th of February there was a great rising of the populace of Cork, who threatened to pull down the Mayor's house. . . . From 1728 fever gained ground, and continued to be epidemical until 1732. . . . In the winter of 1739-40 an intense frost attended with a high wind at S. E. and E. intolerably piercing, set in on the 27th of December, and continued with little interruption till the middle of February." The following season was one of great scarcity, and in "the autumn of 1740, which was unusually frosty, with a continued prevalence of N. and E. winds, fever which had been frequent became epidemical; it did not cease in the winter, and increased most alarmingly in the spring and summer of 1741." O'Connell estimated the mortality of that epidemic at 80,000 persons. "The year 1800 was nearly as unfavorable to the fruits of the earth as 1799. The summer of that year was unusually dry; then followed a short period of uncommon heat; for three weeks or a month the thermometer, when at its greatest height during the day, seldom fell below 70 degrees; cold and wet weather set in about the end of August or beginning of September. Thus a short period of uncommon heat degenerated into an ungenial autumn, yielding in some soils an imperfect produce, whilst in others, the failure of the crops was little less complete than in the preceding season; so that, notwithstanding bounties were granted on the importation of foreign corn, and the distillation of spirits from grain prohibited, yet the price of bread and potatoes, both of bad quality, together with that of every other necessary of life, was raised beyond all precedent. In the autumn and winter of 1800, the inhabitants of this kingdom universally suffered from a contagious fever, in which the troops still continued to participate. . . . . In August, 1801, the garrison of Dublin suffered greatly from petechial fever, which very generally prevailed among all ranks in the metropolis and its vicinity. The epidemic which had now reached its height shortly after began to decline,

* Bancroft on Yellow Fever, &c. p. 510.

before the good effects of an unusually abundant harvest, in again
g provisions of all kinds to the poor at a moderate rate, had
t. The winter of 1813-14 had been uncommonly severe, that
16 did not fall short in severity; but particularly so in the early
816, when the cold was very great in these countries. In the
' February, 1816, the quicksilver in the thermometer in many parts
nd fell below 0°. Thus at Northampton, on the 9th of February,
fell to 4°, and on the tenth to 2·75°. In the neighbourhood of
it fell to 5° below 0°, and during four days of that month it never
he freezing point. . . . . From a registry of the weather
Dublin, it appears that the mean temperature of the months of
summer, and autumn, commencing with February and ending
tober of that year, was nearly three degrees and a half below
he similar preceding period; thus the medium temperature in
is 54·32°, and during the same time in 1816 it was only 50·9°,
ence amounting to 3·42°. In neighbouring countries similar ob-
is were made. According to those of Mr. Howard, in the neigh-
d of London the mean temperature of the same months in 1815
°, and in 1816 only 49·9°, the difference amounting to four de-
. . . . The quantity of rain which fell during the summer
umn of 1816 was also very great. During the months com-
with July and ending with October in that year, being the
f harvest, the humidity of the atmosphere was almost incessant:
ng during the greater part of the time in these months. . . .
cts occasioned by unusual cold and humidity and absence of
on the productions of the soil were peculiarly injurious. The
of grain was uncommonly late both in this country and in
. Corn remained uncut during the latter parts of October and
er, and much of it was altogether lost. The same injurious
n the quality of the potatoes were produced as upon the grain,
e roots constitute "the principal or only food of the poor" in
rts of Ireland. "The sufferings of the poor at this period did
end on diminution of vegetable food only; in many or most
Ireland, the straw used for bedding was often half decayed, and
in usually disposed to imbibe and retain humidity; perhaps from
y of the woody fibres. . . . . Turf or peat is the chief fuel
oor in this country, and during such wet seasons it could not be
dried for use. So great was the scarcity of fuel, that the
which in ordinary times are respected as the boundaries of
, were destroyed, and the trees in many places were denuded
branches to supply the necessaries of life; a practice at which
proprietors often connived, sensible that it had arisen from
y the most urgent. Hence dampness of clothes and bedding,
t cooking of food and ventilation of apartments, deficient clean-
persons and dwellings, all depending on the want of fuel,
ted to heighten and extend the calamities of the poor of
at this eventful period. The preceding statement refers to
cts of the cold and wet of 1816 chiefly, but the following year
le inferior in severity. The summer and autumn were humid,
d ungenial, and agricultural produce, with the exception of
, which were more abundant than in the former year, was

almost as scarce as in 1816. . . . The year 1818 was remarka for a state of weather the reverse of that in the years immediat preceding. The spring was moist, but the summer set in with unusu warmth, and proved the hottest which has occurred in this count during many years past." To these causes of distress were added a ve low price for labour, and extensive failures in trade and manufactur Drs. Barker and Cheyne also mention the prevalence of fever on t continent of Europe during a series of previous years, and remark ve justly that "the circumstances of the inhabitants of a great portion the continent at this time, arising from the distress occasioned by it being the seat of war, must have strongly resembled those of the peopl of Ireland during the late scarcity of provisions. At a later period, i 1817, after a failure of the crops, epidemic fever existed in the southen parts of Italy. . . . . From the same authority (Dr. Pockles) w learned that in the early part of 1817, scarcity of food was so great i Germany that many died of hunger; but no epidemic fever existed ther at that time. It had prevailed in that country three years previously and did not then originate in scarcity of provisions, but was traced t the miserable remnant of the French army which entered that countr after its overthrow in Russia. From the facts here adduced, it follow incontrovertibly that during the times of its increase in Ireland fever wa very prevalent in most parts of the continent, and that the circumstance which caused it to spread epidemically were not peculiar to this island . . . . . But whatever may have been the causes which have rendered the disease more than usually frequent during the last nine or ten years no distinct evidence has been obtained of its introduction from the con-tinent; and an inspection of the preceding table (vol. i. p. 49) point out that the rapid increase of the disease depended on general causes, operating on most parts of the country at the same time. For we find that it commenced in places situated most distant from each other in different parts of Munster and Ulster, at the end of 1816 or beginning of 1817; and making the proper allowance for the difficulty of deter-mining when fever became epidemical in places, which are always in-fested by the disease, we must admit that the periods of its manifest increase were nearly coincident. In fact, the scarcity of provisions com-bined with want of employment, whatsoever their mode of operating may have been, appears as the main cause of the spreading of fever epi-demically through this country; although it must also be acknowledged that the simultaneous increase of this disease in Ireland and on the con-tinent, leads to the inference that whatever may have been its origin an epidemic constitution prevailed over a great part of Europe during a se-ries of past years. . . . . With respect to the time of its greatest pre-valence in each of the four provinces it is not easy to decide. In Munster, it appears to have been most prevalent in the summer of 1818, and in Connaught about the same time, whilst the other provinces, where its commencement was latest, the time of its greatest prevalence was re-ferred in Leinster generally to the autumn of 1817, and in Ulster to the winter of that year. In the principal cities, Dublin, Cork, and Limerick, it was most prevalent in the summer and autumn of 1818."*

* Barker and Cheyne on Fever, pp. 25-107.

[To be completed in the next Number.]

We have thus made very copious extracts from Drs. Barker and Cheyne's valuable record of the epidemic fever in Ireland; as it contains a greater amount of facts and observations respecting this disease than iny work that we have consulted. Indeed, when we consider that about ne fourth, or a million and a half, of the population of Ireland were ffected with fever, during the two years that it prevailed, and that acurate communications were received from respectable physicians residing i all the provinces, and that these have been admirably concentrated nd illustrated by the authors, it must be considered one of the most imortant as well as interesting descriptions of the rise, progress, and deline of this disease.

Dr. Adams states that "during the winter of scarcity in 1799 and 1800, ver from infectious atmosphere was so general as to excite us to imitate e example of those manufacturing towns which are never free from the isease, and a fever-house was established in London."* Dr. Bateman emarks that "deficiency of nutriment is the principal source of epidemic ever, and that the circumstances just alluded to (improvement in all the rts of life), operate only as accessories in fostering and multiplying it rill scarcely admit of dispute. . . . . The last epidemic which ccurred in London followed a scarcity of two successive years (1799 nd 1800); and it was during the prevalence of this fever that the neessity for establishing a House of Recovery became manifest. . . . Whether the epidemic of 1817 has been really much more extensive than he former, I am unable to determine. . . . . It might have been xpected, indeed, that the present epidemic would exceed the last in the xtent of its course, since it occurred at a period of unparalleled distress mong the labouring poor; when the loss of employment, occasioned by he termination of the war and the general suspension of the manufacures, concurred with the failing harvest of 1816 to increase the diffiulties of procuring subsistence."† Dr. Tweedie observes that "it is an ndeniable fact founded on the experience of many epidemics, that there re certain circumstances which render the system peculiarly predisposed the action of febrific causes; and the connexion of scarcity and priation with the occurrence of fever among the lower classes of the community, has been so often verified by the experience of epidemics, as ow to be received as a general axiom."‡ The same author also makes e following observations on the influence of the temperature and oisture of the atmosphere: "Though fever can scarcely be said to ave prevailed extensively, or to adopt the common phrase, to have been idemic in London since 1820, yet the diminution of autumnal fevers, r the last two seasons, proves decidedly how much some unknown ndition of the atmosphere influences its prevalence. This condition is timately connected with the combined effects of heat and moisture; nce cold and wet summers are always remarked to be comparatively ealthy, while disorders of the bowels in such seasons are seldom obrved. The number of patients admitted into the Fever Hospital in the utumn months of the last three years establish this principle. In August, September, and October, 1827, there were admitted 205; in

* Adam's Inquiry into the Laws of Epidemics, p. 30.
† Bateman on Contagious Fever, pp. 4 to 11.
‡ Tweedie's Clinical Illustrations of Fever, p. 78.

the same months of 1828, the numbers were 170; in the autu
1829, only 94 were received. The cause of this progressive dimin
is undoubtedly to be traced to the cold wet summers of the las
seasons."* An opinion, exactly opposed to that of Dr. Tweedie, is
by Dr. Armstrong. He states that "in England typhus is evi
favoured by a low temperature, being most prevalent in the cold se
of winter and spring, generally abating or disappearing as the h
summer advances, and often prevailing to a considerable degree in
wet autumns."† Dr. Alison makes the following statement resp
the cause of the epidemic fever which prevailed in Edinburgh durin
years 1826-7: "The chief cause of the unusually great and rapi
tension of fever during last winter was no doubt the very distressed
dition of a great part of the lower order of inhabitants, in conseq
of the diminished expenditure of the higher ranks, and particula
the failure of many speculations in building, which had given em
ment to great numbers of masons, joiners, plasterers, and laboure
. . . A very great number of the patients received into the hos
in fever belonged to families of which the working members had
out of employment for periods varying from six weeks to six mo
and Edinburgh has furnished but too many opportunities, both re
and formerly, for observing that it is among such distressed familie
fever spreads most rapidly and extensively."‡

Dr. Cowan attributes the increase of fever in Glasgow, whic
steadily been going on since 1816, principally "to the total wa
cleanliness among the lower orders of the community, to the abse
ventilation in the more densely peopled districts, and to the accumu
for weeks or months together of filth of every description in our
and private dunghills; to the over-crowded state of the lodging-
resorted to by the lowest classes; and to many other circumstanc
necessary to mention."§ The same author, in another part of hi
tistics, illustrates the causes which tend to render fever epidemic
he makes the following observations: "From the close of 1836
of those periodical depressions in trade, arising from the state
monetary system, has visited this city, and deprived a large prop
of the population of the means of subsistence. From the existe
secret combinations among the working classes in various depart
of trade, but especially among the cotton spinners, and the 's
which resulted from these combinations, a very large proportion
inhabitants, in addition to those already suffering from the state
money market, were suddenly deprived of employment, and conseq
of the means of procuring food. The high price of coal was the
of diminishing the hours of labour, and consequently the amou
wages, in numerous factories, and placed fuel beyond the reach
lower classes for domestic purposes. And in addition to these s
of misery, the average prices of grain were much higher during
than they had been for some years previously.‖ . . . A ref
to the tables of the state of the weather given in the preceding p

* Tweedie's Clinical Illustrations of Fever, p. 80.
† Armstrong on Typhus Fever, p. 8.
‡ Edinburgh Medical and Surgical Journal, vol. xxviii. p. 236.
§ Cowan's Vital Statistics of Glasgow, p. 13. ‖ Ibid. p. 33.

this paper, will show the quantity of rain which fell, monthly, during the period of my attendance on the Fever Hospital, and the average temperature indicated by Fahrenheit's thermometer. From these it appears that the quantity of rain was much above the average, while the temperature of almost every month was lower than that of the previous year; and while the mean heat of Glasgow is 47° 75, the mean heat of 1835 was 46° 58, and that of 1836 only 44° 52.* . . . .

"The number of fever patients treated in Hospital in 1837, was	5387
" " 1836 "	3125
Being an increase in 1837 of	2262
The number of fever patients treated by the district surgeons in their own houses in 1837, was	2320
Ditto Ditto, in 1836, was	716
Being an increase of . . .	1604"

The above table gives a very inadequate idea of the comparative frequency of fever in 1836 and 1837. During 1836, till the month of December, every applicant for admission was received into the hospital: while in 1837, seldom a day passed without numerous applicants being refused admission for want of room; and many were deterred from applying for admission from a knowledge of the over-crowded state of the wards.† Dr. Cowan calculates the number of fever cases in Glasgow, during the years referred to, as follows:

"In 1835	6·180
1836	10·092
1837	21·800
	38·072‡"

After having given a table of the deaths from fever during each month of the years 1836 and 1837, the same author makes the following deductions from it. " Many interesting observations may be drawn from this table. It shows the slow progress of an epidemic disease when trade is prosperous, compared with what occurs in seasons of distress. Up to November, 1836, the period at which the commercial embarrassments were felt, the mortality from fever had not been rapidly increasing. In November it was just about double what it had been in January preceding, the number of deaths being 45 in January, and 89 in November. The moment, however, the effects of the stagnation in trade extended to the working classes, the mortality increased with fearful rapidity, aided, no doubt, by the season of the year, the high price of grain, and the scarcity or high price of fuel. . . . . The table also marks the period in which the epidemic reached its maximum amount of mortality, namely, in the second quarter of 1837, and in the month of May in that quarter, being the month succeeding that in which the strike of the cotton spinners took place, by which 8000 individuals were thrown out of employment."§ The total quantity of rain, according to Dr. Cowan, which fell during 1837, as ascertained at the University of Glasgow, was 26·629 inches; and the mean temperature for that year was 46° 31. ||

* Cowan's Vital Statistics of Glasgow, p. 16. † Ib. p. 37. ‡ Ib. p. 38.
§ Ib. p. 39. || Ib. p. 35.

We have constructed the following tables from the registers of weather and the table of deaths from fever in Glasgow, as given in Cowan's work, in order to show, that though the increased quantit rain during the years 1836 and 1837 was influential in the diffusio fever, yet that it had less effect in spreading the epidemic than o causes; for during the year 1837, at which time it had reached its m mum, the total quantity of rain that fell was less than during 1 while the average temperature of these two years did not materially d from each other.

1836.	Deaths from Fever.	Mean Temperature.	Quantity of Ra
January	45	37°84	3·868
February. . . .	27	35 39	0·732
March	57	38 60	2·375
April	64	42 05	1·098
May	67	51 04	0·173
June	71	55 19	1·812
July	61	54 38	4·536
August	82	53 17	5·317
September . . .	56	47 37	2·134
October . . .	89	42 89	4·988
November . . .	89	38 11	2·004
December . . .	133	38 27	2·673
	* 841 Total.	† 44·52 Average	‡ 31·710 Tot
1837.			
January	201	36°13	1·956
February . . .	138	40 55	2·674
March	224	34 39	1·500
April	202	38 28	1·646
May	233	48 21	1·857
June	199	56 89	2·241
July	194	60 80	3·322
August	172	56 90	2·610
September . . .	126	52 99	1·570
October	149	48 92	2·997
November . . .	147	39 68	2·293
December . . .	195	41 95	1·963
	§ 2180 Total.	‖ 46·31 Average	‖ 26 629 Tot

The number of deaths from fever in a given period is not an ex criterion of the number of persons affected; for the intensity of this dise varies considerably at different seasons of the year; but as the numl of deaths which occurred in each month of 1836 and 1837 respective are given, the relative mortality will give a pretty near approximation the relative number affected.

Dr. Arthur Thomson, in his Statistics of Fever, has given two tabl which show the influence of the seasons on the prevalence of fever; a as his conclusions are drawn from a large number of cases, they well adapted for illustrating this part of the subject.

* Cowan's Statistics of Glasgow, p. 38. † Ib. p. 5. ‡ Ib. p. 4. § Ib. p. 38. ‖ Ib. p.

"TABLE XII. *Showing the maximum, minimum, and mean temperature in Great Britain during each month, from the observations of about thirty years, together with the average monthly quantity of rain in inches from thirty-four years' observation (from 1797 to 1830.)* *

Months.	Mean temperature.	Average quantity of rain in inches.
January	36°	1·90
February	38	1·49
March	43 9	1·39
April	49 9	1·84
May	54	2·00
June	58 7	1·94
July	61	2·55
August	61	2·15
September	57	2·29
October	48	2·41
November	42	2·79
December	39	2·58 "†

From the above it appears that July and August are the months during which the average temperature is greatest, and that the quantity of rain falling during the last six months of the year is considerably more abundant than during the first six. The following table this author has "compiled from materials selected indiscriminately from all the reports which he could obtain, showing the number of fever cases admitted into the various hospitals in Great Britain and Ireland; but he is chiefly indebted to Drs. Barker and Cheyne's account of the epidemic fever which prevailed in Ireland in 1817-18-19."

"TABLE XIII. *Showing that of 51,944 cases of fever admitted into different hospitals in Great Britain and Ireland, the number and relative ratio of admissions in each month were as follow:*

Months.	No. of Cases admitted.	Relative ratio of admissions per cent.
January . .	2895	5·6
February . .	2825	5·4
March . .	3152	6·1
April . . .	3374	6·5
May . . .	3990	7·6
June . . .	4365	8·3
July . . .	4999	9·6
August . .	5261	10·1
September .	5046	9·7
October . .	5624	10·8
November .	5054	9·7
December . .	5359	10·6
	Total 51,944	100·0

"It appears from this table that the greatest number of fever cases were admitted into the different hospitals, during the last six months of the year, or from July to December. And the number of cases admitted

* The maximum and minimum temperature is omitted.

† Howard on Climate of London, 2d Edit. Vol. i. p. 136.

from January to June are few, compared with the admissions fi to December." *

In order to compare the number of admissions in each mon its mean temperature and average quantity of rain, we shall c a table out of the two that have just now been quoted; which the number of admissions, the mean temperature, and the avera tity of rain for each month.

Months.	No. of Cases admitted.	Mean Temperature	Average quantity of in inches
January .	2895	36°	1·90
February . .	2825	38	1·49
March . .	3152	43 9	1·39
April . . .	3374	49 9	1·84
May . .	3990	54	2·00
June . . .	4365	58 7	1·94
July . .	4999	61	2·56
August . .	5261	61	2·15
September .	5046	57	2·29
October . .	5624	48	2·41
November .	5054	42	2·79
December .	5359	39	2·58
	51·944		

This table shows that the greatest number of fever cases were into the various hospitals from July to December, or during the months of the year; and that during this period the average qu rain which falls is much greater than during the first six mont year. If we compare any one month of the last six with any on of the first, there will be found a similar difference. The sa also shows that the temperature may vary considerably during prevalence of fever, and that nearly the same temperature ma with a great variation in the number of cases. Thus, in Au number of cases is 5261, and in December the number of cases being a difference only of 98; but the mean temperature of t mentioned month is 61°; while that of December is only 3 quantity of rain, however, in both of these months is above the In March the mean temperature is 43° 9, and the number of case while in November the mean temperature is 42°, and the nu cases 5054; but the quantity of rain in March is 1° 39, while in ber it is 2°79, being double the amount of that which falls in t mentioned month. In February the mean temperature is 38°, number of cases 2825; while in December the mean temperatur aud the number of cases 5359; but the quantity of rain in the these months is 1·49 inches, while in December it is 2·58 inches

The conclusions which may be drawn from this table, are, th the months in which the quantity of rain is above the average, fe vails to a greater extent than in those months in which it is be point. It does not appear, however, from it that the average temperature of this climate has much influence on the preval

* Edinburgh Medical and Surgical Journal. July, 1838. p. 100.

fever; for if moisture be present, it may prevail to about the same extent, when the average temperature is 61°, as in August, or when it is 39°, as in December.

The diffusion of fever is thus generally connected with humidity of the atmosphere; yet certainly there are other causes of a more influential kind that are also in operation. This is well exemplified by the two tables (page 52), which show the prevalence of fever and the corresponding weather in Glasgow during the years 1836 and 1837. Thus, although in both of these years the quantity of rain was greater than the average, and in the first of them greater than in the second, yet the number affected with fever during 1836 amounted only to about the half of those that were seized during 1837. The increased prevalence of the epidemic during 1837, must, to a very considerable extent, have depended upon the scarcity of provisions, want of fuel, &c., and their concomitants, filth, &c., which followed the commercial embarrassments of that year.

An accumulation of persons not previously affected tends to diffuse typhus. There is also another important point connected with the history of eruptive typhus, which has seldom been taken into calculation in attempting to account for its diffusion, namely, that it does not often attack the same person more than once during his life. Now if this be admitted,—and we have endeavoured to show at page 16, the analogy between typhus and other exanthematous fevers in this particular, but even though M. Hildenbrand's modified view only be granted, namely, that it secures the person who has been affected only for some years—it follows:

1st. That after an epidemic fever has prevailed for some time, it must cease after the lapse of a particular period, from deficiency of *material* to act upon; and the history of almost every pestilence of this kind, shows that it rarely exceeds two years in duration, even in a large city.

2d. That, though fever may constantly exist in a large town, in a minimum proportion, varying in numbers according to the habits of the people, those who have never laboured under the disease are gradually accumulating; and that when the state of the atmosphere, as to humidity, and the scarcity of provisions, with their consequents, filth and deficient ventilation, are concurrent with this accumulation of susceptible individuals, that fever has rarely failed to spread among the community. And if the population of any large city be increasing very rapidly, such as that of Glasgow, at the rate of ten thousand persons annually, the number of susceptible individuals will be accumulated in a few years to an amount, sufficient for the existence of an extensive epidemic.

3d. That a severe epidemic fever of one or two years' duration is never succeeded by another until several years have elapsed.

A very important enquiry may be deduced from the foregoing statements. Can the very rapid increase of the population of Glasgow account, to a greater or less extent, for its being visited for a series of past years with more frequently recurring epidemics of fever than any other city of Great Britain, similar in size and population? Can the influx of several thousands of unprotected individuals from the country every year afford any explanation of the occurrence; just as there is always a great

mortality from smallpox in Glasgow, from the influx of unvaccinat Highlanders, while in many other cities of the kingdom this disease comparatively rare? There can be no doubt that the influx of so ma strangers to this city must have a powerful effect in increasing the nu ber of fever patients; but certainly the filthy and irregular habits of working population are equally operative as predisponents to contagi Is there then any prophylactic measure which may either ward off diminish the extent of an epidemic diffusion of fever in a large city; o is this beyond the control of human means and calculations? We think not; although we do not entertain the notion that fever will ever be completely extinguished in any large manufacturing town; or that the spread of it, epidemically, can be checked *in limine*, when the concurrent circumstances are favorable for its propagation; but certainly much might be done to lessen the intensity of the evil. It has already been shown that filth and deficient ventilation tend much to spread the contagion of typhus, being almost constant concomitants; and that while it generally affects the whole members, or the large proportion of a family, among the lower orders, it rarely spreads in this manner among the better classes of society, who attend more to cleanliness and ventilation. It is quite obvious that an amelioration of the physical condition of the lower orders, in these particulars, would, in proportion as this was effected, diminish their chances of catching the contagion; which would not only operate in lessening directly its diffusion, but, by reducing the number of its sources, must tend to lessen the actual quantity of this principle that might be generated in a given time.

But can this amelioration be effected to any appreciable extent; or if effected, could it be maintained for any length of time? We fear that little permanent amelioration could be effected without a legislative enactment; for though our philanthropists are very active in their charities during the prevalence of an epidemic, it no sooner subsides than they relapse into a comparative quiescence, and our working population into their former habits of filth and intemperance. And the evil will continue to assail us so long as our cities contain so many narrow and filthy lanes, so long as the houses situated there are little better than dens or hovels, so long as dunghills and other nuisances are allowed to accumulate in their vicinity, so long as these hovels are crowded with inmates, and so long as there is so much poverty and destitution. Why, then, should we not have a legislative enactment that would level these hovels to the ground, that would regulate the width of every street, that would regulate the ventilation of every dwelling-house, that would prevent the lodging-houses of the poor from being crowded with human beings, and that would provide for their destitution? It may be said, that this would interfere too much with the liberty of the subject, and no doubt it would be vehemently opposed by many interested persons. In place, however, of being an infringement on the liberty of the subject, it might rather be designated an attempt to prevent the improper liberties of the subject; for what right, moral or constitutional, has any man to form streets, construct houses, and crowd them with human beings, so as to deteriorate health and shorten life because he finds it profitable to do so? As well ought the law to tolerate the sale of unwholesome food, because it might be profitable to the retailer of it.

CHAPTER III.

STANCES WHICH TEND TO RENDER FEVERS COMMUNICABLE FROM ONE PERSON TO ANOTHER.

quite obvious, if the doctrine of contagion be admitted, that all ircumstances which favour the diffusion of fevers tend also to hem communicable from one person to another; it is therefore ry to include them in the following arrangement, although it nly necessary to illustrate one of them a little farther, namely, uence of filth and deficient ventilation.

imstances which tend to render fevers communicable:

1. Humidity of the atmosphere.
2. Scarcity of provisions, &c.
3. No previous affection with typhus.
4. Filth and deficient ventilation.
5. Age.
6. Acclimatization.
7. Idiosyncrasy of constitution.

ed circumstances which tend to render fevers communicable:

1. Weakness of constitution.
2. Greater susceptibility of females.
3. Depressing passions.
4. Intemperance.

ed exemptions from fever:

1. From trade or occupation.
2. From chronic diseases.

different points shall be considered in the following part of the hough not exactly in the order enumerated, as this might derange eral connexion of the observations.

ence of filth and deficient ventilation. In a previous part of the e entered into the consideration of filth and deficient ventilation ng very powerfully to spread the contagion of typhus; and showed iere it was concentrated, as in crowded hospitals, or in the small and lated houses of the lower classes, it rarely failed to be communi- o the unprotected attendants or inmates of a family. Filthiness nal habits, however, although it tends to render it more commu- , as we shall endeavour to show from the statistics of the Glasgow lospital, does not seem to act so powerfully in this respect as defi- entilation, which by concentrating the contagion may render its n on the system more certain. In proof of which we may quote ous attendants of our fever hospitals, who are generally very atten- cleanliness in their persons, and yet, if unprotected, are almost ly affected with fever during some period of their attendance, if ds be in a crowded state. This fact, and the more frequent ex- of the attendants when the wards are moderately filled and well ed, seem to prove that contact with the patient is not so essential communication of the disease, as being surrounded by an at- re highly impregnated with the contagious miasmata. And there ny instances where students have been affected with fever after

visiting the wards of an hospital, without having come into contact with the patients or their bed-clothes. There can be no doubt, however, that simple contact of a typhus patient, or of clothes that have been attached to him in any shape, may communicate the disease without the aid of even a partial impregnation of the atmosphere with contagious effluvia, and where the most perfect ventilation has been maintained.

We are at present unacquainted with the channel by which the contagion of typhus most generally enters the body; and though the opinion be generally entertained that the lungs are the organs through which it passes into the system, yet it is equally probable and consistent with analogy and facts to believe, that the skin is, at least, as important as a medium of communication. There are many animal poisons that operate on the system through the skin very powerfully, and yet have little effect when applied to the mucous membrane of the stomach or intestines; now though, in the one case, the poison operated with be ponderable and be applied to the mucous surface of the stomach and bowels, while, in the other case, the contagion of typhus is imponderable, and is applied to the mucous surface of the bronchial tubes, yet, in the absence of direct experiment, this analogy is entitled to some consideration.

The following tables will tend to show that filthiness in personal habits is very frequently connected with the production of typhus, and it includes all the cases of fever that were admitted into the Glasgow Fever Hospital from May 1st to November 1st, 1839, in whom their state as to cleanliness or filthiness was ascertained:*

	FILTHY.		CLEAN.	
	Males.	Females.	Males.	Females.
Scotch..................	92	81	64	93
Irish.....................	88	70	60	47
English..................	4	2	3	3
West Indies and North America...............	3			1
	187	153	127	144
	Total filthy...340 cases.		Total clean...271 cases.	

The following table shows the number of cases that were filthy and those that were clean in typhus characterized by the eruption, and also the proportions, regarding this point, which were ascertained in febricula:

	FILTHY.		CLEAN.	
	Males.	Females.	Males.	Females.
Eruptive typhus	133	112	73	77
Febricula.........	6	8	19	15
Total no. of cases	of febricula, filthy......14		Total no. of cases of febricula, clean..34	
—	of typhus, —......245		— of typhus,	—...150

* The reports, respecting the clean or filthy state of the patients admitted, were taken by the barber of the Fever Hospital, and afterwards transferred by the author, along with other statistical facts, into his own journal.

These two tables show that among 611 cases admitted as continued fever, there were 340 filthy and 271 clean, or about 55 per cent. filthy; that among 395 cases of eruptive typhus, there were 245 filthy and 150 clean, or about 62 per cent. filthy; and that, among 48 cases of febricula, there were 14 filthy and 34 clean, or about 29 per cent. filthy. The following deductions may be drawn from these facts. 1. That the proportion of filthy persons is greater than that of the clean among the whole number of cases admitted, and including not only typhus, but bronchitis, febricula, pneumonia, and several other affections which are specified in a table already given. 2. That among the eruptive or decided cases of typhus, the proportion of the filthy to the clean is still greater than what exists among the whole number of cases. 3. That among the cases of febricula, the proportion of filthy persons is only 29 per cent., while in eruptive typhus it is 62 per cent. 4. That, as the proportion of filthy persons in the whole number of cases is less than in those affected with eruptive typhus, it is fair to infer that this is owing to an admixture, with the latter, of febriculous, bronchitic cases, &c., since it has been shown that filthiness is much less frequently a concomitant of febricula, &c., than it is of eruptive typhus.

We are entirely ignorant of the nature of those substances which absorb the typhus contagion with most facility; but, as filth is very frequently a concomitant of its ready communicability, it may be assumed that either the clothes or the deposits on the skin of filthy persons have a tendency to absorb the contagion and to retain it until the system become affected. We know that certain gases, and even odours and fetid effluvia, are absorbed more readily by some substances than by others; and though we are only warranted to assume from this analogy, that typhus contagion is very probably regulated by a similar law, yet, on the other hand, if want of cleanliness facilitate the operation of contagion on the system, it is not possible to explain this effect on the principle of pulmonary inhalation, while the theory of cutaneous absorption is not opposed by any fact or analogy. If this view be adopted, it will obviously lead to a prophylactic measure of considerable importance, namely, the daily and thorough ablution of the skin, and the frequent changing of the wearing apparel, for it is not probable that the contagion will be absorbed immediately after its application to the clothes or skin of the person who has been exposed to it; and by the daily ablution of the whole body, it may be removed before this can occur.

Influence of idiosyncrasy of constitution. The contagion of typhus is not communicable to all persons with the same facility. Some individuals are infected after the first exposure, while others may be exposed for weeks, or even many months, almost constantly, before they are attacked. It may be said, however, that in this last case the contagion has remained for a longer time latent than in the former. There is no very precise evidence existing as to this point, and the opinions entertained by many authors are often conjectural. Dr. Bancroft states as follows: "It results, therefore, from this statement, that among the ninety-nine orderlies and nurses who had probably not been exposed to the contagion before their attendance on the sick commenced, the earliest attack was on the 13th day, and the latest on the 68th; but these returns were made up about the 20th of April, and it appears that some

who had escaped till that time were afterwards attacked; and therefore, though there may be reason to conclude that febrile contagion does not remain inactive so long after being received into the body as marsh miasmata, I see none for believing that an interval of five or six months may not sometimes elapse before the actual production of fever by it."* Dr. Perry is of opinion, that "the earliest period of the disease making its appearance after exposure to contagion is eight days, more frequently fourteen, and sometimes so long as two months."† What the circumstances are which render some persons, who enjoy good health, are well fed and cleanly in their personal habits, more susceptible of contagion at one period than at another are totally unknown; but we are in the same state of ignorance as to the reason why scarlet fever, &c., may be caught at one period and not at another, and why vaccination frequently succeeds at last after five or six unsuccessful trials. Again, a certain proportion of persons appear not to be susceptible of the disease. Dr. Perry is the only author that we are acquainted with that enumerates the proportions of susceptible and non-susceptible individuals. His 10th proposition is the following: "That between the ages of seven and fifty, sixteen out of twenty are susceptible of being affected with contagious typhus, if exposed to the contagion, and not protected by having previously had the disease."‡

That there is a certain proportion of individuals who are not susceptible of typhus contagion there can be no doubt, for there are many medical practitioners and nurses of fever-hospitals who have never laboured under the disease, although they have been exposed to its influence for many years, but there is no proof that in the present state of our fever statistics we can define the proportion of unsusceptible persons. It is a common opinion that constant exposure lessens the susceptibility to fever; but this, in the present state of our knowledge, can only be considered as a probable hypothesis.

Influence of sex. Hildenbrand is of opinion that delicate men, who have a fine skin and feeble bodies, are most subject to contagion; while, on the contrary, those that are robust, plethoric, vigorous, and well nourished, more seldom contract it. These opinions are entertained by several writers on fever; and for a similar reason, it is sometimes concluded that females are more subject to this disease than males. It is natural for an author, who advocates the absorption of contagion by the skin, such as Hildenbrand, to infer that a fine skin, like that of the female, will absorb more readily than one which is coarse; and, although this theory be supported by the statistics of some hospitals, it is opposed by those of others. The number of admissions into the Glasgow Fever Hospital during the year 1836 were 1116 males and 1141 females,‖ which is only a small excess of females; but if the excess of the female over the male population of Glasgow be taken into the account as about one sixth, the proportion of males that have been affected with fever will be plus instead of minus. In the same institution were admitted, from May 1st to November 1st, 1839, 270 males and 276 females, classified under typhus.

* Bancroft on Yellow Fever, &c. p. 516.
† Edinb. Med. and Surgical Journal, vol. xlv. p. 69.
‡ Ibid. p. 67.
‖ Cowan's Vital Statistics, p. 19.

into the Cork-street Fever Hospital, Dublin, from 5th January, 1817, to 30th April, 1818, there were admitted 2883 males and 2849 females, which is a small excess of males.* Again, in other hospitals, there has occurred an excess of females. There were admitted into the Waterford Hospital 1277 males and 1452 females,† into the London Fever Hospital 1229 males and 1308 females,‡ into the Limerick Fever Hospital 1332 males and 1895 females, being a large excess of females,§ and into the Edinburgh Royal Infirmary 962 males and 1075 females.‖ The facts which have been hitherto published regarding the susceptibility of the different sexes to fever are not yet sufficiently extended to warrant us drawing any certain conclusion from them; but certainly it does not appear to be established by satisfactory evidence, that the one sex is more liable to the disease than the other; and, where this does occur in any particular place, that it cannot be accounted for by the general excess of female population in large cities, or by other circumstances connected with their history. Drs. Barker and Cheyne remark, that "in Dublin, when the epidemic had completely established itself, the males admitted to hospital were most numerous, but in its progress the admissions of females exceeded those of males.... As to the comparative frequency of fever in the male and female sex in the country at large, we can form no decisive opinion, the answers to our enquiries on that head not having been perfectly satisfactory."¶

Although the comparative frequency of fever among the sexes has not been accurately determined, it has been proved satisfactorily by the statistics of almost every large hospital, that a larger per cent. of males than of female patients die of the disease; and it is proved by the Glasgow Mortality Bills, that a much greater number of the male than of the female population of that city are carried off by it. Thus, in Glasgow, during the year 1836, 465 males and 376 females died of fever; during 1837, 1187 males and 993 females; and during 1838, 439 males and 377 females.** If the average mortality of each sex could be accurately ascertained, this large amount of deaths might be made available for determining the liability of the different sexes to fever, by the same method of approximation which Dr. Cowan has adopted in calculating the amount of fever in Glasgow during the years 1836 and 1837;†† but as the proportionate mortality of the different sexes is not the same during every season, and as it may not be the same among those treated at home as in those treated in hospitals, this method, although well adapted for giving a general approximation, is not well calculated for determining a nice question of this kind.

Influence of delicacy, or weakness of constitution. We have already remarked, that it is a prevalent opinion among medical men, that persons naturally weak and delicate are more liable to fever than those

* Barker and Cheyne on Fever, vol. i. p. 91. † Ib. p. 193.

‡ Dr. S. Smith's Treatise on Fever, p. 432.

§ Dr. Geary's Report, Dublin Journ. of Med. Science, vol. xii. p. 10.

‖ Edinb. Med. and Surgical Journal, Oct. 1839, p. 448.

¶ Barker and Cheyne on Fever, pp. 89-90.

** Glasgow Bills of Mortality for 1836, 1837, and 1838.

†† Dr. Cowan estimated the proportion of the whole mortality in 1837 as 1 in every 10 patients; and to determine the amount of cases, multiplied the whole number of deaths, which were 2180 by 10 = 21·800.

who are healthy and vigorous. This opinion seems to be as little c ble of proof as the preceding one regarding the greater liabilit females to the disease. We are not, however, in possession of evidence, and none statistical, so far as we are aware, regarding this p beyond the loose and general observations of authors.

We have kept a record of the physical habit of the patients admi into the Glasgow Fever Hospital from May 1st to November 1st, 18 and the following were the divisions adopted :

1. Moderate, by which is meant a person having an ordinary quan of muscle and cellular substance.
2. Full or plethoric, having an extra quantity of adipose texture of blood.
3. Muscular.
4. Spare.
5. Emaciated or unhealthy in appearance.

	Males.	Females.	Total.
Moderate	116	93	209
Full or plethoric	28	73	101
Muscular	44	...	44
Spare	24	41	65
Unhealthy or emaciated	2	8	10
			429

The whole of these 429 cases were characterized by the typhoid er tion, and will therefore be considered as decided cases of typhus. appears from this table, that there were only 10 cases in an emaci or unhealthy condition; and almost all of them, as far as could be as tained, were engaged in their ordinary occupations at the time of t seizure. The spare and unhealthy, when added together, only f about 17 per cent. of the whole number.

Influence of chronic diseases. The evidence, such as we have lected from the previous history of patients admitted into the Glas Fever Hospital; and from post-mortem examinations, seems to p that persons affected with any particular chronic disease of the ches belly, are very rarely affected with typhus fever. Hildenbrand st that phthisical persons are very rarely affected with typhus fever; that, out of many hundred cases of this disease that he has treated, one instance of a phthisical person has occurred. We have heard same opinion expressed by several physicians of extensive hospital e rience, and that they have scarcely ever met with a case of tubercle a person who has died of eruptive typhus.

This opinion we can nearly confirm from our own experience, for of more than 100 post-mortem inspections we have met with only t cases; and the number of tubercles in each did not exceed three, w were small and only partially softened.

Influence of fear and the depressing passions. The influence of and the depressing passions has also been considered as very pow in predisposing persons to be affected with typhus contagion. T can be no doubt that fear has a tendency to produce a temporary

pression of the physical powers; but, as has been already shown, there is no proof that persons of a naturally spare or weak habit of body, who are generally very sensitive, are more liable to fever than those of an ordinary constitution, this opinion must also be considered hypothetical. Indeed, the facts, as far as our enquiries have enabled us to judge, seem to prove that the apprehension of fever, more particularly when it is not epidemic, is very rarely felt until the person is actually seized with the disease; for some cannot recollect of a single circumstance by which they could be exposed to contagion; and a considerable number of those who had undoubtedly been exposed to it, were only made aware of the fact when it had been elicited by cross-examination. We are quite aware that cases may be brought forward, of sensitive individuals who have been seized with fever soon after visiting a person labouring under the disease; but as this fact can be opposed with at least an equal number of persons who were destitute of fear, and yet caught it after an exposure to contagion, no conclusion whatever can be drawn from them. It must be observed, however, that though there is no proof that persons who are naturally weak in body or of a sensitive disposition are more susceptible of fever than those who are naturally vigorous and robust, yet that, during famine or commercial distress, poverty by depressing the mind and lowering the physical status from insufficient aliment, does powerfully predispose a community to become affected with fever. This has been already shown in a former part of the essay; and has been again alluded to, in order that the distinction might be made between an individual of naturally weak mental and physical stamina, and one who has been reduced to that state by deficient nutriment.

Influence of intemperance. It is a question of vital importance to the inhabitants of large towns, whether intemperance predisposes those who indulge in it to be affected with fever. A solution of this point in a satisfactory manner cannot, we are afraid, be made from our present data; for no statistics regarding it have been published. Indeed, it is sometimes very difficult, even after the most careful enquiries, to find out the habits of patients who are sent to an hospital; for most of them are ashamed to acknowledge intemperance when it does exist, and those who admit that they indulge a little are sometimes more abstemious, in point of quantity, than those who deny any indulgence whatever. The ascertaining of such habits, accurately, is in many cases impossible, and the evidence must be viewed principally as an approximation to the truth. At the same time, this approximation may be often rendered very convincing, by sifting the answers of the patients, by an attentive examination of their appearance, and by the evidence of friends; and occasionally conclusions confirmatory of the opinion formed may be drawn from their trade or occupation; for it is well known, that in some occupations the majority of the workmen are addicted to excessive drinking. The frequent combination of drunkenness with filthiness of personal habits is another circumstance which complicates this question very materially, and renders the appreciation of the value of each a matter of some difficulty. The following table shows the proportion of temperate and intemperate individuals, that were admitted into the Glasgow Fever Hospital from November 1st, 1838, to November 1st, 1839, whose habits

could be ascertained with more or less certainty; and the eruptive cases are only included.

	Temperate.	A little Intemperate.	Intemperate.
E. Typhus (MALES)	125	51	73
E. Typhus (FEMALES)	76	8	30

In this table the proportion of intemperate males is much greater than that of the females. Can this circumstance account, to a greater or less extent, for the greater mortality of the former in almost all hospitals? It would be natural for a person, who wished a certain theory supported, to conclude that as such a large number of those affected with fever were reported to be more or less intemperate, this could not be an accidental and uninfluential concatenation; but that the two circumstances must stand to one another, in the relation of cause and effect. It would be necessary, however, before such an inference could be drawn, to ascertain whether the proportionate amount of the intemperate to the sober was greater in the cases of fever than what existed among the community from whom they were sent. We fear that this question cannot be determined; for the prevalence of intemperance among the working population of large cities has been calculated principally from the amount of drunkards that appear on our streets, from the large and increasing number of our spirit shops, and from the enormous quantity of ardent spirits consumed in a year. And though there can be no doubt that drunkenness has increased among the lower classes to a lamentable extent, its numerical amount has never been ascertained, and perhaps never can be accurately ascertained; but certainly there are grounds for believing that the proportion enumerated as intemperate, in the table which has been given, is not greater than what really exists among the inferior grades of our working population. A similar opinion is entertained by Chomel, who states that alcoholic excesses appear to exert no influence on the production of the typhoid fever. Intemperance, however, tends indirectly to predispose the system to contagion, by the production of filthy habits. It also exercises a most powerful influence in increasing the mortality from fever. In the Glasgow Fever Hospital there occurred eighty-one deaths from eruptive typhus in individuals whose habits were ascertained, and thirty-four of these were reported as intemperate, nineteen a little intemperate, and twenty-eight temperate. In Dr. Craigie's table of the deaths in thirty-one fever cases that occurred in the Edinburgh Royal Infirmary, there are fifteen stated to be irregular or dissipated, only two regular, the habits of the remainder are not stated.*

It is also a singular fact, which has been noticed by several writers, that fever is more fatal among the higher than among the lower classes. Dr. Braken states, in reference to the fever which prevailed at Waterford during the years 1817-18-19, that "it would be difficult to adjust the rates of mortality in the upper classes, but it seems probable that one fourth or perhaps one third of all those persons who were attacked with fever fell victims to its power."†

Drs. Barker and Cheyne, in their historical account of the Irish epi-

* Edinburgh Medical and Surgical Journal, vol. xlvii. p. 296.

† Barker and Cheyne on Fever, vol. i. p. 277.

demic, state that "in every part of the country fever was reported to have been much more fatal amongst the upper than the lower classes."* To what is this difference of mortality, so generally remarked by experienced hospital physicians, to be attributed? and which in Ireland seemed to be very remarkable, namely, in the lower classes about one in twenty-three cases, and among the upper classes one in three or four generally, but in other places about one in seven. Can the difference in the mode of living account for this anomaly? as the first live very much on potatoes, while the other use a larger or smaller proportion of animal food; and the lower classes almost everywhere in this country use less animal food and stimulating dishes than those who are more wealthy and in a higher sphere of society.

This subject is highly worthy of farther investigation; for the difference of mortality which exists among these different classes most probably depends more upon some cause connected with their habits and kind of aliment than upon their *morale*.

Influence of age. Almost all modern authors who have written on fever statistically state that the susceptibility to this disease is greater among young persons than among the old; and there is sufficient evidence brought forward to establish this; but certainly the conclusions which have been drawn respecting the greater liability of one period of youth when compared with another have not been satisfactorily proved.

From an examination of the ages of 117 patients, and by comparing his table with the results obtained by M. Louis and some other observers, M. Chomel thinks it may be established that the most common period of life for attacks of typhoid fever is from the eighteenth to the thirtieth year; that it is rarely observed beyond forty years; and that perhaps no case has yet been observed where the patient was beyond fifty-five years.†

Dr. Cowan states that, "from an examination of these (his) tables, it appears that the period of life at which fever is most liable to occur is from the age of twenty to twenty-five years for the males, when the proportion is 21·23 per cent., and from the age of fifteen to twenty for females, when the proportion is 23·83 per cent."‡ Dr. Geary, in his report of the Limerick Fever Hospital states, "that children are much more liable to fever than is generally supposed, and to the little apprehensiveness of disease being transmitted by them may be attributed the spread of disease through families in many instances. It will be seen underneath that nearly one sixth of the admissions for 1836 were under ten years of age, a fact which bears out what we have stated, and is also a satisfactory proof of the increasing confidence which public hospitals are acquiring from the community. . . . . Of the entire treated for the year, full two thirds were under twenty years of age.§ We have selected the statistics of these two last-mentioned authors chiefly on account of the large number of cases from which their conclusions have been drawn—the first having treated 2257, and the second 3227—in order to show the fallacy of the principles by which the susceptibilities

* Barker and Cheyne on Fever, vol. i. p. 95.
† Chomel, Clinique Médicale, vol. i. p. 311.
‡ Cowan's Vital Statistics of Glasgow, p. 20.
§ Dublin Journal of Medical Science, vol. xii. pp. 98-9.

of persons to fever at the various periods of life are estimated. I obvious that the proportionate number of cases at the various a given by the above authors is only that which exists in an hospital; it by no means follows that the same ratio will be maintained among t general community.

Before any such inference could be drawn, evidence must be broug forward to prove that the admissions of cases into hospitals were in t same proportion as to ages as that which existed among the populatio from whom they were sent; for it is well known that children in man towns are not so frequently sent to hospitals as adults. And this circum stance may perhaps account for the discrepancy which exists betwee the conclusions of Dr. Cowan and those of Dr. Geary. This metho however, even though it were ascertained that the same proportional number of cases affected with fever was admitted into hospitals at th various ages, is very unsatisfactory, as has been pointed out by D Arthur Thomson; for it does not show the number of persons living each period of life, so that an estimate may be formed of the proportio which the number living at each term of life bears to those who hav been attacked. In order to supply this deficiency, the author we hav already quoted gives the following table:

"TABLE IV. *showing the estimated number in the inhabitants at Glasgow at ea age during the year* 1836; *the number attacked by fever, together with the rat attacked out of every thousand at each decennial period of life.*

Ages.	No. of inhabitants at each age.	No. attacked by fever.	Ratio per 1000 attacked by fever.
Under 10	67·469	3811	56
10 to 20	50·009	1539	30
20 to 30	46·275	1611	34
30 to 40	32·044	911	28
40 to 50	21·758	392	17
50 to 60	14·090	294	20

"It appears from this table that the greatest susceptibility to fever oc curred under ten years of age, after which fever occurs most frequent among persons between the age of twenty and thirty. The number a tacked after the age of thirty decreases gradually as life advances."*

This method of calculating the susceptibilities to fever is certainly su perior to that which is deduced from the admissions into hospitals; but is attended with the following objections, which must tend to lessen t accuracy of the conclusions:

1st. The number of fever cases stated in the table is not the result actual observation, but is calculated from the rate of mortality which o curred at the various terms of life in a fever hospital, on the same pri ciple that Dr. Cowan endeavoured to ascertain the amount of fever Glasgow, and which is explained at page 62; consequently, the dedu tion is only an approximation to the truth.

2d. The diseases of which persons die in Glasgow are reported by th friends and not by their medical attendants; and though we acknowled

* Edinburgh Medical and Surgical Journal, July, 1838, p. 92.

ralue and utility of the mortality bills, even upon this imperfect ainly errors respecting diseases which are sometimes difficult aish from others must frequently take place. This is parhe case with fever in childhood; which is not so easily recognallpox, measles, and scarlet fever, and which is frequently d with hydrocephalus, teething, derangements of the chylocera, bronchitis, &c.

t be admitted that typhus does not frequently attack individuals once in their lives, or even upon the principle of its protectonly for a certain number of years, it follows that there must er number secured by a previous attack among those at the nced periods of life than among those who are young. This not been prominently alluded to, so far as we are aware, in us account of the disease; but in calculating the susceptipersons to fever, those who have previously undergone the at least a portion of them, ought to be deducted from the pulation.* This subject must, therefore, be considered as not r investigated; and perhaps will remain so until there be some enactment compelling medical practitioners to make a return diseases which have been treated by them throughout the year. ervations of the British and Irish physicians do not agree with [. Chomel, as to the maximum and minimum period of life, ich persons are not susceptible of typhus or the typhoid fever. entioned author thinks it very rarely occurs below ten years d that perhaps no case has occurred where the patient was befive years. Into the Glasgow Fever Hospital there were adring the year 1836, 2257 cases of fever; and out of this numvere 41 under five years of age, and 3 between seventy and e years.† Into the Limerick Fever Hospital, during the year e were admitted 3227 cases of fever, and there were 81 below of age and 10 between sixty-five and seventy years.‡ Dr. eated in the Edinburgh Royal Infirmary 7 cases of fever bey and seventy years, among 343 admissions.§

e met with, in the Glasgow Fever Hospital, 5 cases of eruptive children reported to be three years of age, from the 1st May to ber, 1839.

tization. M. Chomel and some other French authors state phoid fever attacks most readily those who have been only a in Paris, while those who are natives of that city are more exempted. He mentions that among 92 individuals, 64, that nore than two thirds, had lived in Paris, less than two years, ly were natives and residents. The small number of those who and resided in Paris is certainly remarkable; at the same time kept in mind that no patient was admitted into his wards be years of age.

e constructed the following table in order to illustrate this part

wan calculates that about 38,000 persons were affected with fevers in Glasgow ears 1835, 1836, 1837.

's Vital Statistics of Glasgow, p. 20.

Journal of Medical Science, vol. xii. p. 99.

gh Medical and Surgical Journal, vol. xlvi. p. 35, and vol. xlvii. p. 329.

of the subject; and it comprehends 568 eruptive cases, which wer into the Glasgow Fever Hospital from November 1st, 1838, to 1st, 1839. It shows the number of patients born in Glasgow, ber of strangers, and the duration of their residence in Glasgo

	Males.	Females.	
Natives of Glasgow . . .	77	99	
Strangers resident from 1 to 14 days	12	4	
2 weeks to 1 month . .	7	6	
1 to 2 months	10	14	
2 to 3 months . . .	10	8	
3 to 4 months	5	5	
4 to 5 months . . .	5	3	
5 to 6 months	9	12	
6 months to 1 year . .	29	26	
1 to 2 years	24	17	
2 to 3 years	13	10	
3 to 4 years	6	11	
4 to 5 years	12	4	
5 to 10 years	29	32	
10 to 20 years and upwards .	36	33	
	284	284	

It appears from this table that among 568 eruptive cases of whom this point was ascertained, 176 were natives of Glasgow were strangers: 206 of these strangers had resided in Gla from one day to two years, and 186 from two to twenty year wards. The strangers amount to about 69 per cent. of the wl ber of cases; and those who were affected within two year residence in Glasgow to about 52 per cent. of the whole r strangers.

The following deductions may be drawn from these facts: strangers are more liable to become infected with typhus fever tive residents. 2. That the majority of strangers are infecte comparatively short period of their residence in Glasgow. minor proportion of the strangers, like the natives of Glas escape infection for many years, and yet be afterwards attacke results support the views which we have elsewhere given of th typhus.

Most of the strangers come from country districts, in whi be fairly presumed that typhus does not constantly exist, as large towns; it is therefore probable that the majority of the protected by any previous attack; for if typhus attack an many times during his life, why should the natives of a town c 263,000 inhabitants, who are constantly within the sphere of c bear so small a proportion to the strangers.

The facts connected with the propagation of smallpox in Gl of a very similar kind; for the majority of the unvaccinated pe are sent to the Fever Hospital are Highlanders, who have com cently from a district where this disease is not in operation, consequently have not previously been exposed to contagion.

Influence of trade or occupation. Little is known accurately as to he operation of the different trades, in increasing or diminishing the susceptibility to fever. In manufacturing towns there are a greater number of persons connected with cotton manufactures affected with fever than other operatives; but this may be expected; because they generally in such places constitute the most numerous class among the general population. Again, in other towns, labourers are the most numerous class who are affected with fever. Dr. Geary, Physician to the Limerick Hospital, states that "we have a tabular view before us, which shows the number in families of each class of 2416 persons admitted from the city parishes, and the proportion they bear to each other; though the exact relation to the general population cannot be determined, as there is considerable difficulty in ascertaining the amount of each trade. However, as may be expected, the labouring class being the most numerous, constitute the largest number, averaging one half of the entire; and including all, we find that more than one half of those treated for the year cannot be said to be of any trade, namely, females and children."*

It is an ancient opinion that tallow-chandlers, butchers, tanners, and water-carriers are rarely affected with plague or fever. Dr. Hancock quotes the following evidence in reference to the trades that were exempted from the plague. "Volney tells us that at Cairo it is observed that water-carriers, continually wet with the fresh water they carry in skins upon their backs, are never subject to the plague. This fact coincides with the observations in London. George Baldwin, consul-general in Egypt, says that among upwards of a million of inhabitants carried off by the plague in Upper and Lower Egypt, during four years, he could not learn that a single oilman or dealer in oil had suffered. Jackson, in his reflections on the commerce of the Mediterranean, likewise informs us, that in the kingdom of Tunis, there never was known an instance of any of the coolies or porters who work in the oil stores being in the least affected by the disorder; their bodies being always well smeared with oil, as well as their clothes being imbued with it. We are told by Fonseca, that all the tanners at Rome escaped the plague; and Hildererus and Schenck make a similar observation. Dr. Maclean refers to the exemption of tanners at Cairo."† Dr. Tweedie notices the exemption of butchers from fever, and states that though almost every description of mechanics was admitted during the year into the London Fever Hospital, he did not recollect of a single instance of a butcher.‡ Other physicians, however, have met with patients who followed this occupation. Dr. Southwood Smith, in his table of the occupations of 679 patients affected with fever, enumerates three butchers, two curriers, and two skinners.§ Dr. Craigie, in his table of 181 cases of fever treated in

* Dublin Journal of Medical Science, vol. xii. p. 103.
† Hancock on Pestilence, p. 184.
‡ Tweedie's Clinical Illustrations of Fever, p. 79.
§ Southwood Smith's Treatise on Fever, p. 431.

the Edinburgh Royal Infirmary, mentions three butchers among t
number.*

The following tables show the various trades, occupations, &c. of 5
patients admitted into the Glasow Fever Hospital from November 1
1838, to November 1st, 1839. They include all the eruptive cases
typhus in which the occupation, &c. were ascertained.

MALES.

Bricklayer	1	Fisherman	1	Plasterer	1
Brushmaker	1	French-polisher	1	Pensioner	1
Brickmakers	2	Glass-cutters	3	Printers	2
Blacksmiths	9	Glass-blowers	3	Quill-dresser	1
Bakers	4	Gasmaker	1	Quarriers	2
Currier	1	Gardener	1	Ropemaker	1
Confectioner	1	Ham-curer	1	Schoolmaster	1
Collier	1	Hawkers	5	Lawyer	1
Cooper	1	Joiners	6	Showman	1
Cabinet-makers	3	Labourers	76	Shoemakers	11
Carters	4	Last-maker	1	Sailors	6
Carpenters	3	Malsters	3	Factory-workers	22
Candle-maker	1	Masons	6	Servants	4
Clerks	2	Milk-dealer	1	Slaters	3
Coffee-roaster	1	Optician	1	Tailors	7
Dyers	3	Nailers	4	Tinsmith	1
Engineers	7	Policeman	1	Turner	1
Engineman	1	Porters	4	Tobacconist	1
Firemen	3	Painters	3	Wireworkers	2
Founders	4	Potters	3	Weavers	63
		Watchman	1	Warehouseman	1
	53	+	126	+	133=312

FEMALES.

Weavers	11	Servants	38	Hawkers	6
Factory-workers	77	Fruit-dealers	2	Bark-peeler	1
Sewers	25	Washerwomen	2	Stocking-knitter	1
Beggar	1	Winders of Yarn	3	Straw hat-maker	1
Shearers	3	Calico-printers	2		
Married	97	Nurses in F. Hosp.	4		
	214	+	51	+	9=27

Total of Males and Females =58

Influence of Pregnancy. Among 172 females admitted from May
to November 1st, 1839, there were fourteen pregnant, being about 8
cent. of the whole, and fully three fourths of this number had abor
or premature labour during the course of the disease. This appears a c
siderable number; but in the present state of our knowledge respect
this point, we are only entitled to conclude from it that pregnanc
not an operative circumstance in preventing the communication
typhus, and this opinion is corroborated by the *general experience*
practitioners. Unless there existed a correct enumeration of the nu
ber of individuals belonging to each occupation in Glasgow, no pa

* Edinb. Med. and Surgical Journal, vol. xlvii. p. 286.

cular deduction could be drawn from these tables; but certainly it is worthy of remark that there should be no butcher,* no tanner, only one currier, only six masons, and one bricklayer, who together are a very numerous class of operatives in Glasgow, while there are seventy-six labourers. The latter class of operatives are generally filthy in their habits and live in small ill-ventilated houses, while masons are comparatively cleanly and comfortable in their circumstances.

The evidence which exists on this point, as has been already stated, is still very imperfect and inconclusive; but certainly butchers and tallow-chandlers or candle-makers appear to be more rarely inmates of a fever-hospital than persons belonging to other trades and occupations who are as numerous in the general population. But there are several circumstances which influence the admissions into hospitals, which ought to be taken into consideration before any conclusion can be drawn from them. 1st. Those operatives who are in better circumstances than the average class of them, with the exception of servants, are more rarely sent to an hospital. 2d. There may exist prejudices in a particular class of operatives against hospitals. Whether any of these objections may apply to the butcher or the candle-maker we are unable with certainty to determine, but undoubtedly the persons who followed these two occupations are not below the average in point of comfort in their circumstances.

M. Parent-Duchatelet has made some very curious and important experiments respecting the absorption of putrid emanations by various substances, which may, by analogy, be made to bear upon this subject. He found that distilled water and soups possessed, in a high degree, the property of impregnation with putrid effluvia; but that greasy bodies covering the surface of the liquid oppose an obstacle to the passage of these emanations. The following is his eighteenth experiment: "It might be useful to know if there were any means capable of preventing liquids from being impregnated with putrid emanations; this means chance furnished me with. Having set aside a certain quantity of *bouillon* as an experiment, I found it next day covered with a pellicle of grease, and below this grease it was in a most natural state; inferring from this experiment I poured two or three drops of oil into each of the experimental dishes filled with *bouillon*, as well as into the others filled with water, and after they had remained twenty-four hours among the putrid emanations I remarked that none of these liquids had contracted odour, but the surface of oil gave out in all the cases a very powerful odour."† Solid substances were also infected with the odour of putrid emanations, such as beef and wood,‡ and water, completely inclosed in a piece of intestine, bladder, or strong parchment, was even tainted with it.§ He ascertained also that camphor, valerian, and mineral tar communicated their odour to water when it is exposed to the effluvia arising from these substances.||

Although it has not been demonstrated experimentally, it seems highly probable that contagious effluvia, like fetid emanations, are soluble

* One patient had been a butcher, but had worked as a labourer for six months before he was affected.

† Annales d'Hygiène Publique, tom. v. p. 39.

‡ Ibid., p. 44. § Ibid., p. 39. || Ibid., p. 38.

in water, from the fact that thorough ablution of the clothes of persons who have laboured under fever disinfects them completely. Hence the advantage, as a prophylactic, of frequently sponging the skin of a typhus patient with water, more especially as tepid sponging is useful in the treatment of the disease. It appears, also, that contagious effluvia are volatile, like the emanations from putrid bodies, and may be separated from substances to which they adhere by means of heat. The late Dr. Henry of Manchester found that clothes impregnated with the miasmata of scarlatina and typhus were disinfected by exposing them to a temperature of 204° F. for one hour and three quarters, and that they did not induce any of these diseases when afterwards worn by healthy individuals.*

Are we then entitled to believe that butchers, candle-makers, &c. are more rarely affected with fever than other operatives? Dr. Tweedie supposes the exemption of butchers to depend on their good living; but it appears to us that the common theory respecting the operation of oily or greasy bodies in preventing fever will also explain the matter, and will apply to the butcher as well as to the tallow-chandler. It has already been shown by the experiments of Parent-Duchatelet, that greasy bodies attract powerfully putrid emanations; and it is well known that they unite very readily with odoriferous bodies of almost every kind; is it not therefore probable that contagious effluvia are regulated by a similar attraction, more especially when this hypothesis is coupled with the commonly received opinion in eastern countries, that oil is a prophylactic to contagion. If this be granted, how then does an oily or greasy body protect the butcher or the candle-maker? In the exercise of their various manipulations, the persons belonging to these two occupations have their clothes and the uncovered parts of their bodies more or less imbued with grease, an accompaniment which they almost constantly carry about with them. The contagious effluvia may, therefore, in place of being absorbed by the skin, combine permanently with the fatty body, and in this be fixed and rendered harmless.

We only bring forward this as an hypothesis capable of accounting for the generally received opinion respecting the protecting property of oil; but certainly if there be prophylactic powers in it or in any other substance, it is well worthy of being investigated experimentally.

* Philosophical Magazine, Nov. 1831.

On the Identity of Typhus and the Typhoid Fever.

As we have made several quotations from M. Chomel, as well as from M. Louis, who seem to think that the typhoid fever of France is a different disease from the ordinary British typhus, it may be necessary to show, although it may appear foreign to this essay, upon what grounds we consider them identical. The evidence by which the identity of typhus and the typhoid fever may be established, consists of two kinds, namely, the symptoms during life, and the morbid appearances after death; and in order that the subject may not be entrammelled with unnecessary detail, those symptoms and lesions only which in the aggregate are reckoned diagnostic of the disease shall be described. M. Chomel describes the disease under three septennary periods, each being characterized by peculiar symptoms. First period is characterized by feebleness, stupor, sleeplessness, mutterings, meteorismus, diarrhœa, sensibility of the abdomen, and a sense of fluid gurgling in the lower half of the belly, epistaxis, the typhoid eruption, and frequent pulse. Second period is characterized by the eruption which M. Chomel admits to be similar to that described by Hildenbrand, as observed in the *typhus castrensis*, sudamina, ulcerations and sloughs on various parts, chops and ulcers in the tongue, increased stupor, unconsciousness, dorsal decubitus, difficulty of deglutition, involuntary evacuations, retention of urine, subsultus tendinum, picking of the bedclothes, general and permanent rigidity of the members, deafness, coma, small weak tremulous pulse, or throbbing and intermittent, and varying in frequency from 80 or 90 beats to 120 in a minute, but which sometimes sinks to 40 or 50, a fuliginous coating of the tongue, teeth, gums and lips, diarrhœa, intestinal hemorrhages, increased meteorismus, respiration more constrained, fetid exhalations from the skin and breath. Third period. It is generally during this stage that the febrile disorder subsides, whether the patient recovers or dies. When the termination is going to be favorable the patient becomes more sensible, is more disposed to sleep, the mouth and tongue become more moist, the fecal discharges more natural, and the pulse becomes less frequent. On the other hand, when the termination is going to be unfavorable, the stupor increases, there is an alteration in the features, stertorous breathing, feebleness of pulse, a drier skin, or cold and covered with clammy sweat, general emaciation, hollow eyes, tremulous speech, indistinct and murmuring answers to questions, extreme feebleness, coma, and death. Sometimes death is accelerated by the occurrence of tetanic or epileptic paroxysms, and intestinal perforations and erysipelas are mentioned as occurring during convalescence. Any practitioner who has paid close attention to the symptoms of British typhus will readily discover their identity with those so well described by M. Chomel, as indicating the typhoid fever. There are, however, too or three symptoms which he places more dependence upon as characteristics of the disease than what is generally done in Britain, which it is necessary to notice more particularly. He represents diarrhœa as a very common symptom in the majority of cases, there being from four to eight alvine evacuations daily.

Now this symptom by no means occurs frequently in Britain, but this discrepancy may, to a certain extent, be explained, for the French physicians seldom exhibit purgatives in case of aggravating the *gastro-enterite;* hence the solid excrementitious matter which naturally accumulates in the torpid bowels of a typhoid patient will produce a morbid secretion from their excited surfaces, and being tinged with feculent matter may represent a fecal diarrhœa. This view is supported by the admission of M. Chomel himself. He states that "in some cases, at the time when the first improvement in the symptoms occurs, the alvine evacuations consist of firm, figured motions, to the great astonishment of the attendants, who with difficulty understand how such a change could be effected in so short a time. It is probable that these matters had remained during the whole period of the disease in some of the cells of the colon, and had not prevented the passage of liquid motions. There are discharged sometimes in these cases prodigious quantities of black dry matters."* From M. Chomel's account it would appear that meteorism or tympanitic swelling of the belly is more frequent in France than in Britain, for it has never been considered in this country as peculiarly characteristic of typhus. This discrepancy may, however, be reconciled, for according to this author the meteorism is only to be discovered in the early stages by percussion, while in the latter stages it is discoverable from the convex form of the belly. British practitioners apply the term tympanitis only to prominent distention of the belly by flatus, while those in France apply it not only to this but to minor enlargements not discoverable by the eye. Epistaxis is another symptom which M. Chomel considers frequent, and of great value as a diagnostic of typhus, especially if it occur during the first days of the disease. These hemorrhages are not profuse, but are most generally only a few drops, either from the anterior part of the nasal cavities or from the posterior by the throat, in the form of mucous masses, streaked and mixed with blood. Bleeding from the nose or mouth is certainly not so frequent in Britain as to constitute a diagnostic symptom of typhus, although it does occasionally occur; but it is generally hemorrhage to a considerable extent which has been noticed by authors in this country, and we do not doubt that the smaller discharges of blood or bloody mucosities have occasionally been overlooked or not attended to as unimportant. M. Chomel, although he does not appear to be prefectly convinced that typhus and typhoid fever are the same disease, is strongly inclined to this opinion from the similarity of their symptoms. He says that "another point which is still in favour of the opinion of contagion is the analogy which exists between the typhoid affection and typhus of camps, the contagious character of which is contested by no person. If we compare these two diseases, and from our recollections and from the description which has been given by Hildenbrand, and which it was in our power during 1814 to verify the accuracy, we shall find the same symptoms in the two affections, both of them commence by headach, with most subjects prostration and stupor appear at the beginning, and not solely, as in other affections, after the malady has endured a long time, and has very greatly debilitated the organism. The other symptoms, such as the meteorism, the diarrhœa,

* Chomel, Leçons de Clinique Médicale, tom. i. p. 42.

ole weakness of the senses, the tendency to ulcerations and
ges are common to the two diseases. The progress is the same
o diseases, inflammatory symptoms predominate at first and are
ls followed by nervous or adynamic phenomena. One of the few
es which we have observed between these two affections con-
ie duration, which is more prolonged in the typhoid affection
typhus. This last ceases generally about the fourteenth day,
is rare that the first terminates before the twentieth day.
difference consists in the frequency with which true petechiæ
spots are observed in typhus, which are comparatively rare in
oid malady. With regard to the cutaneous exantheme or
eruption, it presents the same characters in the two affections;
differences are in the number of spots and in the period of their
ice. In place of being confined, as they are most frequently in
oid fever, to the belly and chest, the lenticular spots in typhus
d in greater numbers almost the whole surface of the body.
ıst the eruption is developed generally about the fourth day of
ıse; in the typhoid fever it appears only about the eighth day,
ietimes much later. . . . . The only difference which
'and and Pringle admit between typhus and the most of other
ıich we have referred to the typhoid malady is that the severity
sease is greater in typhus, its progress more rapid, the adynamic
na more decided, and the eruption more general; but these dif-
are not sufficient to make us reject the identity of the malady,
may depend upon circumstances more or less troublesome,
'hich it is propagated. These differences may rather indicate
of intensity than that they are maladies entirely distinct."*
distinctions between typhus and typhoid fever, as stated by
nel, must appear to every one sufficiently acquainted with the
f Britain as very unimportant, for in young persons the eruption
ntly observed upon the extremities as well as upon the breast
y, and even in the same family, when the disease ought to be ac-
ged as identical, the number of spots observed on each member
en varies exceedingly. It is also a well-known fact that com-
nvalescence from typhus fever rarely takes place on the four-
ay except in young persons; while among those more advanced
or a greater number of days may elapse before this occurs.
ler to show still further the identity of the symptoms of typhus
typhoid fever, we shall quote the observations of a very accurate
erienced physician, Dr. Lombard, of Geneva. He states that
his experience and having witnessed numerous dissections of
dead of typhus fever, and having found in every one of them
and at Geneva the morbid state of the intestinal canal which
ıch pathologists consider as essential; under these circumstances,
arrived in Great Britain and had an opportunity of seeing the
ses here, and when I found that they presented a very great
y, if not an identity, of symptoms with those I had been for
the habit of observing, it is not to be wondered at, I say, that I
ave expected to find exactly the same post-mortem appearances.

* Chomel, Leçons de Clinique Médicale, tom. i. p. 336.

I mentioned this subject to my friends at Glasgow, and they allowe me to dissect the body of a person in whom I said no doubt could exi as to the presence of follicular disease; judge then, how great was m astonishment at not being able to detect a single trace of this morbi change in any part of the intestinal canal, and at finding no marks disease save some redness and softness of the mucous membrane of th stomach, which may have been produced by inflammation, but mor probably was owing to muscular congestion, occurring during the la stage of the disease, or even during the agony that precedes death."*

Dr. Lombard, however, was not convinced by this inspection; and o his arrival at Dublin he examined the bodies of two patients who ha died of typhus at different hospitals, and with the same results. It thu appears that the symptoms of typhus and the typhoid fever are nearl the same, and that they cannot be distinguished from one another; s that upon this ground their separation cannot be maintained. But thos who support the difference of the two affections rest their proof chiefl upon the pathological lesions which are found in the intestines.

M. Louis characterizes the typhoid fever under the following description: "An acute malady accompanied with a febrile movement more o less intense, variable in its duration, proper to young persons, chiefly to those who are placed within a short time in circumstances new to them, the cause of which is unknown, commencing by a violent shivering, anorexia, thirst, and in the great majority of cases by colics and diarrhœa, very soon accompanied by feebleness which is small in proportion to the other symptoms, then more or less quickly somnolence, stupor, delirium meteorismus, sudamina, lenticular rose-coloured spots, ulcers on the sacrum, ulcerations more or less deep of the skin, in the parts occupied by blisters, deafness, various spasmodic movements, or permanent contraction of the limbs; symptoms some of which disappear after a certain time, others increase for the most part in a progressive manner, when the patients die, or diminish more or less rapidly, at length to disappear altogether if the affection has a happy termination; the anatomical characters of which consists in a special alteration of the elliptic plates of the ilium.† . . . . Of all these lesions one only is constant being found in all the subjects: I speak of the alteration of the elliptic plates of the small intestines, to which may be added the alteration o the mesenteric glands; I have regarded it as inseparable from the existence of the affection under review in forming the anatomical character And as it was more or less great with some subjects who died on the eighth day of the disease, as with the greatest number the first symptoms indicated a lesion of the intestinal canal, as the alterations of the small intestines was greater than those of the colon, which was sound in a sufficiently large number of cases, I am warranted to conclude that the alteration of the elliptical plates commenced at the beginning of the disease."‡ M. Chomel, although he appears strongly inclined to support the doctrines of M. Louis and the other French pathologists, makes the following candid avowal of his opinion deduced from a rigid examination of all the pathological facts connected with the typhoid fever : "If, to this consideration furnished by analogy, we join these two other

* Dublin Journal of Medical Science, vol. x. p. 18.
† Louis de Gastro-Entérite, tom. ii. p. 317.
‡ Ibid., tom. i. p. 449.

circumstances already established: 1st, that there is no constant proportion between the severity of the symptoms and that of the lesions of the follicles; 2d, that the lesion has been completely absent in subjects who had offered during life all the symptoms of typhoid affection—it will become still more evident that the typhoid malady does not consist essentially of inflammation of the follicles; that this inflammation is only one of the phenomena of the disease, that it belongs, like most of the disseminated inflammations, to secondary inflammations; that it may be compared as to its pathogenic power not even to the pustules in variola, for in this there is always a proportion between the number of the pustules and the severity of the malady, but rather to the bubo in the pestilence of the East."*

M. de Claubry, in his prize essay read before the Royal Academy of Medicine, has adduced very copious evidence to prove the identity of typhus and the typhoid fever. He controverts the opinions of M. Louis respecting the ages that are exempt from the typhoid fever, and states that "it is not rare to see the disease in the Parisian hospitals at the age of four, six, eight, and ten years; and that M. Andral has witnessed it after seventy years."† He adduces Fauvages, Reveillé, Parise, Thruvenel, Ducastaing, and Pellerin to prove that ulcerations having elevated borders and exposing the peritoneal coat were found near the extremity of the small intestine in typhus.‡ The same author also shows that the typhoid fever spreads by contagion in the same way and under the same circumstances as typhus.§ Dr. Lombard, who contends for the distinction of the two diseases, adduces similar evidence to prove that typhoid fever is possessed of contagious qualities.||

We think it unnecessary to adduce evidence to prove that the follicular disease of the intestines is greatly less frequent in British typhus than in the continental typhoid fever; for the pathological investigations which have been made in England, Scotland, and Ireland regarding this point are now numerous and well known. Indeed in this country, in place of finding in almost every subject who died of typhus fever disease in the agminated or solitary glands, the minority has been the proportion found in many hospitals, and the affection of the spleen and brain more frequent than that of the intestines. If then there be no specific difference between typhus and typhoid fever; why are the pathological lesions of the intestines so much more common and intense in France than in Britain? It is perhaps not possible to give a satisfactory answer to this question, unless a difference of climate, diet, habits, &c. be allowed a certain influence. Dr. Lombard, in his first letter to Dr. Graves, seemed to have formed a very correct opinion respecting the nature of typhus, although he afterwards thought proper to change his views. In his first letter he says that "all these considerations, my dear friend, seem inevitably to lead to the conclusion that typhus fever is more a general disease affecting the whole constitution than a malady depending on a local inflammation or any local change of structure. May we not infer, also, that various causes serve to impress

* Chomel, Leçons de Clinique Médicale, tom. i. p. 536.

† Mémoires de l'Académie de Médecine, vol. vii. p. 190.

‡ Ibid., p. 80. § Ibid., p. 120.

|| Lombard's Clinical Remarks on Bilious and Typhoid Fevers, p. 17.

upon this general disease a tendency to associate itself with and produce various local ailments; among these causes, the most influential probably are, climate, seasons, the race of mankind, diet, and various circumstances which act powerfully both on the mind and body, and which when concentrated at any one point of time have given rise to those various epidemics of typhus that have so frequently devastated the different countries of Europe."* The same author, however, in his second letter to Dr. Graves, assumes his old hypothesis that the two fevers are different, and goes even a step further, for he maintains that both kinds are to be met with in the British and Irish hospitals. His views seem to be included in the following quotation from his letter: "But the Irish contagious fever is not the only source of typhoid diseases in Great Britain; the sporadic continued fever, observed in all parts of Europe, is also to be found in the different towns of the British empire. This fever, characterized by the follicular intestinal eruption and by consequent ulcerations, is to be seen in the different places above mentioned; in Glasgow it forms one third of the total number of cases;† in Dublin the proportion is much less; in London it is one fourth, and varies in the different seasons, because the continued sporadic fever is much under the influence of the temperature, being more frequent in autumn than in spring and winter; a proof that the proportion of this sporadic fever is the cause of the greater proportion of ulceration cases found at times in the British hospitals, as already mentioned. Having stated my opinion on your British continued fever, I resume it in the following theoretical view: You have two different fevers, one highly contagious, which I may call the Irish typhus, and in which the cephalic symptoms predominate to the exclusion of abdominal alterations; the other which is sporadic and most likely not so infectious, and in which the abdominal symptoms are more predominant, so much so that the follicular disease and consequent ulcerations are always to be found."‡ Dr. Gerhard, of Philadelphia, is another author who endeavours to show that there is a specific difference between typhus and typhoid fever, and that both are to be met with in Philadelphia. He makes the following observations respecting the post-mortem appearances which were observed in the American typhus: "In this large number of autopsies, amounting to about fifty, there was but in one case, and that doubtful in its diagnosis, the slightest deviation from the natural appearance of the glands of Peyer. In the case alluded to, in which there had been some diarrhœa, the agglomerated glands of the small intestines were reddened and a little thickened, but there was no ulceration and no thickening or deposit of yellow puriform matter in the submucous tissues. The disease of the glands resembled that sometimes met with in smallpox, scarlet-fever, or measles, rather than the specific lesion of dothonenteritis."§

* Dublin Journal of Medical Science, vol. x. p. 23.

† In some places in Scotland ucleration of the intestines seems to be very frequent. Dr. John Reid states that Dr. Goodsir, of Anstruther, examined ten bodies, and in every one the elliptical patches of Peyer and the solitary glands at the lower part of the ilium were elevated and ulcerated, and in four, perforation of the intestines had taken place. Edin. Medical and Surgical Journal. Oct. 1839, p. 459.

‡ Dublin Journal of Medical Science, vol. x. p. 104.

§ American Journal of Medical Sciences. February, 1837.

Dr. Gerhard's account of the epidemic typhus in Philadelphia is written with great accuracy, and his post-mortem inspections seem to have been conducted with much care and ability; but his results are certainly not what might be expected from a disease of the same nature as British typhus, which he describes it to be. For though we by no means believe that the lesion of Peyer's glands is a necessary concomitant of typhus, we are certainly supported by British observations when we state that there never were fifty consecutive inspections of typhus subjects made in this country without finding one decided instance of disease in the intestinal follicles.

It is quite evident that Drs. Lombard and Gerhard lay almost the whole weight of the diagnosis of typhus from the typhoid fever, upon the lesions of the intestinal follicles observed in the latter disease; for the almost identity of their symptoms during life are admitted; and is there any British practitioner that could distinguish those cases of eruptive typhus that had diseased follicles from those that had not? Again, it may be asked, what is the peculiar character of the diseased follicles, which constitutes the distinction between typhus and typhoid fever? In subjects dead of typhus fever which we have examined, the follicles are generally found with their margins only distinctly defined, but with little elevation or thickening of the subjacent textures, but such as to give a comparative opacity to the patch; when viewed with a magnifier, their surface presents irregular mammillated projections, bounded by corresponding depressions; sometimes there is only one patch, more frequently two or three, or a large irregular coalescence of patches at the ileo-cæcal valve; deep ulceration is not very common except in protracted cases; and occasionally there is the appearance of superficial ulceration. Now, if the anatomical and distinctive character of the typhoid fever be a morbid alteration of Peyer's glands, one single diseased patch, characterized by its defined margin, greater or less elevation and opacity, ought to constitute the disease as definitely as if there were twenty; just as smallpox is as essentially distinguished by twenty or thirty pustules as by several hundreds. If this be denied, where lies the line of separation?

Does it consist in a certain elevation of the follicles capable of admeasurement, in the deposition of a yellowish white or puriform matter in their subjacent textures, or in a certain amount of ulceration? But it may be argued that there are two species of fever in Britain, the one characterized by a peculiar disease of the intestinal follicles, and the other unaccompanied by any such lesion; and that some slight disease, characterized by a slight elevation and configuration of the patches, does sometimes take place in the latter, such as occurs in scarlet fever, smallpox, &c.; but that this lesion is totally different from that described by Louis and Chomel as characteristic of the typhoid fever. Now, we are ready to admit, at least as far as our experience goes, that the elevation and texture of the follicles are not in many cases precisely similar to those which are stated to be characteristic of the typhoid fever; but certainly they are even in this state morbidly affected.

The following table shows the lesions that appeared on the inspection of 63 eruptive cases, that were admitted into the Glasgow Fever Hospital from 1st May to 1st November, 1839, and it includes both male and female patients in nearly equal proportions:

Abnormal serum in brain	34
Bronchia red	25
Spleen rather large and soft	14
Spleen large and pulpy	30
Peyer's glands enlarged 1 to 3	12
Peyer's glands enlarged 3 to 6	14
Peyer's glands enlarged 6 and upwards	22
Solitary glands enlarged	14
No intestinal glands enlarged	12
Ulceration of intestines	13

The mesenteric glands were almost uniformly enlarged when ulceration of the intestines was present, but very rarely in other cases.

Now it may be contended that this simple enlargement or figuration of the intestinal follicles is a different affection from that which occurs in the continental typhoid fever, and hence ought to have a different classification. Such an assumption would lead to an endless and very unphilosophical division, and obviously to the formation of three species of typhus, out of the various complications or appearances which are observed in the intestines; namely, 1st, typhus without any intestinal affection whatever; 2d, typhus with simple enlargement of Peyer's glands; 3d, typhoid fever complicated with the follicular affection described by M. Louis; for if one author distinguish a species by a peculiar morbid appearance of the intestinal follicles, another has the same right to form a second, if the affection of these glands, in a certain number of other cases, be denied a pathological similarity to the first; whilst the morbid affections of the spleen, the lungs, the brain, &c. might all be brought forward to increase the subdivision still farther. The strength of our argument, however, that typhus and typhoid fever are the same diseases modified by place, season, epidemic influence, and perhaps by circumstances not yet ascertained, lies in the fact, that it has been admitted that cases of the latter disease, although rare, have occurred without any morbid appearance being discovered in the intestinal follicles; proving that this morbid condition of these glands is not a necessary anatomical character of the disease, such as hepatization or suppuration is of pneumonia, or serum of hydrocephalus. It has also been admitted that the intensity of the symptoms is not proportional to the lesions which ought to occur if the latter were the cause of the former; and it would be contrary to all experience to attribute the formidable symptoms of typhus or the typhoid fever to the lesion of one or two intestinal follicles, even though affected in the form described by the French writers. Would it not, therefore, be refining our classification of diseases beyond all precedent, to separate typhus and typhoid fever into two species, where it has been shown that the symptoms in both are the same, or very nearly so, that they have nearly the same laws, as far as these have been ascertained; that the severity of the symptoms in both is not in proportion to the lesions of the intestinal follicles; and that the other complications of both are similar, although various in the same places at different periods, while the only characteristic in dispute has been acknowledged not a constant and therefore not a necessary element for the existence of the disease.

THE

BRITISH AND FOREIGN
IEDICAL REVIEW,

FOR APRIL, 1841.

PART FIRST.
Analytical and Critical Reviews.

ART. I.

.NTIN *de Functionibus Nervorum Cerebralium et Nervi Sym-*
i. Libri Quatuor.—*Bernæ*, 1839. 4to, pp. 161.
Functions of the Cerebral Nerves and the Sympathetic. By
.LENTIN. In Four Books.—*Berne*, 1839.

cience is under great obligations to those who, during the period ost rapid advancement, are not seduced by the novelties that owd upon their notice into the paths of brilliant speculation; devote their time and talents to bring together what has been in various and distant quarters as ascertained truth; and who,—scriminately heaping up the materials they have collected, but es carefully examining their several merits, deciding by newly-evidence between conflicting statements, and filling up by their uiries the numerous small gaps that are left between the erec-others,—aim at presenting a systematic and harmonious view *ositive* knowledge on the subject. And in no science is such a nore needed than in physiology. We have often heretofore no- peculiar uncertainty attending the results of experiment, and tation that must always be felt in drawing inferences from ob-, in this science; this want of definiteness arising, there is good o believe, not from anything less certain in the *laws* of vitality hose of other departments of philosophy, but from the peculiar in which those laws are brought to bear upon one another, and y perturbations which arise from their mingled operation. Phy-then, more than any other department of science, needs such an al review; and the qualifications for success, needed by him who dertake it, are scarcely less,—in some respects much greater, hose required for pursuing the path of original research. What spectively are, we have no doubt that our intelligent readers will rceive. They are possessed in a very high degree by the author eatise before us. Already well known as an original discoverer in the most intricate departments of his science, he has applied his

talents most laboriously, and we think successfully, to the determination of our positive knowledge of neurology; and though his volume contains no one brilliant discovery, and not many original speculations, there are few which, in our estimation, can rank above it in real value. We shall, therefore, offer to our readers a tolerably full analysis of its contents, for the information of those who are inclined to be satisfied with such a concise view, and for the purpose of inciting those who make physiology a main object of pursuit, to possess themselves of the original.

This physiological treatise is to be regarded as a continuation of the anatomical description of the nerves, published three years previously. A third part will contain an account of the evolution of the peripheral portion of the nervous system; and the author hopes then to be able to treat of the development of the central organs in the same detail.

In the first book, after a chapter devoted to the *Lex Belliana*, the functions of the cerebral nerves are discussed. The second book contains the physiology of the sympathetic nerve. In the third book are considered the laws which govern the actions of the peripheral nerves. And in the fourth is discussed the influence of the nerves on the several functions, organic and animal. Throughout the whole, a very complete knowledge of the literature of the subject is evinced; and the references to the writings of others are extremely copious and valuable, yet are so arranged as not to interfere with the perusal of the text. What is sufficiently well ascertained to be physiological truth is carefully separated from that which, though probable in itself, requires confirmation; the author's own theories are stated quite distinctly from his experimental results; and the general conclusions may be always readily apprehended. When discussing the special functions of the nerves, the author first points out the information to be derived from anatomical observation of the origin and distribution of each. He then describes the method in which he derived it, states the results of his experiments, and compares them with those obtained by others, of which a general review is given. And lastly, the evidence obtained from pathological phenomena in man is considered.

CHAP. I. *On the Law of Bell.* The general fact that the posterior roots are sensory and the anterior motor is considered by Valentin to have been experimentally established beyond all question; and he regards the observation of pathological phenomena to be here quite subordinate in value to well-conducted experiments, since a natural disease is not a simple but a composite and varied experiment, and its results are liable to be influenced by a great variety of disturbing causes. Where pathological phenomena coincide with the facts ascertained by physiological investigation, they may be accepted as confirmatory of these; but if they should seem to run counter to them, they are not to be set down as altogether disproving them, since a more complete knowledge of their conditions would probably enable us to account for what seems anomalous. We are not yet in a condition to say, for example, what amount of integrity of the nervous tissue is compatible with the performance of its functions; and a mere statement that it is indurated or softened is by no means sufficient to establish its loss of power. With these remarks we pretty much agree; but, applying them to the case in question, we must own that we consider our knowledge of the respective functions of the

two roots of the spinal nerves to be much more positive than that of the offices of the divisions of the cord with which they are connected.

That the transmission of impressions is always centripetal in the sensory nerves and centrifugal in the motor appears to be an equally well-established fact. Recent microscopic examination has also demonstrated that which was long ago suspected,—that the individual nervous fibres run with perfect distinctness from their origin to their termination; and that, though an interchange of fibres, producing an anastomosis, often takes place among the trunks, there is no such an anastomosis in the individual fibres. Hence it is probable that every fibre maintains its peculiar endowments through its whole course. This doctrine is the foundation of the entire modern system of nervous physiology.

It is well known that Bell and others have laid great stress on the presence of ganglia on the posterior roots, and other points of anatomical correspondence, in their comparison of the cerebral with the spinal nerves; but no evidence drawn from such anatomical examination can, in Valentin's opinion, be fairly put in competition with that derived from physiological experiments,—although confirmatory of it, should it correspond. For this he states his reasons in detail. Nor can the ultimate distribution of the nerves be alone relied on. For though in general the nerves which proceed to the skin or mucous surfaces are sensory, and those which are sent to the muscles are motor, there are many exceptions; and the absence of general sensibility in the nerves of special sense could not be thus predicated. The primitive fibrils of the nerves appear to be the conductors of impressions; and these are isolated from each other by sheaths, which are thicker in the nervous trunks than in the central organs, as if to provide for their more complete segregation in the discharge of their functions. In the opinion of Valentin it is in the gray matter alone that motor changes originate, and that sensory impressions are received. A few remarks on the nervous system of invertebrated animals are appended to this chapter. They contain some experiments on a lobster, the general result of which is confirmatory of Dr. Carpenter's views as to the respective functions of the cephalic and ventral ganglia. Valentin considers that there is no physiological evidence of the motor and sensory functions of the two strands of the cord, as attributed to them by Newport, and, in a different way, by Grant.

CHAP. II. *Olfactory Nerve.* In this chapter, the account of the individual nerves is commenced with the first pair. This, it is stated, possesses in itself no motor properties; nor is it an excitor of motion through the nervous centres; for no contraction of muscles can be produced by irritating either end of it when divided. It is not a nerve of common sensation; for animals do not exhibit any sign of pain when it is subjected to any kind of irritation. The division of the nerve, or the destruction of the olfactive ganglia, does not seem to inconvenience them materially. They take their food, move with their accustomed agility, and exhibit the usual appetites of their kind. The common sensibility of the parts contained in the olfactive organ is in no degree impaired, as is shown by the effect of irritating vapours. But the animals are destitute of the sense of smell, as is shown by the mode in which these vapours affect them. At first they appear indifferent to their presence; and then suddenly and vehemently avoid them, as soon as the Schneiderian mem-

brane becomes irritated. Moreover, if two dogs with the eyes bandaged, one having the olfactory nerves and ganglia sound, and the other having had them destroyed, are brought into the neighbourhood of the dead body of an animal, the former will examine it by his smell; whilst the latter, even if he touches it, pays no attention to it. This experiment Valentin states that he has repeated several times, and always with the same results. Further, observation shows that sensibility to irritants—such as snuff, and acuteness of the power of smell, bear no constant proportion to one another; and there is ample pathological evidence that the want of this sense is connected with some morbid condition of the olfactory nerves or ganglia.

CHAP. III. *Optic Nerve.* No chemical or mechanical stimulus of this nerve produces *direct* muscular motion; but a reflected motion is excited through the third pair, as is proved by the contraction of the pupil when the nerve is divided, this not taking place if the central organs be removed. The contracted state of the pupil usually soon disappears. That common sensibility is retained when the functions of the optic nerve are completely destroyed is well known; as is also the fact that division of it puts an end to the power of vision. Valentin states that, although the optic nerve may, like other nerves, be in appearance completely regenerated, he has never been able to obtain any evidence that the power of sight has been in the least degree recovered. He remarks that animals suddenly made blind exhibit great mental disturbance, and perform many unaccustomed movements. The complete destruction of the sense of vision, by division of the optic nerve, is easily proved by experiment. The eyelids are not closed when a strong light is suddenly directed on the eye, as is the case when the eye is sensible to it. Again, the state of the pupil is not affected when a strong light is made to impinge upon the retina, except, however, in the following cases: 1. Where the division of the nerve is incomplete, imitating a partial amaurosis in man. 2. Where the rays of light impinge on the iris, in which case they cause contraction of the pupil by reflex action through its own afferent nerves; and Professor Valentin remarks that this irritability of the iris seems much increased by section of the optic nerve. 3. Where the solar rays fall on the eye for a period sufficient to affect its temperature, so that the nerves of common sensation may serve as excitors to the reflex action. (This explanation of the contractility of the pupil in eyes entirely amaurotic has been already suggested by Mr. Grainger.) 4. Where the pupil of the sound eye undergoes alteration in form, so that one stimulus excites the motor nerves of both eyes. This is often seen in amaurotic patients. We see no reason, however, to depart from the view proposed by Dr. M. Hall and Mr. Grainger, that the activity of the pupil in some cases of amaurosis may be owing to the completeness of the nervous circle requisite for reflex action, the disease which obstructs vision being higher up in the encephalon. On this point Professor Valentin's experiments do not bear, as in all of them the optic nerve itself was divided. He justly remarks that the contraction of the pupil is by no means the only reflex action excited through the optic nerve; but that the orbicularis is thrown into action by the incidence of light upon the retina when in an irritable state, as is so peculiarly seen in scrofulous ophthalmia; and that the involuntary movement

ball (generally in a horizontal direction), which is so often noticed es of imperfect sight, is to be attributed to the same cause. Moranges are sometimes observed to take place in eyes whose optic has been divided; but these are by no means so constant or exten- when the fifth pair is paralyzed; and they may not improbably ibuted to the injury occasioned by the operation itself to the parts the orbit. It is well known that eyes which have been amaurotic ny years preserve their usual appearance; but where the amau- complete, the texture of the retina is changed. It appears as a membrane, composed of white cylindrical threads; and does not t any appearance of granules, of nucleated globules, or of primitive s fibrils; and the yellow spot of Soemmering becomes paler, and st undistinguishable. But if a very slight degree of sensibility to remain, these changes are much less decided. It is well known hen the sight is destroyed by a disease or injury which prevents ssage of light through the pupil, the whole eye becomes more or rophied; and the retina and optic nerve are found after death (if rbid condition have lasted sufficiently long) to have lost their chaistic structure. It seems evident, then, that the continuance of unctional operations is a necessary condition of the maintenance of ormal organization; and we can very well understand that this be the case, from the analogy of other parts of the system.

author next proceeds to discuss, with considerable minuteness, enomena of *subjective* vision, as distinguished from *objective;* the being excited by the impressions of external objects on the retina, rmer by some internal affections of the nervous system. Subjecsion may originate from changes either in the nervous centres, as rium, or in the vivid conceptions of the memory; or from changes optic nerve or retina, as when stimuli directly affect these parts. ding the causes of the different images thus produced, we have ttle knowledge; yet it may be noticed that particular effects comsucceed the application of certain stimuli. Sometimes the interior bulb itself may be represented to the mind in this way, as in the ited experiment of Purkinje, in which the circulation of the blood vascular plexus of the retina may be made the subject of obser-. Professor Valentin gives a minute description of the appearances himself produce in one of his eyes, which he considers as a repreion of certain parts in its interior. As an instance of subjective originating in the central organs, he mentions that he has frey, after devoting several hours to the use of the microscope, and lly when he has been much interested in the object of his investi-, perceived during sleep (but not he thinks while dreaming) most te and beautiful representations of the objects which he had been ing. In some cases it would appear likely that visual impressions cited by external causes, which are made to assume a particular ter in the mind by the influence of memory and fancy. And it is, s, owing to the want of the connexion of such impressions with s perceptions, that persons born blind, and sometimes those who nce enjoyed the power of vision, but have long been destitute of not seem to have any visual conceptions even when dreaming. respect, then, the case does not resemble that of a person who

has lost a limb by amputation, and is subject to a perception of its pains and movements; and the observation tends to confirm the marked distinction between the perceptions acquired from common and from the special sensations, which has latterly been much insisted on, both by metaphysical and physiological writers.

CHAP. IV. *Oculo-motor Nerve.* The experiments hitherto performed on this nerve are not satisfactory in regard to the degree of sensibility it possesses. From those of Valentin it appears that pain is felt when the nerve is divided; but that this is not so great as that occasioned by section of the fifth pair. At the moment of section, the pupil of the affected eye, and frequently also that of the other, is strongly contracted. If the animal lives long enough after the operation (which, on account of the almost inevitable division of the carotid artery, is seldom the case,) the pupil becomes again dilated and remains paralyzed. No amount of light thrown on the retina or the iris occasions its contraction; nor does the iris ever move by sympathy with that of the other eye. Hence it follows that the oculo-motor is the chief source of the movements of the iris; but it will be hereafter shown not to be the only spring. The other functions of the nerve may be best examined by removing the brain, and applying mechanical or chemical stimuli to the trunk. Its influence on the pupil then becomes very evident. The eyeball is at the same time rapidly rotated inwards, more rarely inwards and upwards, and more rarely still downwards. Motion of the upper lid is very seldom seen; but this is probably due to the injury which the fibres of the levator palpebræ have sustained by the operation. The general motor functions of this nerve are sufficiently well established by cases of paralysis in man. When the distribution of the branches of this nerve is considered, in connexion with the actions of the different muscles it supplies, it will appear probable that they possess various endowments. The superior branch derives additional sensory fibres from the naso-ciliary branch of the ophthalmic (fifth pair); and in this manner the sensory endowments of this division of it are probably increased. The sensory endowments of the seventh pair will be shown to be entirely derived from anastomosis with the fifth and cervical nerves; and it will be recollected that a similar connexion appears to exist between the anterior and posterior roots of the spinal nerves. (See Br. and For. Med. Rev., vol. IX., p. 547.) This superior branch of the oculo-motor supplies the superior rectus and levator palpebræ; the action of which appears to be of a purely voluntary character. The inferior branch supplies the internal and inferior recti and the inferior oblique, and also the iris. Now the motions of the iris are altogether automatic or reflex. Some have regarded them as in a degree voluntary; but the so-called voluntary contraction of the pupil is always connected with some action of the muscles of the eyeball. The inferior oblique appears to have an almost solely involuntary action; by it is performed the rotation of the ball upwards and inwards, which is observed to take place during sleep, the internal rectus perhaps assisting. The pupil is at the same time contracted; and this also happens if each eyeball be rotated, by an effort, towards the inner canthus. But the pupil is not contracted when the eye is voluntarily rotated upwards; so that the action of the iris is evidently dependent upon the inferior rather than upon the superior branch of the oculo-motor; and it is only by such

an impulse transmitted along the former, as will also excite the action of the muscles of the ball which it supplies, that the so-called voluntary movement of the pupil can be produced. It is apparently by this branch only that the reflex actions are performed, which are excited by impressions transmitted through the optic nerve and fifth pair. This view of the somewhat antagonistic functions of the two branches is supported by the symmetrical distribution of the muscles. The rectus externus, supplied by the sixth pair; the rectus superior, supplied by the superior branch of the third; and the obliquus superior, supplied by the fourth pair,—are all, in Professor Valentin's estimation, purely voluntary muscles. Whilst the rectus internus, rectus inferior, and obliquus inferior, supplied by the inferior branch of the third, are more or less automatic in their action, the iris being entirely so. These may be likened to the flexors, whilst the three voluntary muscles perform the part of extensors.

CHAP. V. *Pathetic Nerve.* The course of this nerve within the cranium renders experiment upon it difficult; and it cannot be satisfactorily ascertained whether any pain is felt by the animal when it is divided. Of its motor action there can be no doubt. This may be most readily demonstrated by removing the brain and irritating the trunk of the nerve. The eyeball is then rotated downwards. The character of this movement is discussed along with that of the sixth pair, to which we shall next proceed, with the view of bringing together all which relates to the motions of the eyeball.

CHAP. VII. *Abductor Nerve.* Experiments upon animals and observation of cases of paralysis in man leave no doubt that the sixth pair of nerves is chiefly if not entirely motor, and that its action is confined to the external rectus. The course of this nerve within the cranium renders it almost impossible to determine by experiment whether or not it contains any sensory fibres; but if any exist in it, they are probably very few in number. The muscle which this nerve supplies appears to be entirely under the control of the will.

The following classification of the movements of the eyeballs and the succeeding remarks are, we think, of sufficient importance to merit being given in full:

"The regular conjoint movements of the eyeballs are rather *harmonic* than *symmetrical*. The varieties which they present may be reduced to the following classes: 1. *One eye is rotated inwards, the other outwards.* Here the internal rectus of one eye and the external rectus of the other are put in action. 2. *Both eyeballs are elevated* by the contraction of the superior recti. 3. *Both are depressed.* This is at first effected by the conjoint action of the inferior recti alone, but if the movement go beyond a certain point the eyes are rotated inwards, showing that the internal recti are also in action. 4. *Both are drawn directly inwards and downwards* by the action of the internal rectus, joined either with the inferior rectus or the superior oblique. 5. *One eye is rolled upwards and outwards, the other upwards and inwards.* In this case the rectus superior of both eyes acts with the external rectus of one and the internal rectus of the other. 6. *One eye is drawn downwards and outwards, the other downwards and inwards;* the inferior rectus acting in both with the external rectus of one and the internal of the other. All these movements may be voluntarily performed by man; there are two others which he cannot directly perform by an act of the will, the one occurring during sleep, the other when an object is brought very near the eyes. In the first of these cases, 7. *Both eyeballs are rotated upwards and inwards* by the action of the inferior oblique,

assisted perhaps by the internal rectus. In the other, 8. *Each eye is directed inwards* by the interior rectus, conjointly with the superior or inferior rectus; in the latter case it is most complete.

"Hence it follows: 1. There is one movement amongst all these, which is at the same time harmonic and symmetrical, the elevation of the eyeball by the superior rectus. The depression of it by the inferior rectus is so in the first instance; but when the eyes are rotated inwards the motion loses its harmony, though it is still symmetrical. 2. All the regular motions of the ball which are harmonic, but asymmetrical (as in class 1), are effected by a contraction of one of the muscles formerly specified as voluntary, and one which is partly automatic. 3. Two voluntary muscles whose actions will produce want of harmony between the eyes (such as the superior rectus and superior oblique), cannot be made to contract together. 4. Two automatic muscles whose action destroys the harmony of the eyes may be contracted together, as in classes 7 and 8; in the first case the action cannot be voluntarily produced; in the second it may be, but becomes more decided when the internal rectus is assisted by the inferior than when the action of the superior is conjoined with it. [We are disposed to question whether this movement is voluntary; it seems to us to require an object to which the eyes may be directed.] 5. Voluntary squinting (lusciatio) is performed by those muscles only whose action is chiefly automatic; as a result of disease (when not depending upon lesion of the iris or retina) it is mostly caused by the automatic muscles.

"From these data, the following account of the muscles of the eyeball may be given. In each eye of man and animals there are two opposed problems to be reconciled. On the one hand, *symmetrical* muscles should exist, as in other parts of the body. On the other, the movements should not be symmetrical but *harmonic*. Again, some of the movements of the eyeball must be voluntary, whilst others depend upon various affections of the retina. To the former objects the external muscular circle (consisting of the rectus superior and externus and the obliquus superior) is destined; and to the latter the internal muscular circle (comprehending the rectus inferior and internus and the obliquus inferior). The pupil is dilated by the voluntary circle [we do not see the evidence of this]; and contracted by the automatic. The former corresponds with the extensors, and the latter with the flexors, and is hence more readily caused to contract, both in health and disease. But the several muscles belonging to one circle are respectively opposed by others in the second circle; and harmonic motion always results from the action of a muscle of the voluntary circle on one side and of the automatic on the other. Hence we see why this peculiar disposition of the muscles should constantly exist. Nor is the arrangement of the nerves less elucidated by this view. It is evident that the automatic muscles should derive their power from the same source as that which produces contraction of the pupil, and that the external rectus and superior oblique should have their own nerves. The supply of the superior rectus from the third pair is also accounted for, its action being harmonious with that of the levator palpebræ, and these being the only two muscles of the voluntary circle whose action is accompanied by a dilated state of the pupil, and which, therefore, would require to be connected with the same system of nerves as that which governs the iris." (pp. 30-1.)

CHAP. VI. *Fifth Pair.* When the trunk of this nerve is divided in the living animal (as Prof. Valentin has seen very successfully accomplished by Magendie) evident signs of acute pain are given. After the incision has been made through the skin, the animal remains quiet until this nerve is touched, and when it is pressed or divided it utters doleful cries, which continue for some time, showing the painful effect of the irritated state of the cut extremity. The common sensibility of all the parts supplied by this nerve is entirely destroyed on the affected side. The jaw

es not hang loosely, because it is partly kept up by the muscles of e other side; but it falls in a slight degree, and its movements are en, when carefully observed, to be somewhat oblique. If the trunk be vided on each side, the whole head is deprived of sensibility, and the imal caries it in a curious vacillating manner, as if it where a foreign dy. If the anterior branch only be divided, all the parts supplied by are found to have lost their sensibility, but their motions are unimired. The same is the case with regard to the second branch. But the third branch be divided, the jaw droops, as just stated; and itation of the trunk will produce convulsive contraction of the mastitor muscles. When the whole nerve or its anterior branch is divided rabbits, the pupil is extremely contracted, and remains immoveable. ut in dogs and cats and pigeons it is dilated. The pupil of the other e is scarcely affected, or if its dimensions be changed, it soon returns its natural state. The changes in the eyeball consequent upon secon of the fifth pair, which have been described by Magendie, have been so observed by Valentin. He states that their commencement may e noticed before the lapse of twenty-four hours after the operation. ll these conclusions are confirmed by pathological observations on the man subject.

In regard to the *ophthalmic* branch, all experiments and pathological servations concur in attributing to it sensory endowments only. The nly apparent exception is in the case of the naso-ciliary branch, since ere is good reason to believe that the long root of the ciliary ganglion nd the long ciliary nerves possess motor powers. But these, as will be reafter seen, are derived from another source,—the sympathetic nerve. he same may be said of the *superior maxillary* branch considered in self; but its connexions with other nerves, through the spheno-palatine anglion and its anastomosing twigs, introduce other kinds of primitive bres into it. These connexions are minutely described by Valentin.

The *inferior maxillary* branch is the only one which possesses motor s well as sensory endowments from its origin; but its different subdiisions possess these endowments in varying proportions, as Valentin hows in much detail. Sir C. Bell's views of the special character of e motor portion as a nerve of mastication, are fully borne out by his searches. The function of the lingual branch will be discussed with at of the glosso-pharyngeal.

The general analogy of the fifth pair with the spinal nerves is sufficintly evident. Yet there are several important points of difference. For the spinal nerves, after the conjunction of the two roots, the branches nto which the trunk divides have mixed endowments, both the anterior nd posterior spinal nerves being at the same time motor and sensory. ut in the fifth pair, the anterior divisions (corresponding with the nterior spinal branches) are purely sensory, and remain so until they cquire motor properties by inosculation with the facial. Moreover the ranches of the third division, which in the first instance possess a mixed function, often lose their motor fibres in their course, and become entirely sensory. The three branches of the sensory division are united by a plexus, as are also the second and third by themselves. This plexiform arrangement finds no analogy in the spinal nerves; the junction of whose anterior and posterior roots is only represented in

the fifth pair by the imperfect union of the sensory and motor division of the third branch in the plexus of Santorini and Girard. All the subdivisions of this branch are from their commencement, of a mixed function; and it is here therefore, and not with the whole of the fifth pair, that we are to regard the analogy with the spinal nerves as existing.

CHAP. VIII. *Facial Nerve.* That this nerve is solely one of motion, as maintained by Bell, has been more lately denied by Arnold and others, who have maintained that it both possesses motor endowments, and arises by a double root. The latter assertion is quite fallacious; the former can only be determined by experiment. Valentin found that the only way to divide the seventh pair within the cranium, without so much disturbing the roots of the other nerves as to render the experiment useless, was to remove the whole posterior part of the bony covering. In this way he ascertained that the animal gave no sign of pain when the facial nerve was touched or divided; although it uttered violent cries when the roots of the vagus were touched. By this it was demonstrated that the facial at its origin contains no sensory fibres. The numerous anastomoses which it undergoes before its exit from the stylo-mastoid foramen are minutely described by Valentin, who traces microscopically the course of the individual fibres from one nerve into another; but his details would be out of place here, though no cultivator of physiological anatomy ought to neglect them. That the facial nerve, when about to be distributed to the muscles of the face, possesses some degree of sensibility, seems to have been allowed by Bell, and has been pointed out by many subsequent experimenters; and some have attributed it to anastomosis with other sensory nerves, especially the fifth. Eschricht and Lund endeavoured to determine this by laying bare the portio dura at its exit from the stylo-mastoid foramen, and then dividing the trigeminus at the base of the skull. It was then found that the sensibility of the parts anterior to the auditory foramen was entirely extinguished, whilst posteriorly it remained. It may be inferred, therefore, that the sensibility observed in the portio dura depends on anastomosis with the fifth; that which remained in the back part of the head being derived from the cervical nerves. Panizza also proved that, if the seventh pair be cut in a horse in front of the parotid gland, signs of pain are given; but that if the gland be dissected from over it, so that it may be divided before its anastomosis with the fifth, no indication of sensibility is then given. Several such anastomoses appear to take place; the degree of each varying in different animals, so that it is difficult to lay down any general propositions as to the sensibility of the several branches of the vagus. However, the fact that whatever sensibility it may possess is derived by its anastomoses with other nerves appears fully established. The account given by Valentin of its motor functions corresponds almost entirely with that of Bell. He considers it now beyond a doubt that it is the general motor nerve of the face, that the expression of the countenance depends upon its integrity, and that all the respiratory movements of the mouth and nose are performed through this channel; those of mastication, on the other hand, being entirely due to the third division of the fifth pair. His testimony is valuable, being evidently the result of a very detailed and

careful examination of the question; but there is no such novelty in the evidence adduced as to render it desirable for us to present it more fully in this place.

CHAP. IX. *Auditory Nerve.* The functions of this nerve are easily determined by anatomical examination of its distribution, and observation of pathological phenomena. Atrophy or lesion of the trunk destroys the sense of hearing; whilst irritation of it produces *subjective* phenomena, analogous to those of vision. From experiments made upon this nerve before it leaves the cranial cavity, it appears satisfactorily ascertained that it has no motor power whatever, and that it is not endowed with common sensibility, the animals not giving any sign of pain when it is irritated or divided. Microscopic examination of this nerve clearly indicates its intermediate character between the anterior and posterior sensory nerves issuing from the cranium, namely, the olfactive and optic, and the glosso-pharyngeal. Nucleated globules are not found in its origin, but they may be traced in its passage through the meatus auditorius, and in the labyrinth. The primitive fibres are not so soft as those of the olfactive nerve, nor so slender as those of the optic; but they are softer than those of the glosso-pharyngeal. It forms a plexus with the facial, to which there is no analogy in the optic and olfactive nerves, but to which a similar one exists in the glosso-pharyngeal.

CHAP. X. *Glosso-pharyngeal Nerve.* The functions of this nerve, as ascertained by anatomical research, by experiment, and pathological observation, are very minutely discussed; and, although we are on the whole more inclined to accord with the results obtained by Dr. Reid, and with his inferences from them, than with those of Valentin (which only differ however in subordinate particulars), we think it worth while to follow our author through the heads of his argument. He begins by remarking that, contrary to what has been up to this point observed in the cranial nerves (the third branch of the fifth, which receives the recurrent branch of the superior maxillary being the only exception), the glosso-pharyngeal anastomoses freely with the vagus, hypoglossal, and accessory nerves, before leaving the cranial cavity. The cause of this Prof. Valentin considers to be, that these nerves are not cerebral, but belong to the medulla oblongata, and are thus to be regarded as formed by an *incomplete* metamorphosis of the cervical nerves. A cursory view of the distribution of this nerve would lead to the conclusion, that it possesses motor properties, since several of its branches may be traced into muscles. But a more careful examination shows that these only penetrate the muscles, and really pass on to be distributed to the mucous membrane, forming plexuses with the third branch of the fifth, the facial, and the vagus. Dr. Reid's experiments, which clearly prove that the muscular movements, apparently produced by stimuli acting through this nerve, are, in reality, of a reflex character, and are immediately dependent on the pharyngeal branches of the par vagum, are next adverted to. But the most certain information is that to be derived from examination of the roots of the nerve. According to Müller, the two roots have corresponding endowments with those of the spinal nerves, the one possessing a ganglion (the superior jugular), and the other having the course of its fibres undisturbed. But, argues Valentin, if this be the case, the

motor branch, which does not pass through the ganglion, would be the largest of the two; and it is quite clear that if the glosso-pharyngeal possesses any motor properties, they form a small part of its general functions. The petrosal ganglion is not properly accounted for on the theory, Müller attributing it to the anastomosis of a branch of the sympathetic with the glosso-pharyngeal. But, observes Valentin, other anastomoses of equal importance take place elsewhere, without the formation of a ganglion. Stimulation of the roots of the glosso-pharyngeal nerve does not produce those motions of the pharynx which are excited by irritation of the pharyngeal branches themselves, nor does it produce any muscular movement; whence it appears, that no motor fibres originally exist in the nerve. The small amount of the signs of pain exhibited by animals when the trunk of this nerve is irritated or divided, after its exit from the cranium, compared with those which stimulation of the lingual branch of the trigeminus produces, appears to prove that this nerve is by no means entirely one of common sensation, though it certainly contains sensory fibres. These may be introduced into it, however, by the tympanic branch of the trigeminus, and by the branch of the vagus communicating with the petrosal ganglion. But it is probable that some are derived from its own origin. These communicating branches seem to transmit fibres of each nerve to the other; but, on account of their slenderness and their situation, minute examination of them is very difficult.

The important question next arises, whether the glosso-pharyngeal is the nerve of taste. The experiments to be made for this purpose must be performed on the nerve after its exit from the cranium; for even if the animal remains sufficiently long alive after the roots of the nerve have been exposed, the disturbance produced by this severe operation is too great to prevent the results of the experiments from being worthy of reliance. For dividing the nerve as near its exit from the cranium as possible, Valentin employs the method of Panizza, whose results he confirms in every particular. He states that, when the division of the nerve has been complete, the sense of taste seems altogether destroyed. Dogs will take decoction of colocynth, and other nauseous ingredients mixed in their food, without any appearance of dislike, although after a fast of two days an uninjured dog will reject with disgust that which has but a slight admixture of the same. But if a very small portion only of the nerve on either side remains undivided, enough sense of taste still remains to cause the rejection of the food, and the excitement of reflex motions in the pharynx and upper part of the œsophagus. From such experiments it is concluded by Valentin that the glosso-pharyngeal is the special nerve of taste as well as that which communicates nauseating impressions, and which stimulates the reflex actions resulting from them. In the two latter particulars, the results of Dr. Reid's experiments (which he only quotes at second hand from l'Institut) fully bear out his; but we cannot see that he brings sufficient evidence of the former. Of one very important function of the glosso-pharyngeal nerve,—the conveyance of those impressions from the fauces which excite the reflex movements of deglutition, and which do not appear necessarily to involve sensation, he takes no notice. This has, we think, been fully established by Dr. Reid's experiments; and the small amount of sensibility evinced when the nerve

irritated, is thus fully accounted for, without having recourse to the ipposition that the bulk of the nerve is a conductor of special sensory ipressions.

The question how far this is also a nerve of taste partly turns upon e function attributed to the lingual branch of the fifth pair, which alentin next discusses at much length. This he regards as nothing more an a nerve of common sensation; and he states that, after complete ction of it on both sides, the sense of taste remains unaffected; whilst, ı the other hand, irritation of the trunk evidently causes very severe in. We do not, however, regard his experiments as by any means ifficient thus to determine the question. They seem to have been per-rmed in ignorance of those of Dr. Alcock, who arrived at very different sults, this gentleman inferring from them that the lingual branch of the fth pair ministers to the sense of taste in the anterior part of the tongue, ı which conclusion he is borne out by Dr. J. Reid. If these experiments ad been quoted by Valentin, and any fallacies which they may contain ad been exposed and corrected, we should have had some ground for oming to a decision upon their respective merits; but at present, the vidence derived from experiment being, to our minds, nearly balanced, he question is rather to be decided by pathological observation, and xamination of the anatomical distribution of the glosso-pharyngeal erve. There is this fallacy, as it appears to us, in Valentin's inference egarding the absence of the gustative sense in the lingual branch of the ifth, that, although it is sufficiently evident that the sense of taste emains in some degree after its division, there is no proof that it is not lestroyed in the *anterior portion* of the tongue, which other evidence hows to depend on it for this endowment. Considering, therefore, that he results of physiological experiment are here by no means certain, we annot agree with our author that pathological phenomena, where they ppear to differ from them, are to be thrown to one side. Though the ases in which the gustative sensibility of the anterior part of the tongue as been destroyed, with its tactual sensibility, when there was no reason to suppose that any other than the fifth pair of nerves was involved, are by no means rare, he refers to a few only, on which he thinks that he can throw some doubt; and he seems to think that what is certainly the more frequent case, the loss of common sensibility whilst that of taste still remains, is a powerful argument that the two do not depend on the same nerve. But this proves nothing more to our minds, than that the fibres which serve these two purposes are separate, and have distinct central terminations; for cases are not uncommon in which one of two endow-ments of a nerve, even when these have a general similarity, as tactual ensibility and sensibility to temperature, or the voluntary and sympa-thetic motor powers of the seventh pair, is lost without the other being affected. In regard to the anatomical distribution of the glosso-pha-ryngeal nerve, Valentin asserts that it is not so completely restricted to the root of the tongue as is ordinarily stated; but that it sends a branch forward on each side, somewhat beneath the lateral margin, which inosculates with the fifth, and supplies the edges and inferior surface of the tip. It is quite possible that these may contribute to the gustative sensibility of these parts; but we can by no means agree with Valentin, that the sense of taste is confined to the portion of the tongue thus sup-

plied by the glosso-pharyngeal; and we remain, therefore, in our previous conclusion, that both nerves contribute to it, the lingual branch of the fifth having also a high degree of tactual sensibility, and serving as an excitor to the movements of mastication (when these are involuntary as in the infant, or so habitual as scarcely to require the constant operation of the will), whilst the glosso-pharyngeal has little common sensibility, but has for its chief function to excite, through the nervous centres, the movements of the pharynx concerned in deglutition, vomiting, &c.

There is one argument on this subject that may be drawn from comparative anatomy, which we do not recollect to have seen noticed,—that of all the senses commonly denominated *special*, that of taste appears most nearly connected with *common* or *general* sensibility; and that some amount of it appears to be possessed by animals which are endowed with no other special sense. Where this is the case, it is evidently connected with the nerve which, issuing from the cephalic ganglion for the supply of the head and anterior part of the body, corresponds with the fifth pair of vertebrate animals. That we may bring together all that relates to the tongue, we shall next proceed to

CHAP. XIII. *Hypoglossal Nerve.* That this nerve is principally motor has long been known; but there has been less certainty as to its possession of sensory properties. Indications of pain are given when its trunk is irritated after its exit from the cranium; but these may be supposed to proceed from its anastomosis with the cervical nerves, which probably imparts sensory fibres to it. But in rabbits, each trunk passes out in two bundles through separate orifices, and when the anterior of these is stimulated (in a dead body still irritable) no muscular movements follow, whilst excitement of the posterior bundle causes movement of the tongue. Hence Valentin concludes (without, as it seems to us, that full proof which he might have obtained) that the anterior fasciculus is sensory, and that the hypoglossal is analogous to the spinal nerves; which is further indicated by a slight ganglionic enlargement on the posterior side of its root, which is more perceptible in many of the mammalia than it is in man. The lingual branches of this nerve, however, do not seem to possess any large share of sensory endowment. Valentin seems to consider that *all* the movements of the tongue are dependent upon this nerve; but this we consider by no means established. It often happens in paralysis that the masticatory movements of the tongue are but little affected, when the power of articulation is much injured; and although there can be no question that the lingual branch of the fifth is principally distributed to the surface of the tongue, we do not think that its participation in its movements (which might be *a priori* expected from its origin in the third division of the trigeminus) has been altogether disproved. There is a curious circumstance attending paralysis of this nerve in cases of hemiplegia,—that the hypoglossal is not always palsied on the same side with the facial, but sometimes on the other. This is perhaps due to the origination of the roots of this nerve from near the point at which the pyramids of the medulla oblongata decussate; so that some of its fibres come off without crossing, like those of the spinal nerves; whilst others are transmitted to the contrary side, like those of the higher cerebral.

CHAP. XI. *Par Vagum.* We shall give a less full analysis than we otherwise should have done of Valentin's account of the functions of this nerve, since he does not seem to have become acquainted with Dr. Reid's very full, and to our minds very complete and satisfactory, elucidation of them.* His most important original observations, however, with his general conclusions, we shall detail. The result of his first experiment is very unexpected. He states that *at its roots* the par vagum possesses no direct motor power. If these be carefully separated from those of the glosso-pharyngeal, and (which is a matter of some difficulty) from those of the spinal accessory nerve, and be then irritated, no movements of the pharynx, œsophagus, stomach, larynx, trachea, or heart, can be seen in the irritable body of a horse, dog, or rabbit. This experiment requires great care; and various precautions are mentioned, to which those who repeat it should attend. In the living rabbit, a slight irritation of the roots of this nerve appears to produce great pain, although this does not seem to last so long as when the trigeminus is divided. Such irritation produces muscular motions, when the part irritated is in connexion with the nervous centres; but not otherwise. Whence it is clear that these motions are of a reflex character, and that the nerve possesses *in itself* no motor endowments. The spinal accessory nerve, as will hereafter appear, is principally or entirely of a motor character; so that the idea of Arnold and Scarpa, that the par vagum and spinal accessory are together analogous to a spinal nerve; the one answering to the posterior, and the other to the anterior roots, appears sufficiently probable. When the par vagum swells into the jugular ganglion, an interchange of fibres takes place between it and the spinal accessory; but many more fibres pass from the latter to the former, than from the former to the latter. Hence it results that, of the branches into which the par vagum subsequently divides, many enjoy a high degree of motor power; whilst those of the spinal accessory do not appear to possess any great share of sensibility.

In regard to the *pharyngeal* branch, Valentin's opinion coincides with Dr. Reid's, that it is chiefly motor, and that it is in fact *the* motor nerve of the pharynx. He states that it seems also in some degree a nerve of sensation; for, when the glosso-pharyngeals on both sides had been divided, tickling of the fauces produced vomiting. Nevertheless, he observed, as Dr. Reid had done, that the indications of sensibility afforded on pinching the trunk of this branch, were very feeble. The explanation seems to us to consist in this, that the power of conveying such impression as shall excite reflex actions, does not necessarily involve sensibility, as is shown by the action of the lower part of the œsophagus. Valentin states that the anatomical derivation of the pharyngeal branch harmonizes with his view of its chiefly-motor function; Bendz having ascertained that the larger portion of it proceeds from the spinal accessory. As to the endowments of the *laryngeal* branches, also, Valentin's general conclusions agree with Dr. Reid's. He states that the superior laryngeal is especially a sensory nerve, though it may have slight motor power; but that the inferior laryngeal is, on the contrary, almost solely a nerve of motion, although perhaps possessing a slight degree of sensibility. And he seems to think it probable that the movements of the

* See Vol. VIII. p. 264; and Edinb. Med. and Surg. Journal, vols. xlix. and li.

larynx, excited by irritation of the former, are in reality of a reflex character; but this he has not determined, as Dr. Reid has done, by direct experiment. The branches of the vagus which are distributed to the thoracic and abdominal viscera appear to possess both motor and sensory functions. The latter may be inferred from the indications of pain afforded by the animals experimented on, when these branches are irritated.

Before passing to the consideration of the special influence of this nerve on the organs it supplies, Valentin refers to the contraction of the pupil observed after division of the vagus in the neck, in those animals in which the sympathetic anastomoses freely with it, as in the dog, which was long ago observed by Petit, and recently confirmed by Dr. Reid. It seems doubtful whether this effect is produced through the sympathetic or through the vagus. Valentin states that, if the inferior ganglion of the latter be removed, in black rabbits, a decided contraction is produced; though a less manifest effect is produced in white rabbits, by division of both the sympathetic and vagus lower down in the neck. On the other hand, Dr. Reid states it as the result of his experiments on the cat (in which animal the sympathetic and vagus are easily separated in the neck), that section or even compression of the sympathetic instantly produced contraction of the pupil; whilst injury of the par vagum was followed by no such effect. Valentin states that the signs of sensibility given by the cervical portion of the vagus are very indistinct, when the animal has previously suffered much pain; and in this way the contradictory results obtained by experimenters may probably be accounted for.

That the movements of the heart are influenced by the cardiac branches of the vagus, Valentin states that he has obtained distinct evidence; convulsive actions being excited, after the regular movements have ceased, by irritating the cervical portion of the nerve (the result of this experiment, however, being much influenced by the mode of death). But this subject will be further considered when the functions of the sympathetic are enquired into. Passing over the special influence of this nerve on the larynx, as having been much more satisfactorily elucidated by Dr. Reid, we come to the connexion it has with the functions of the respiratory organs. The mucous surface of the trachea and bronchi appears (according to Valentin) to be endowed with some degree of sensibility by branches of the inferior laryngeal; and he remarks that reflex movements may be more easily excited by irritating the surface than by stimulating the trunk of the nerve, which corresponds with what we consider to be a general law of the excito-motor system. He has seen also distinct contractions of the rings of the trachea produced by irritating this trunk in a rabbit; and he thinks it probable that the pulmonary plexus accompanying the subdivisions of the bronchi has a similar influence upon their diameter. No such movements can be seen however in the horse. As to the influence of this nerve on the respiratory process, we find nothing new. All the changes consequent upon section of it he attributes (as we think, with perfect justice) to the insufficient introduction of air into the lungs. But the source of this deficiency he seeks for in the diminution of the opening of the glottis; and he quotes from *Arnold* the statement that the number of respirations is increased by the

operation. Now Dr. Reid has shown that, when free entrance is given to the trachea, the usual effects are still observed; and he attributes them to *diminution* in the number of the respiratory movements, which he considers to be the immediate effect of the partial interruption of communication between the lungs and medulla oblongata, so that the transmission of the necessary stimulus is cut off; and he points out the cause of the idea that these movements are increased in frequency, which many observers have entertained, as he himself did in the first instance.

In order to understand the influence of the par vagum on the function of digestion, Valentin remarks that we must examine three different conditions: 1, its connexion with the movements of the pharynx, œsophagus, and stomach; 2, its influence on the secretion of the gastric juice; 3, the variation which the supply of the stomach with imperfectly arterialised blood will produce. In his opinion, the movements of the alimentary tube are directly affected, the secretion of gastric juice unaltered, and an alteration in the character of the blood indirectly produced, by section of the par vagum. In regard to the first point, his experiments are not sufficiently full, no attempts having been made by him to discover how far the motions of the œsophagus result from a reflected stimulus operating through this nerve. This, indeed, he seems to have taken for granted, in consequence of the distribution of its branches upon the muscular parietes; but from the experiments of Dr. Reid, it would appear that this influence does not go beyond a certain point, which varies in different animals, and that the contractions occurring below this result from the *direct* stimulation of the muscular fibres. Valentin states that the influence of the par vagum on the muscular coat of the stomach, which has been asserted by some experimenters and denied by others, may be most distinctly demonstrated in the rabbit. The experiment is less likely to succeed, if the irritability of the stomach, which is soon lost after death, should be already on the wane, and if the stimulation of the nerve should be performed too high up. The result is seldom wanting, when, in a newly-killed animal, the nerve is irritated in the lower part of the neck, or in the thorax. He further states that the par vagum appears to be the sensory nerve of the stomach, communicating the impressions which produce the sense of hunger or of satiety. Dr. Reid's experiments, however, show that, though this is not improbably true, there is some other source of hunger; as animals who have sustained section of the nerve on both sides will eagerly take food, if they have not received too great a shock from the operation. That when this is the case, however, no feeling of satiety is experienced when the stomach is loaded, Valentin concludes from experiments similar to those of Baglivi, Legallois, and others; but he states that it may be best seen in puppies, who, after the operation, will take three times, and even more, the same quantity of milk as uninjured individuals of the same age, so that the abdomen is greatly distended. He considers it also sufficiently proved that vomiting may take place as a reflex action, excited through the sensory portion of this nerve; although it may be produced also in other ways.

That the par vagum has no direct influence on the secretion of gastric juice, either as to quality or quantity, is the conclusion at which Valentin arrives; not so much, however, from his own experiments, as from those of Arnold, Broughton, and others. On this point, however, the experi-

ments of Dr. Reid have left little to wish for. Its immediate influence upon the function of digestion, therefore, he regards as confined to the movements of the stomach, by which that incorporation of the gastric juice with the whole mass of food is effected, which is essential to its solution. He does not seem to be aware that animals may live for a considerable length of time, and digest enough food for their nourishment, after complete section of the nerves on both sides, as Dr. Reid ascertained. It is probable, however, that the motor influence of the par vagum on the stomach, as on the œsophagus, varies in different animals; and that in some, the movements of the stomach may be nearly independent of it, though capable of being influenced by it. We cannot but regard the independence of the secretion of the gastric juice of any nervous influence as a point now fully established, as far as the par vagum is concerned; and we trust that we shall not be doomed to see Dr. Wilson Philip's often-quoted experiment transcribed from one work to another as an established fact, just as if it had not been proved to be of no value whatever. Verily, there *are* as many false facts as false theories in physiology! On the remainder of the alimentary canal, Valentin does not think that the par vagum has any direct action. No renewal of movement can be observed, upon irritation of the nerve, when it has previously ceased; and absorption, as well as secretion, appear to be properly performed. The inferences deduced from observation of pathological phenomena in man, confirm, so far as they extend, those derived from physiological experiment.

CHAP. XII. *Spinal Accessory Nerve.* From the roots of this nerve, as already explained, all the motor properties of the par vagum appear to be derived; no indications of sensation can be obtained by irritating them. When the external branch of this nerve is irritated, the trapezius and sterno-mastoideus are thrown into action, as remarked by several observers, and some, though not violent, indications of pain are given; these, probably, depend on admixture of the sensory fibres derived from the par vagum. In this respect Valentin's observations coincide with those of Dr. Reid, and also in the fact, that section of the nerve does not prevent the trapezius and sterno-mastoideus from sharing in the automatic movements of respiration, which Sir C. Bell imagines to depend upon it.

CHAP. XIV. *Phrenic Nerve.* It appears to be sufficiently established that this is the sole motor nerve of the diaphragm, and acts upon it alone. The movements of the heart and stomach, which have been described as resulting from stimulation of this nerve by galvanism, have not been witnessed by Valentin, although carefully looked for in a great number of trials. Moreover, no movements can be produced in the diaphragm by irritating the vagus or sympathetic nerves; so that its action would seem to depend on the phrenic alone. There is reason to think that this nerve contains sensory as well as motor fibres; signs of pain being given when it is cut or compressed.

The SECOND BOOK is devoted to the Sympathetic system; the true nature of which is briefly discussed in the *first chapter*. Contrary to the opinion of most other physiologists, he does not regard it as a system complete in itself, but as entirely derived from the cerebro-spinal; its most prominent difference from the branches of that system being the

repetition of its ganglia, so that the ganglionic influence is communicated to the fibres several times in their course. We may remark, however, that this distinction does not hold good in regard to the analogue of the cerebro-spinal system in articulated animals, in which there is a similar repetition of ganglionic centres—these being continuously united in the spinal cords of vertebrata. As in the ganglia of other nerves, so in those of the sympathetic, nucleated globules are seen, which are surrounded by sheaths of their own, and are either superposed on the bundles of fibres, or are dispersed through the plexus. Their sheaths are prolonged, with certain fibres peculiar to them, amongst the primitive nervous fibres proceeding from the ganglion. But, according to Valentin, these peculiar fibres are not nervous, as maintained by Remak and Müller.

CHAP. II. *On the Motor Functions of the Sympathetic Nerve.* These may be ascertained by experiment, not so well on the living body as on the dead body still irritable. They are dependent upon the fibres transmitted from the cerebro-spinal system; and they may be reduced to laws as definite as those which govern the movements of other parts of the body. But as the parts supplied by this nerve may be excited to motion otherwise than by a stimulus transmitted through it, experiments upon its functions are liable to the influence of many disturbing causes, and, to be satisfactory, must be frequently repeated. The results stated by Valentin have been obtained by experiments on more than three hundred animals—principally horses, sheep, and rabbits; dogs and cats being less employed, not because the experiments are the less successful in them, when performed under the same conditions, but because they very rapidly lose their irritability after death.

The first series of results relate to the *vascular system.* Valentin states that, with certain precautions which he enumerates, motions of the *heart*, more or less decided, may be excited by irritation of the roots of the spinal accessory nerve, and of the first four cervical nerves: also of the trunk of the par vagum, between the larynx and thorax; of the lowest cervical, and (in a slight degree) of the first thoracic ganglion of the sympathetic; and of the cardiac plexus. Hence it appears that the motor fibres of the last-mentioned nerves are derived from the first; and it is only when the cervical portions of the vagus and sympathetic are uninjured, that irritation of the roots of the spinal nerves produces any effect. He thinks that he has witnessed a distinct contraction of the thoracic aorta, the inferior cava, and the thoracic duct, upon irritation of the neighbouring portion of the sympathetic system; but upon this point he does not speak with certainty.

The second series refers to the *digestive system.* According to our author, the *pharynx* is strongly contracted on irritation of the roots of the accessory and first two cervical nerves, as well as of the pharyngeal branches of the vagus. The lower part of the *œsophagus* is made to contract peristaltically from above downwards by irritation of the cervical portion of the sympathetic in the rabbit, and of the sympathetic and vagus, which are united, in the dog and horse; also by irritation of the roots of the first three cervical nerves, and more rarely of the accessory. The thoracic portion of the œsophagus is made to contract by irritation of the lowest sympathetic ganglion of the neck, and of the higher cervical ganglia, of the œsophageal plexus, and of the roots of the lower

cervical spinal nerves. Muscular contractions of the *stomach* are produced by irritation of the cervical part of the vagus, and also of its thoracic and abdominal portions; the lower the part of the nerve irritated, the nearer to the pylorus do the effects manifest themselves. Irritation of the roots of the fourth, fifth, sixth, and seventh cervical nerves and of the first thoracic in the rabbit produces very distinct contractions of the stomach, so that a complete furrow is evident between the cardiac and pyloric portion of the viscus; and the lower the nerve irritated, the nearer the pylorus do the contractions extend. Irritation of the first thoracic ganglion of the sympathetic produces the same effect. Hence it appears that the motor fibres of the stomach proceed from the four lowest cervical and the first (and in the sheep the second) thoracic nerves. Contractions of the *intestinal tube*, varying in place according to the part of the spinal cord experimented on, may be excited by irritation of the roots of the dorsal, lumbar, and sacral nerves, and of the trigeminus. In the horse and cat, a distinct peristaltic action of the small intestines has been seen on irritating the roots of the oculo-motor nerve; but this has not been noticed in the dog and rabbit, and in the cat a similar motion was once observed on irritating the roots of the accessory nerve. Similar effects to those obtained by experiments on the spinal nerves were produced by irritation of the lower part of the thoracic portion of the lumbar, and of the sacral portions of the sympathetic; also of the splanchnic branches, and of the gastric plexus. A peristaltic contraction, evident though slight, of the *ductus choledicus*, was produced by irritating the right splanchnic nerve (r. major).

The movements of the *respiratory system* are produced rather through the vagus than through the sympathetic nerve.

The motions of the *uropoiëtic system* may, like those of the alimentary canal, be excited by irritation of the roots of the spinal nerves, and of the sympathetic, which contains the fibres of these. A very distinct and powerful peristaltic action of the *ureter*, proceeding from the kidneys to the bladder, may be produced by irritating the abdominal ganglia, or the roots of the abdominal spinal nerves. Strong contractions of the *bladder* are excited by irritation of the inferior portion of the abdominal sympathetic, but especially of the sacral portion; and by irritation of the roots of the middle and inferior abdominal nerves of the spine. In these, as in former cases, no effect is produced by irritation of the spinal nerves, unless the portion of the sympathetic connected with the particular organ is entire.

Of the *genital system*, Valentin has found that contractions may be excited in the vas deferens and vesiculæ seminales, especially in the guinea-pig at the time of heat, by irritation of the inferior lumbar and highest sacral portions of the sympathetic; and also that contractions of the fallopian tubes and of the uterus itself, from the fundus to the neck, may be excited by irritation of the same nerves as those which excite the rectum, namely, the lower lumbar and first sacral nerves of the spine. This contraction was sometimes much less in the gravid than in the unimpregnated uterus.

The following inferences are drawn by Valentin from these experiments: 1. That all the parts which exhibit involuntary movement are excited by action, like voluntary muscles, by stimuli applied to the

which they are endowed. 2. That from whatever part of hetic system their nerves arise, their actions are governed by aws. 3. That the sympathetic system has the following re- other nerves: *a*, its motor fibres are distributed to remoter e body; *b*, still, throughout their long course, there is no between them, so that definite contractions are excited o the fibres irritated, as in other nerves; *c*, that these motor ally proceed from the cerebro-spinal system, and that irrita- r origins acts through the sympathetic trunks.

rs therefore that, although any particular division of the c nerve must be regarded as extremely complex in its charac- g its motor fibres from many different sources, the ultimate of these fibres is sufficiently simple, so that each organ is upplied from a certain part of the cerebro-spinal axis. But roceeding from the roots of the cerebro-spinal nerves do not e nearest organs, but are transmitted through three or more the sympathetic, before being distributed on them. In this roscopic examination fully bears out the results of experi- e great bulk of the fibres which go forth from one of the tho- dominal ganglia come into it by the trunk with which it com- with the one above; a few only originating in the ganglion

lts of his own experiments are then applied by Valentin to he contradictory accounts of other physiologists, of which he y complete summary. In doing this, he adopts a view sub- developed more fully, that the movements of the heart, ali- nal, &c. are ordinarily of a reflex character, and that they the living animal excited by those *direct* stimuli by which e caused to take place in the dead body, when cut off from influence. On the merits of this doctrine we shall hereafter

d to the *sensory* functions of the sympathetic nerve, discussed d chapter, Valentin states as the result of his experiments on inal ganglia, that a slight irritation, whether mechanical or oes not occasion pain immediately after the abdomen has been t that a more powerful irritation, as by compression, ligature, excites not only pain but vehement cries. The longer the as been exposed to the air (of course within certain limits) the ly does irritation of it cause pain. The cords which pass out anglia are not so readily excited by slight irritants as the which enter them, and the branches which communicate e ganglia and the spinal nerves are sensory to such a degree ly any difference can be perceived between them and the es themselves. It is not as easily proved with regard to the with respect to the motor fibres of the sympathetic system, roceed from the spinal nerves; nevertheless, there can be t that this is the case, as Valentin clearly shows by many and to these are due all the sensations which are referred to in health or disease.

ourth chapter, the origin of the nervous fibres of the sympa- riefly treated of. These he considers, in opposition to the

opinion of most anatomists, to proceed ultimately from the cerebro-spinal system; and he does not recognize the gray or organic fibres which are described by Remak and Müller. To this point we shall return hereafter, when we follow our author in his account of the nucleated globules. He states that the sympathetic in the neck is at first developed in the same manner as in the lower part of the trunk, each vertebra having a corresponding ganglion, but that a metamorphosis afterwards takes place, some of these disappearing and others being more fully evolved.

In the THIRD BOOK are discussed the laws which govern the actions of the peripheral nerves; and the *first chapter* is devoted to an exposition of their *physiological symmetry*. That of the spinal nerves is obvious enough, but that of the cranial is more recondite. Of these last, the vagus, spinal accessory, glosso-pharyngeus, and hypoglossus are to be regarded in the light of spinal nerves; and it is only, therefore, the first seven pairs that can be properly termed cerebral. The account given of their embryonic development is very curious and interesting, and throws great light upon their varieties of distribution in different classes. The three nerves of special sense arise, as has long been known, from the three vesicles which, at an early period, indicate the primary divisions of the encephalon. Afterwards, when the cranial vertebræ are developed, other nerves, intervertebral, are interposed among them. A separation of the primitive fibres of these, however, takes place, so that their distribution appears irregular. Thus the greater part of the sensory fibres are contained in the larger division of the trigeminus; whilst the motor seem to be chiefly transferred to the oculo-motor and patheticus. Again, of the primary anterior motor fibres, some form the small division of the trigeminus, whilst others unite with the first pair from the medulla oblongata to form the facial. Hence is understood the union of this nerve with those proceeding from the medulla oblongata in fishes and some amphibia. In regard to the medulla oblongata, which is gradually metamorphosed (as it were) from the medulla spinalis, it is to be noted that its nerves are all in the first instance double-rooted; but that the motor fibres of the first pair unite with those of the facial, whilst its sensory form the glosso-pharyngeal; whilst the second and third pairs become blended, the motor portion becoming the accessory and the sensory the par vagum. Our author lays much stress on the more complete metamorphosis or alienation from the character of spinal nerves, which may be traced in the anterior pairs of cerebral nerves, and on the gradual nature of this change, when traced upwards from the spinal cord, and he points out that, even of the special sensory nerves, the character of the olfactory is most unlike that of the spinal, whilst that of the auditory is most allied to it. He also notices that the relation of the olfactory with other sensory nerves is greater than that of the optic, and this last greater than in the auditory, and that the contrary is true regarding the motor fibres.

The division of the body into vertebræ or rings affects the formation and distribution of the nerves, as well as of other parts. According to Valentin (who thus confirms a statement formerly made on this point), the chain of sympathetic ganglia seems to be the first centre of the nervous system; the spinal nerves, or at least those which are subsequently

to become so, rather coming off from the sympathetic than from the spinal cord. But, in process of time, the small cords by which they were connected with the spinal axis become the true roots, and those which proceeded from the sympathetic ganglia shrink into connecting branches. Some other very curious relations between the sympathetic and cerebro-spinal systems (for an account of which we must refer to the original) are unfolded by this kind of study. Those ganglia of the sympathetic system, with one of which every organ of sense is provided, are regarded by Valentin as having for their office to harmonise the motions of the external parts, and bring them in relation with the sensory actions of the organs themselves. Other kinds of symmetry in the nervous system are then pointed out; but these we may pass over, as of less special interest.

CHAP. II. *Of the Primitive Fibres.* That these, wherever they occur, serve only as conductors of the agency to which the nervous system ministers, can now scarcely be denied. This agency, of the actual nature of which we as yet know nothing, originates in the gray substance of the central organs, and in certain parts connected with the peripheral extremities of the nerves; and it depends on the peculiar relation of this system with the vascular. These two systems are denominated by Valentin *concreto-generalia*, being distinguished from other systems of the organism in this, that each forms, as it were, a whole, possessed of certain general properties, which undergo but slight modification in the several organs to which they are distributed, the peculiarity of *these* being rather adapted to *them*; so that the speciality of the actions depends upon the added or altered structure, and not upon anything peculiar in the operation of these systems. Such, at least, we understand to be Valentin's idea, which he refers to as more fully developed elsewhere. By the almost constant changes occurring in the nervous system, a continual action of some kind is maintained, both from the periphery towards the centre, and from the centre to the periphery; and this action seems to be intimately connected with the nutrition of the nerves themselves, since, if it be interrupted for any length of time, they lose their peculiar endowments. The peculiarity of the actions of the several kinds of fibres seems to depend more on the peculiarity of their terminations at the two extremities, than in anything in their own structure; and the specialities of sensory impressions are probably to be chiefly referred to the structure of the organs surrounding the peripheral terminations of the nerves. The influence of the conditions of the vascular systems on their functions is afterwards pointed out; and on this, as well as the foregoing points, his views completely correspond with those which we ourselves expressed not long since. (Vol. IX. p. 99.) A somewhat lengthy discussion of the primary changes, which are probably subservient to *objective* and *subjective* perceptions then follows; but this we shall not attempt to analyze, referring such of our readers who are interested in these abstruse metaphysico-physiological questions to the work itself. The general laws regarding the transmission of impressions along the nerves are also enumerated; but these, also, we shall pass by, as not containing anything of novel interest, though characterized by great clearness and caution in generalizing.

CHAP. III. *On the Formation of Ganglia.* That which essentially

constitutes a ganglionic formation is the interposition of nucleated globules among primitive fibres, which have more or less of a plexiform arrangement. It is, therefore, erroneous to confound the *ganglionic* system with the *sympathetic*, since ganglionic formations occur in the cerebro-spinal system, as well as in this. A copious summary is given by Valentin of the opinions of authors as to the functions of ganglia, and he then proceeds to set forth his own, which proceeds upon the fundamental dogma (of the correctness of which we by no means feel satisfied), that the functions of the ganglionic structure are everywhere essentially the same. In the first place, he remarks, a ganglion does not produce any essential change in the endowments of the fibres which pass through it; sensory impressions are not reflected by them into motor impulses, but the branches which proceed from them contain a much greater admixture of fibres than those which enter. Ganglionic formations are found almost exclusively connected with sensory, or at least afferent nerves. The nucleated globules of the ganglia appear essentially to correspond with the globules of the gray substance. But the fibres of the central masses differ from those of the peripheral nerves in having their sheaths more slender and more similar to the coverings of the nucleated globules, which in the gray substance are so delicate that the globules themselves can with difficulty be distinguished, the whole mass having a tendency to form a soft colliquamentum. In the peripheral ganglia, however, the colliquamentum is firmer, the globules can be easily distinguished, their sheaths are thicker, and can be observed to send out processes amongst the primitive fibres; but there are among them several varieties, some of which indicate a near approach to those of the central system. Now, since the peripheral nucleated globules do not give rise to any actions of their own, it follows that their functions must be connected with those of the central system; and it may be inferred that they add strength to the operations of the nervous fibres which pass through them, especially to sensory impressions; perhaps, also, rendering the motor impulses of an automatic and periodic character more independent of the will. This general view of the actions of the peripheral ganglia is carried out into particulars with great ingenuity, but we cannot regard it as based on a solid foundation. The observations of Remak concur with those of Müller in leading to the conclusion, that the nucleated globules of the peripheral ganglia are related to a peculiar set of fibres, termed by them the gray or organic; which abound in the sympathetic system, constituting, indeed, its peculiar character, and which exist, in a less degree, in the cerebro-spinal, being mixed with them just as the fibres of the cerebro-spinal are mixed with the sympathetic. Admitting, therefore, all that Valentin has proved by experiment as to the influence of the cerebro-spinal system on the motions of the parts to which its fibres are distributed through the medium of the sympathetic, we cannot but think that this last has an independent action peculiar to itself (and to its ramifications scattered through the cerebro-spinal nerves), which we have on former occasions attempted to define. This is the portion of Valentin's treatise which seems to us least satisfactory.

CHAP. IV. *On Reflex Action.* In this chapter we find a full summary of the facts established by the late researches of Dr. M. Hall, Müller, Volkmann, and others; which, having on former occasions

pretty fully discussed, we shall not now revert to. In the opinion of Valentin, the reflexion of motions is not dependent upon sensations; the former being performed by the spinal cord or its segments independently of the brain which is the seat of sensibility. But he does not distinctly avow his belief in a separate system of fibres appropriated to the excito-motor or, as he denominates it, the *excitato-excitatorial* function; although from his previous statements respecting the distinctness of the endowments of individual fibres, it may be inferred that he holds that doctrine. He constantly speaks, however, of motor fibres being excited by *sensory;* which, on his own showing, is untrue. It is much to be wished that physiologists would abandon the use of this term, where the excitement of a sensation is not implied. The *direction* of the fibres,—afferent or efferent—centripetal or peripheral,—is in our minds a much better way of expressing their general endowments. According to Valentin, the motions of the heart and alimentary canal are ordinarily of a reflex character; and here sensation is certainly not excited. This, our readers will remember, is the doctrine long ago advocated by Whytt. That a motor impulse *may* be communicated from the spinal nerves to these muscles is, however, no proof to our minds that their actions are *dependent* upon it; and it only points out the channel through which, as every one knows to be the case, they are affected by particular states of mind, or by conditions of the general system. The performance of regular movements under the influence of direct stimuli, when these parts have long been separated from the nervous centres, and the analogy of these movements with those exhibited by plants (which we have formerly taken occasion to point out) are, in our opinion, quite sufficient evidence in favour of the Hallerian doctrine, especially when coupled with the fact, for such we esteem it, that the property of contractility is everywhere inherent in muscular fibre, and that the stimulus of innervation is only *one* of the excitants by which it may be called into action.

In the FOURTH BOOK is considered the influence of the peripheral nerves on the several functions. The *first* chapter is devoted to the special physiology of the senses; and in this a summary of our previous knowledge of the subject is combined with many interesting views of the author's own; but these it would be difficult to present out of their connexion. One of the most interesting novelties is an extension of Weber's table of the distances at which two points can be separately distinguished in various parts of the surface of the body. Four additional sets of measurements by different individuals, varying in some particulars from Weber's and from each other, are given in a comparative form; and Valentin's own are added, with a column showing the mean of all.

CHAP. II. *On Motion.* After indicating the passive organs of locomotion, Valentin proceeds to consider the structure which actively contributes to the performance of this function—muscular fibre. He describes the two conditions in which it exists; in the muscles of voluntary motion and some others, the fibres being composite and marked with regular transverse striæ; whilst the muscular tissue of the alimentary canal and of the excretory ducts has the ultimate fibres separated and irregularly woven together. He then discusses the physiology of muscular action; and remarks very truly that no powerful contraction

can be excited in muscular fibre, except by stimuli acting through the nervous system. But he does not thence infer, as many do, that the *contractility* of the muscle is dependent upon nervous action; but fully recognizes it as an inherent power, although in some way dependent for its continuance upon the integrity of the portion of the nervous centres with which the muscle is connected. On this point his views appear to correspond closely with those recently expressed by Dr. M. Hall. They are liable to the objection that, as Dr. J. Reid has shown, the irritability of a muscle after being exhausted may be recovered, even though deprived, by the section of its nerves, of all nervous agency; and there is no more difficulty in understanding why the peculiar property of a muscle should not be diminished and finally lost by the alteration in its nutrition consequent upon the cessation of its ordinary functions, than why the same should happen in other organs, as it is very well known to do. The investigation into the relations of nerve and muscle is of considerable length, and contains several facts of much physiological interest. A minute comparison is instituted between the composite and simple muscular fibres; but the general conclusion—that the former are more readily excited than the latter by stimuli acting through their nerves, and less readily by stimuli directly applied to themselves—does not differ from that ordinarily maintained. To Valentin, however, is due the merit of determining, with much more certainty than had been previously attained, that the simple muscular fibre *can be* excited to contraction through the nervous system. How far its ordinary actions are dependent upon such stimulation will be hereafter questioned.

A remarkable series of experiments on the brain and spinal cord of the frog then follows; having for their object to determine the connexion of their several parts with the movements of different organs of the body. The following are amongst the most interesting results: 1. In the frog, the motor fibres of the two sides of the medulla oblongata decussate just before they arrive at the fourth ventricle; so that the decussation affects those of the base of the cerebellum. 2. Neither the superior nor inferior strands (posterior and anterior columns in man) of the spinal cord solely possesses motor functions; but when the former are irritated sensations predominate, and when the latter, motions are chiefly excited. 3. If the *superior* column of the spinal cord be irritated at the point at which the nerves of either extremity are given off, that extremity is *extended;* if the *inferior* be irritated, the extremity is *flexed.* 4. Unless the irritation is excessive, or the will of the animal interferes, irritation of one side does not affect the other. At their entrance into the spinal cord, therefore, it would appear that the motor fibres of the extensors pass towards the superior stratum (posterior column in man), whilst those of the flexors are continuous with the inferior stratum; but their course is found to be altered, when they are examined at some distance from their point of exit. This doctrine is confirmed by experiments on mammalia; and by pathological phenomena in man. It fully explains the facts which led Bellingeri to maintain that the anterior roots of the nerves were for flexion, and the posterior for extension. By Valentin the Bellian view of the functions of the roots is rigorously upheld; and it is only in the account of the course of their fibres in the spinal cord, that he differs from the discoverer of their true endowments,

According to Valentin, relaxation of the sphincters is analogous to the extended state of the extremities; and he has noticed a relaxation of the sphincter ani in the frog, when the posterior part of the spinal cord was irritated so as to produce extension of the limbs. Further he states that the peristaltic and antiperistaltic motion of the intestines occurs under the same conditions respectively with the flexion and extension of the extremities.

The results of various experiments already recorded, on the relation of different parts of the nervous centres to muscular motion are then fully discussed; and lastly, the general laws of involuntary, automatic, and voluntary motion are laid down.

CHAP. III. *On Digestion.* In this chapter is considered the influence of the nervous system on the movements of the various parts of the alimentary canal, on which we have already sufficiently enlarged; and its influence upon the functions of secretion and assimilation. That the secretions necessary for digestion are not checked by section of the nerves has already been mentioned; and arguing from this, Valentin endeavours to make it appear that the functions of the solar plexus and its ramifications on the glandular system are confined to the maintenance of the movements of the alimentary canal, of the contractions of the ducts of the glands, and of the tonicity of the minute subdivisions of these.

CHAP. IV. *On the Circulation.* In this chapter Valentin attempts to prove that the rhythmical contractions of the heart are performed by *reflex* stimulation; and that the ganglionic plexus accompanying the vessels regulates their contractions. In reference to the first point he admits that the regular contractions of the heart removed from the body, continuing for a long time, and capable of being excited after they have ceased, is a considerable obstacle to his theory; in our minds it is an insuperable one.

CHAP. V. *On Respiration.* The several movements of respiration are discussed; and rightly attributed to reflex action, capable of being controlled by the will to a certain degree: according to our author, the nervous system has no direct influence on the chemical changes to which these movements are subservient.

CHAP. VI. *On the Preparation and Excretion of Urine.* This secretion affords great facilities to the experimenter, on account of the comparative facility with which all the nerves supplying the organ may be destroyed without interrupting the circulation. The results obtained by previous physiologists are collated by Valentin; and details of his own experiments are added. By these, and by pathological phenomena, he considers it proved that the secretion is not checked by the complete cessation of nervous influence; and that whatever change takes place in its character may be explained without the supposition that nervous agency has any direct influence on the act of secretion. Of the movements of the ureters and bladder an account has already been given.

CHAP. VII. *On Generation.* The formation of the seminal secretion is not influenced by the nervous system; but its ejaculation is as reflex movement excited through the spinal nerves. Erection of the penis and clitoris are regarded by Valentin as of a similar character; but though, when the action is commenced it may be increased by reflex stimulation of the muscles, we cannot regard the afflux of blood by which it is pri-

marily effected as having any such cause. The movements of the fallopian tubes and of the uterus have been already noticed.

Chap. viii. *On the Nutrition and Reproduction of Parts.* The influence of the nervous system on these functions is not regarded by Valentin as any greater than on that of secretion. That they are affected by it is clearly proved by experiment and by pathological observation; but there is equally good reason to believe that they may go on, though with less than their normal regularity, when all nervous influence is cut off. He does not seem able to imagine that such nervous influence can consist in anything but the power of exciting motion; and it is, therefore, to the reflex actions of the blood-vessels, secretory ducts, &c., that he attributes the ordinary agency of the nerves upon these functions. He denies, as we have formerly hinted, the existence of those organic or gray fibres which have been described by Müller and Remak, as essentially constituting the sympathetic system. He conceives that the filaments, so denominated, are in reality extensions of the sheaths of the nucleated globules, which are formed of a sort of cellular tissue composed of primitive fibres interwoven together, and furnished with a kind of epithelium. This is not to us, however, by any means a satisfactory account of them; and we are much more disposed to believe, with Müller and Remak, that there is a distinct system of nervous fibres, to which the nucleated globules in the ganglia are the centres, specially connected with the organic functions, and designed to harmonise and blend these together, independently of any direct action of the cerebro-spinal system. That the great bulk of the so-called sympathetic nerve consists of fibres derived from the cerebro-spinal system, we can well believe, when it is considered how many are the influences exercised by particular states of mind upon the organic functions, and how close must be the connexion between these last and the animal functions which depend upon them for their continuance. And we think that it is rather too much a *mechanical* view to limit the operations of the nervous system to the production of motion. But in his general proposition that the sympathetic system of nerves does no more than direct and control the functions of nutrition, secretion, &c., the readers of this Journal need scarcely be informed that we fully participate; and it gives us much satisfaction to find this view adopted by so high an authority. The chapter concludes, and with this closes the volume, with an account of Valentin's own observations on the regeneration of nerves,—an occurrence of which he has fully satisfied himself, when the interval is not too large, and the trunk of sufficient size. In one instance a piece of the ischiatic nerve of the rabbit was cut out, to the extent of eight lines, and the injury was completely repaired after two or three months. The process of reproduction is very much what might have been predicted from observation of the embryonic development of the nervous system. At first the space is filled with lymph, which gradually becomes organized into cellular tissue. A yellowish oily substance is then seen to exude into this from the two ends of the nerve; and it permeates the cellular structure in both directions, so as to meet midway between the ends. Around its particles, which finally become white, the fibres of the cellular tissue are seen to arrange themselves,—at first separating it, as it were, into fasciculi,—and afterwards *into* what become the primitive nervous fibres.

Having thus brought our analysis to a conclusion, we would again express our conviction of the great importance of this work, and of the high rank which we think it ought to take amongst physiological treatises. We have never met with a work on any subject in which so much original matter of sterling value is so judiciously mixed up with the knowledge previously obtained from other sources. With all but the most recent enquiries prosecuted in this country (of which some of the most important have been made public subsequently to the completion of his labours), he shows himself well acquainted. We have thought it right to indicate, as we have proceeded, the chief points on which we differ from him; and these rather concern matters of mere speculation, than direct deductions from experiment and observation. There are many of his results which we should be glad to see confirmed in this country; but, when once fully established, we trust that no superfluous animal suffering will be inflicted, for the mere purpose of *exhibiting* them. There are still questions enough to be decided, to require as large a sacrifice of dogs and rabbits as the humane physiologist will feel at ease in offering upon the altar of science.

ART. II.

. *Traité de Pathologie externe et de Médecine opératoire.* Par AUG. VIDAL (des Cassis), Chirurgien des Hôpitaux de Paris, Professeur agrégé à la Faculté de Médecine de Paris, &c.—*Paris*, 1839-40. 3 vols. 8vo, pp. 502, 486, 588.

Treatise on External Pathology and Operative Medicine. By AUG. VIDAL, Surgeon, of Paris.—*Paris*, 1839-40.

. *Handbuch der Chirurgie zum Gebrauche bei seinen Vorlesungen.* Von MAXIMILIAN JOSEPH CHELIUS, der Medicin und Chirurgie Doctor, &c. &c. (*Vierte, vermehrte und verbesserte original Auflage.*) —*Heidelberg und Leipzig.* 2 vols. 8vo, pp. 806, 730.

Manual of Surgery for use at his Lectures. By M. J. CHELIUS, Doctor of Medicine and Surgery. *Fourth Edition.—Heidelberg.*

. *Elements of Surgery.* By ROBERT LISTON, Surgeon to the North London Hospital, &c.—*London*, 1840. 8vo, pp. 795.

. *A Dictionary of Practical Surgery.* By SAMUEL COOPER, Senior Surgeon to University College Hospital, &c. *Seventh Edition.—London*, 1838. 8vo, pp. 1518.

In some of the recent Numbers of this Journal, we have made it our business to lay before our readers a comparative review of the progress and present state of opinions and practice in some of the more important departments of surgery; we allude especially to the subjects of "inflammation, and ophthalmic and reparative surgery," including under the latter title our articles on plastic surgery and tendon-cutting. It is our present intention to select and examine some of the less prominent divisions of our art, which, though they possess not the charm of novelty in theory or practice, are of sufficient interest to demand at our hands some attention in the continually advancing state of surgery. The subjects we propose for consideration are the following: 1, various forms of

wounds, their nature and treatment; 2, the effects of extreme heat and cold; 3, mortification and hospital gangene; 4, erythema and erysipelas.

Of the works before us, some are already well known to the profession. The Manual of Chelius has passed through several editions, and is in well-merited repute all over Germany; indeed, the simple arrangement, lucid style, and conciseness with which this excellent surgeon treats his subjects, makes it matter of wonder with us that no attempt has been made to introduce his work to our own students in an English garb. Of the present edition of Cooper's valuable Dictionary we have had occasion to speak in a recent Number of our Journal; it offers an excellent digest of surgical information, to which we may freely appeal with full confidence in the authenticity of the sources whence the information it contains is compiled. Mr. Liston's Elements of Surgery now likewise appears before us in a second and enlarged edition: it is a very valuable work. As the work of M. Vidal is the most recent treatise on surgery that has issued from the Parisian press, we shall select his volumes as our principal text-book, and compare the opinions and practice set forth in them with those of his English and German contemporaries.

In his first introductory chapter, M. Vidal considers the subject of surgical diagnosis, treating separately of the several senses, and the proper mode of employing them in aiding us in arriving at a correct conclusion regarding the nature and condition of diseased or injured parts. The second preliminary compartment comprises the subject of operations in general; with some valuable hints and observations regarding "the methods of operating;" "the time and place" as determined by necessity or with the ability to make an election; "operations which should be avoided, and others which are performed for the gratification of the patient;" "the conduct of the operator during and after operations," &c. The various modes of employing cutting instruments, and the description of the minor operations of surgery succeed; and thus the ground is cleared for the more immediate object of the treatise, namely, the description and treatment of the various injuries and diseases which come under the observation and care of the surgeon. We shall pass over the subject of inflammation in general, and proceed at once to the consideration of the different forms of wounds.

Wounds. Of the various forms of wound the most simple and tractable is that which is produced by a cutting instrument. It is not difficult to account for the readiness with which nature generally undertakes the cure of incised wounds, when the divided surfaces are placed in contact: there has been no injury or destruction of texture beyond simple division, and therefore there is nothing to be thrown off as dead and useless, and nothing to be reproduced: in other words, neither sloughing, ulceration, nor granulation with suppuration are called for, and the process of cure resolves itself into a simple reunion of disunited healthy surfaces. This subject therefore offers but few points for remark, except as regards, perhaps, the somewhat disputed question of the manner in which the reunion takes place. Surgeons are aware that, however carefully an incised wound may be cleansed, even till the blood has to all appearance ceased to flow, that still a layer of material which has been

ned plastic lymph, is poured out by the vessels, and is therefore inter-ed between the opposed surfaces. It was Hunter's opinion that this tion of the blood became organized, and served as a means of union ween the divided tissues. As an analysis of the most recent opinions, ching this division of our subject, will be found in our review of the ours of Hunter, Rasori, and Macartney, we shall satisfy ourselves with racting, as newest, the views of M. Vidal on this subject, together 1 his comments on the opinions of others, before proceeding to the ussion of punctured wounds.

The hypothesis of the elongation of vessels by successive circles, applied in ance of one another, is too extravagantly unphysiological to merit a serious tation. It is, however, clearly essential that new vessels should be formed; r, then, is this production effected? This is the way in which I would ac-nt for it: The blood which is poured on the wound resolves itself into three ts: one dies, namely, that which is towards the cut surface, and subjected to action of the air; a second part is converted into coagulable lymph; and ly, the third part is composed of the globules of the blood which were st nearly approximated to the living flesh, and which have their vitality: y oscillate and move in the still uncoagulated lymph; they pass from one cut face to the other in such a way that there is already motion, transmission (or erchange) of blood between the two lips of the wound before the appearance vessels. But these globules, which were at first irregularly agitated in the stic lymph, then evidence a mutual attraction for each other, and unite into re or less direct lines; at the same time the lymph about them becomes more se and consistent, and the currents thus begin to be protected by walls; and this way the vessels are formed." (Vol. i., p. 23.)

So much for M. Vidal's series of postulates, which are as likely to be le as any other conjectural explanation of that which has not and can-t be very well *proved* by observation. By the way, this does not aid in unravelling the mystery of vascular inosculation—a point of diffi-lty of which M. Vidal seems not unconscious, for he adds a little fur-er on, "For that purpose (the anastomosis), we must not consider the timate capillary ramifications as formed by complete parietes; but it necessary to regard the canals as losing their form, their parietes 'fray-g out,' and presenting apertures which approximate to the structure of e vessels in vegetables." Mr. Liston's explanation of the same process simple and unpretending: "The vasa vasorum, ramifying on the di-ded ends of the minute vessels, secrete a substance which is transformed to a set of minute capillaries; and these also, assuming a secretive tion, produce an arterial or venous tube, similar to that nourished by e original vasa vasorum. By this process the lymph becomes well sup-ied with blood-vessels; those from the opposite surfaces meeting, and ely inosculating with each other." We observe that our French author incides with Langenbeck, and other eminent surgeons in Germany, in e propriety of the free employment of the suture in incised wounds: his ictures on the Academy of Surgery for condemning them are rather vere. We cannot help thinking that they might be employed more nerally with advantage amongst ourselves: the suture is an innocuous ans of procuring accurate approximation of the edges of incised unds, and is rarely attended with ill consequences, except from neg-t on the part of the surgeon.

The subject of punctured wounds—perhaps the most important, cer-

tainly often the most troublesome form with which surgeons have t deal—is not treated at much length, or very satisfactorily, by either ou German or French authors; and we turn with pleasure to the very prac tical pages of Mr. Liston, to aid us in the few remarks we have to offe upon this subject. A punctured wound necessarily implies the agenc of a penetrating instrument; and the mischief resulting from such injury where the weapon is uncontaminated by virus, is usually proportioned t the size and depth of the wound, or the relation they bear to one anothe The various wounds which come into this category may be classed unde three principal heads, namely, 1, punctures, or penetrating wounds o soft parts; 2, poisoned punctures; 3, penetrating wounds of the thora and abdomen. *A priori*, one would scarcely anticipate that there should b anything formidable in a simple penetrating wound, apart from the lesio of vessels or nerves; yet experience teaches the surgeon that he has fre quently more to dread from such form of injury than even from contuse or lacerated wounds. To analyze the cause of this, we must conside the positive nature of the mischief, and the means of reparation require In the first place, it appears essential that the instrument with which th injury is inflicted should be sufficiently large to contuse or break dow some of the more highly-organized structures in its progress; for we a well aware that very fine and clean instruments may be introduced int soft parts, perpendicularly to the surface, with perfect impunity, as re gards consequent mischief; as, for instance, the acupuncture needle Probably, in such cases, there is little else than a separation of structur during the passage of the instrument, and the parts become readapte immediately on its being withdrawn; where, however, the weapon ha been of sufficient size to inflict a wound, the character of which is a elongated or sinuous contusion, then mischief of greater or less intensit may be anticipated. Constitutional diathesis has doubtless much to d with characterizing the nature, extent, and consequences of the resultin inflammation; but, apart from such casual influence, there is that in th character of the wound which seems to excite mischief, by baffling na ture in the usual expedients she has recourse to for the cure of other le sions. Incised wounds are healed in part, or altogether, by the adhesiv process; whilst severe contusions or lacerations, which are followed b sloughing, are remedied by granulation: these two forms of the curativ process are, in truth, merely modifications of one another; in both the is a reproduction, the result of a secreted deposit, which becomes pro gressively organized; but in punctured wounds neither form seems accu rately adapted to procure a restoration to a healthy condition: there a contusion of soft parts which forbids the plastic process, and there is depth of wound which renders the granulating process tedious, and pro ductive of a tendency to considerable constitutional disturbance. It a generally advocated opinion, that the orifice of a penetrating woun should be closed if there be no extraneous matter therein. This plan doubtless, may be pursued with a prospect of advantage, as long as loca irritation or constitutional disturbance are absent or trifling; but w very much question the expediency of persevering in this treatment whe that is not the case; indeed, our own observation induces us to depre cate the practice as a general rule, where the soft parts all along th course of the wound cannot be kept in contact, so as to favour th

chance of adhesion: without this the confinement of the serous secretion is rendered an active source of irritation, and suppuration is encouraged and more readily established; where such is the case, it often becomes necessary to adopt the ordinary treatment for sinus, by laying open the wound through its whole extent—a practice which Chelius recommends where this difficulty exists. As this surgeon observes, where the instrument is flat, with cutting edges, the resulting wound heals much more readily (vol. i., pp. 1-9); in fact, the injury then belongs as much to the class of incised as penetrating wounds.

When a large artery is wounded, the case becomes much complicated; and if the injury be attended by hemorrhage to any extent, it admits of such treatment only as shall arrest the bleeding. Of this class of remedies, either pressure or the ligature is alone available; the former may be employed most advantageously to the trunk of the supplying vessel, especially where the puncture is at some depth from the surface: provided the ligature be deemed necessary, it will be found in most cases useless unless applied above and below the seat of injury. Mr. Liston's observations on this subject are simple and practical; he says, "The wounded vessel must be exposed, as already stated (by enlarging the external wound), but not detached more than is sufficient for the application of the ligature, and, at the same time, the ligatures ought to inclose nothing but the vessel; neither ought the ligatures to be placed at any considerable distance, but as close to the wounded point as possible, otherwise circulation in the included part may be restored. The ligature, round, narrow, and firm, ought to be tightly applied. .... If the vessel is merely punctured, it is necessary to apply the ligature by means of a blunt-pointed needle, and the parts are to be disturbed as little as possible."

Mr. Liston thinks the application of *slight* pressure to restrain hemorrhage from a wounded artery a plan worthy of consideration; and believes, with Dr. Davy (whose experiments most of our readers are familiar with), that the adoption of this treatment may be followed by beneficial results, without obliteration of the arterial canal, in cases where increased pressure would merely tend to augment the violence of the bleeding. Where the trunks of nerves have been injured by penetrating wounds, the resulting symptoms are occasionally of the most distressing nature; even requiring, in some cases, removal of a portion of the wounded nerve. If virus, as from the dead body, be introduced by a punctured wound into the system, it becomes the frequent cause of serious and sometimes, as is too well known, of fatal results. Perhaps nothing more aptly illustrates the influence of the general health, or natural diathesis of an individual upon local causes of excitement than the present class of affections. Some individuals there are who exhibit an inherent disposition to reject poisonous matter thus submitted to the agency of the absorbents; or, if absorbed, whose system receives with impunity that which would be venom to another. This is doubtless partly referrible to the temporary condition of the secretions generally; but much is alone explicable by appealing to the natural excitability of the nervous system. We are far from believing the question settled, as to whether such poisons act directly or indirectly in producing constitutional disturbance; and we regard as the best guide to the solution of the question the order of appear-

ance of the symptoms: those cases are most to be dreaded in which th constitutional symptoms precede or are coincident with the first appear ance of local mischief, and in such we believe the nervous centres to be—doubtless mysteriously but—directly affected, that is, without the inter vention of the absorbents. Our own troublesome susceptibility to these sources of annoyance, and the experience we have derived from lon intercourse with the dissecting-room, lead us unhesitatingly to recom mend what we practised with success in preventing the evil consequence of a poisoned *puncture*, namely, the simple plan of converting the punc ture, by the aid of a clean lancet, into an incised wound. This shoul be done as soon as possible after the wound is received, and to the ful depth to which the instrument has penetrated; the hand may then be soaked freely in warm water, and subsequently covered by adhesive plas ter; the occurrence of heat or throbbing at once indicate the propriety of poulticing: the water dressing, covered with oil-silk, is a very agree-able and cleanly application. Caustic is, we are satisfied, very rarely of any use, and often induces the very mischief it is intended to prevent. Where pain is extreme, the greatest relief is obtained by soaking the part in hot water. Opium is useful as a general remedy to allay irritability, but it should never be given until the bowels are well cleared. With re-gard to penetrating wounds of the thorax or abdomen, their frequent fatality is dependent either on the injury of some important organ or blood-vessel: the latter instances have more often an instantly fatal re-sult, although not invariably so. If the sufferer survive the immediate effects of such an accident, as wound of the heart, or one of the large vessels emanating from it, he is usually indebted to the influence of syn-cope. We believe the explanation of such cases adopted by Mr. Liston to be correct, namely, that a coagulum forms, during the fainting fit, in the wound, preventing thus the further extravasation of blood, until, under some violent physical exertion or moral excitement, the plug be-comes forced out, and the patient at once falls a victim. We have had occasion to examine some cases illustrative of this subject, in which in-dividuals have for some time survived penetrating wounds of the heart.

Wounds of a bruised or lacerated nature are rarely succeeded by much hemorrhage; indeed, as Mr. Liston justly observes, "in proportion to the severity of the bruise is the bleeding slight and the danger great." This phenomenon is readily accounted for by a consideration of the form of lesion sustained by the blood-vessels: nature thus first taught the sur-geon the value of torsion as a means of arresting hemorrhage. The chief characteristic of this class of wounds is the destruction—the annihilation of the vitality of the injured structures, which, in most instances, results where the contusion or laceration is severe; the process of cure is thus rendered tedious, in consequence of the necessary preliminary step of se-paration of the dead from the living parts. Gun-shot wounds belong to this class of injury, and probably the destruction of soft parts produced by projectile missiles is the most formidable that the surgeon can be called upon to treat. Of these cases our hospital wards present but rare ex-amples; although, of late years, we have to thank certain *machines* of the projectile class, 'ycleped railway carriages, for sending us (as the young student would say) some very *good* cases of as severe laceration and contusion, with or without fracture, as the most enthusiastic surgeon

could desire to see. These cases—say, of extensive laceration of soft parts generally, or of skin only—are interesting and instructive, when contrasted in their progress and results with severe and even compound fractures, where the surrounding structures have not sustained much injury. Many are the cases of the former class, in which the surgeon knows by experience that he dare not attempt to save the limb, whilst he confidently anticipates a favorable result to the latter. The principal causes to which this difference is referrible are twofold: the risk of tetanus in the early stage, or, if that be escaped, the immense draught upon the system during the reparative process. The simple loss of a large superficies of skin is, in some cases, sufficient to render the removal of a limb advisable, if not immediately essential, to the preservation of life. We must, however, give the railways the credit of rarely puzzling the surgeon; the work of mischief is generally so thoroughly done, as to leave no alternative as to the treatment. We observe that M. Vidal recommends, in cases of contusion, the employment of pressure evenly applied; which he thinks (appealing in proof to M. Velpeau's practice) "not only favours the absorption of the blood and arrests further extravasation, but likewise prevents and moderates inflammation." In some parts of the middle of France, he remarks that the universal treatment of contusions of the head is by the application of a compress, consisting simply of a piece of money.

The question of what should be done, when the extravasation of blood is considerable, and indisposed to yield to the simple treatment of leeching and local applications, is of considerable moment. The plan of breaking up the so-called sac, which contains the coagulum, has been recommended, under the impression (perhaps well-founded, in some instances) that absorption would then more readily be effected. We do not doubt that, in some instances, where blood has for a long time remained unabsorbed, and the extravasation has not been very diffused, that nature walls in the extraneous matter, as she would any other solid: in such case we are of opinion that the employment of the lancet, where it is practicable, is not only the surer but a safer method than that of "*écrasement*," as M. Vidal terms it; indeed, this author admits as much in his concluding observations. It is rare that severe inflammation follows an operation of this character.

Burns and Scalds. This is the next class of injuries of which M. Vidal treats. The diagnosis of these lesions is so simple, and their treatment so generally understood and agreed upon by the best surgical writers at home and on the continent, that we can expect to elicit but little of novelty or interest in comparing the practice recommended and adopted by the authors whose works we have now before us. The different titles of "burn" and "scald" merely have relation to the active cause of the injury; the condition and requisite treatment being essentially the same, whilst the intensity of the mischief is modified by accidental circumstances. Scalds are produced by the contact of heated liquids, and, in these cases, the modifying circumstances are referrible to the consistence of the fluid and its capacity for caloric, its adhesiveness, &c. In like manner, burns may result from the contact of heated solids at different temperatures; but by far the most frequent cause of these latter injuries is the contact of some inflammable material in a state

of combustion. Of this class are the frightful accidents, which are so common in our hospitals, of individuals, for the most part (indeed, almost exclusively) women and children, whose clothes have, by incautious exposure, become ignited. For most practical purposes, the recognition and classification of these lesions under two heads will be found sufficient, namely, that in which the connexion between the cuticle and true skin is alone destroyed, and that in which the vitality of organized tissues is at once annihilated. We say this division will suffice for practical purposes, because the two conditions above noticed are very essentially different, and require appropriate treatment; but under the latter head we may comprise the various modifications of injury, from that in which the cutis alone suffers to the more extended mischief in which all the softs parts beneath are implicated, and which all require, in their different stages, corresponding modifications of the same plan of treatment, namely, that adapted to the separation of slough and subsequent healing by granulation. Both Chelius and Liston are satisfied to ground their observations upon this simple nosology; but M. Vidal sets forth at more length the six degrees of burn adopted by Dupuytren, in his Clinique Chir., tom. i. We will briefly enumerate these, before passing to the few remarks we have to offer on treatment. The *first*, or mildest form (erythematous form of Rayer), is characterized by simple, diffused, bright redness of the skin, disappearing, for the moment, on pressure, and accompanied by sharp smarting pain: these symptoms subside in a few days, and desquamation ensues. In the *second* degree (vesicular or bullous of Rayer), the effect is produced by the operation of more intense heat, or its application has been longer continued: the appearance of vesicles may be immediate, or after the lapse of some hours; the pain is greater than in the last form, and is aggravated by the tension of raised cuticle, but exposure of the true skin is a source of very great suffering: the curative process consists in reproduction of the separated inorganic scarf-skin. The *third* and following degrees are comprehended by Rayer under one head (the gangrenous); but we now speak of them as distinct. The third form, then, presents a superficial and delicate eschar of gray, yellow, or brown specks or spots, soft, and insensible to slight pressure. This condition, say Dupuytren and Vidal, characterizes destruction, mortification, of the rete mucosum; thus assuming what is anything but generally admitted, that this tissue is something more than a mere inorganic product and secretion of the true skin: an opinion to which we ourselves are strongly biassed—but this in passing; the separation of the eschar must of course precede the cicatrizing process.* In the *fourth* degree the whole cutis, together with (sometimes) the delicate subcutaneous cellular tissue, loses its vitality; and the resulting eschar is deeper, drier, and harder, separating after an interval of two or three weeks, and being replaced by granulation. In the *fifth* degree all the soft parts are implicated—cellular membrane, fascia, muscles, vessels, nerves, even down to the bone; the eschars black and brittle; or if the result of the application of boiling liquid, the condition of the part differs in presenting a soft, gray, insensible, and pulpy mass. We naturally pause here, and ask what can there possibly be worse than this to constitute the *sixth*

* In this form we are disposed to recognize the superficial or partial destruction of *cutis vera*, which may occur without involving the whole thickness of this texture.

degree; we have it presented to us in the form of an illustration quoted by Vidal from Dupuytren: "A young man, on crossing a foundry, stepped into a conduit for boiling metal; he was unable to extricate himself from this fiery stream, until the unhappy member ceased to possess a foot or lower part of the leg; he had scarcely been conscious of pain, and was not at first aware of the horrible mutilation of which he was the subject." Thus, then, this last degree consists in a complete carbonization of the injured member. In the prognosis of burns, the extent of surface and depth of texture implicated are important points to attend to, though scarcely more so than the locality of the injury: both circumstances deserve equal consideration in determining the treatment to be adopted. The shock that the system receives from sudden suspension of the important functions performed by the skin, resulting from its disorganization over a large surface, is of itself sufficient to place life in imminent peril; and this danger seems to be enhanced where the lesion is situated on the trunk, more especially, we should say, over the abdomen. We are not in these cases disposed to ascribe so much to the simple (albeit excessive) suffering of the patient; pain alone, we take it, rarely kills. Such, however, was not the opinion of Dupuytren; and M. Vidal coincides with him in attributing the fatality of many cases in an early stage to excessive pain; yet we find the former excellent surgeon remarking most truly that *partial* destruction of the cutis is attended with *more* suffering than where the whole depth of texture is involved: a circumstance to which he directs attention, as of importance in forming a diagnosis. Unquestionably the great cause for dread, where an extensive burn is situated over a large cavity, is the occurrence of secondary, or, if we may be allowed the term, metastatic inflammation: the derangement of equilibrium which the circulation sustains, together with the extra call on the internal membranes for a vicarious performance of function, may probably be recognized as reasons sufficiently substantial to account for this dangerous phenomenon. As might be anticipated, if this explanation be received, the mucous membranes are those which are generally found to have suffered in cases which have terminated fatally; and of these, the lining membrane of the bowels is most frequently affected, though inflammation of the bronchial membrane is not uncommon. Vidal also enumerates "purulent and sanguineous effusion into the articulations of injured members, together with congestion of the cerebral vessels, and manifest traces of inflammation of the serous membranes." (Tom. i., p. 86.) This surgeon speaks of three stages through which the sufferer of a severe and deep burn has to pass; each period presenting conditions under which he may sink exhausted. The first we have already alluded to, with an expression of doubt as to the validity of the cause in producing a fatal result, namely, excessive pain: of course we do not accept, as included under this head, the derangement of the nervous centres which necessarily succeeds, in a more or less marked manner, the disorganization of a surface so highly endowed and sensitive as the skin. The second period is that in which "inflammation is carried to such an extent as to produce an irregular and too powerful reaction." The third stage, or that of suppuration during the granulating process, proves fatal by the absolute exhaustion which accompanies a very profuse discharge. The treatment recommended by our different

authorities does not differ in any material points. After directing attention to the more usual topical means of allaying suffering, M. Vidal remarks that bleeding and other antiphlogistic remedies of an accessory nature are sometimes the best "quieters;" adding, that "it is particularly necessary these should be employed to prevent and combat too severe inflammation." We are glad, however, to read the saving caution of the next sentence, namely, that the anticipated drain upon the system during the subsequent stage of suppuration should never be lost sight of. The fact is, that bleeding is rarely required where proper caution is used, during the early stage of depression, in the exhibition of stimulants to rouse the sinking powers of the system from the shock it has received; it is never required during the state of collapse, and therefore ought not to be spoken of as a preventive means; and lastly, as M. Vidal himself observes, where there are many or large eschars, particular care should be observed to avoid every remedy which is likely to enervate the powers of the frame. We have said that great caution is requisite in the exhibition of cordials during the depression which is usually the immediate consequence of extensive and severe burns; the condition of the system at this period evidences in a marked manner the implication of the nervous centres in the shock; the feeble pulse and cold surface are accompanied by rigor, vomiting, &c., and the exhibition of some stimulant becomes absolutely essential to prevent fatal collapse. With regard to topical applications, in the milder forms of burn and scald, those which procure most relief to the sufferer are best adapted to meet the exigencies of the case. Thus, as protecting or vicarious coverings to the true-skin, lime-water and olive oil, as commonly used in our hospitals, or the latter with the yolk of egg, as recommended by Chelius, are excellent liquid applications; where the discharge is profuse, finely carded wool has been recommended by some; but we do not hesitate to coincide with Mr. Liston, in giving preference to the practice of dusting the surface with flour or starch: the want of cleanliness which the application of cotton to a discharging surface involves, is the great objection to its employment. The topical use of preparations from opium and lead require great caution; for where a large surface of true-skin is denuded, of course the direct effect of such powerful poisons on the nervous system (to say nothing of the chance of absorption) may be very great. We do not remember to have witnessed the occurrence of colic as a sequence of the application of lead ointments, but the narcotic effect of opium when thus employed is not so rare: we need scarcely add how essential it is to be doubly cautious in the use of this remedy, whether exhibited internally or applied to the surface, where the patient is a child. We remark that Mr. Liston deprecates the use of topical stimulants; but we can scarcely think that this excellent surgeon will not admit with us, that, for instance, turpentine is an excellent dressing where the separation of an eschar is tardy. We are satisfied that in burns, as in simple cases of gangrene from other causes, the sound parts require sometimes the fillip of a stimulant application to aid them in throwing off the dead matter; when that has been effected, and suppuration is fully established, the simpler the dressing the better: poultice, cetaceous ointment, water dressing, &c. each have their uses and advocates. M. Vidal notices that M. Velpeau employs pressure very generally and successfully; finding that it not only hastens

cicatrization, but that it moderates pain, and prevents simple and phlegmonous erysipelas: this treatment is also spoken of favorably by Dr. Thomson. It is scarcely necessary to point attention to the importance of position during the progress of the cicatrization of extensive burns in the neighbourhood of articulations; the resulting deformities, from want of proper precaution on the part of the surgeon, are not only unsightly in the extreme, but frequently entail cruel and irremediable inconvenience on the patient. The parts most liable to these contractions are the neck, elbow, fingers, &c.; but, even further, the fingers may grow together, or apertures, such as the vagina or nostrils, have become closed during the healing process. Where this unfortunate result occurs (and it will sometimes ensue in spite of the surgeon), Mr. Liston remarks that "the hardened cicatrix which is in fault may be either divided or excised, and, by paying attention to position in the after-treatment, the evil may be greatly lessened. In the case of contracted joints, it is not necessary to excise the whole or greater part of the callous web; simple division is sufficient, if carried deep enough through the altered and condensed cutaneous tissue." (p. 262.) We shall conclude this division of our notice by a short extract from Mr. Earle's excellent lectures on burns, in which this lamented surgeon indicates clearly the proper course to be pursued to prevent contraction of limbs, and the frequent cause of its failure. He observes,

"I am quite ready to admit that it is not in our power to arrest the law of nature, by which a cicatrized surface becomes smaller, and occupies less space, than the original wound; but it is in our power, in most cases, to direct and modify that which we cannot wholly prevent; and thus, at all events, to counteract its injurious effect. .... To take the upper extremity as an example, I will suppose a case, where the whole integuments on the inner and front part of the arm and forearm have been destroyed. If such extremity be kept carefully extended on a splint, *not only during the whole process of healing, but long subsequent to the perfect cicatrization*, you will find that the cicatrized surface will diminish in a circular direction, drawing the healthy integuments together from side to side, but that no contraction will take place in the long axis, in which alone it can impede the due motions of the limb. This permanent extension should be persevered in during the *day and night*, until all changes have ceased, and the cicatrix has contracted to its smallest dimensions. Care, however, should be taken, during this time, to give passive motion to the different joints, by which the proper secretion of synovia will be kept up, and the eventual free use of the limb will be ensured. This plan of maintaining the limb in a state of permanent extension should be commenced as soon as the wound has begun to granulate." (p. 43.)

Effects of extreme Cold. These are very analogous to those of heat—or, as M. Vidal expresses it, "the abstraction and accumulation of caloric produce effects which are very closely allied;" and this not only in the after-consequences, but in the immediate results of these apparently antagonist sources of mischief. We do not perceive any peculiarity or novelty in the chapters devoted by Chelius and Vidal to this subject, and shall therefore content ourselves with noticing the *degrees* into which the latter author divides the effects of cold upon the living frame.

The progress of the symptoms which characterize the influence and operation of this powerful agent are familiar to those whose duties require their attendance on the poor, though in England we have rarely the opportunity of witnessing the effects which are so common in the

more nothern parts of Europe; the mutilations of the face are very ra with us, the local results being, for the most part, confined to the extre mities, especially the feet; yet even here, in some of our colder winter the instances have not been few in which individuals have literally bee frozen to death. The order in which the symptoms appear, where th effects are localized, is, first, a gradual loss of sensation and motion, the total cessation of arterial action, and ultimately mortification ensues where the effect is general, "the surface," says Chelius, "becomes gra dually blanched and numbed; from the congestion of blood in the inte rior of the body, especially in the head and lungs, there ensue, in suc cession, anxiety, feebleness, disposition to sleep, and, where this i yielded to, and the cause continues in operation, death supervenes."

The *first degree* described by M. Vidal manifests itself in redness and swelling of the part affected; a deeper shade of violet or blue soon suc ceeds, resulting, doubtless, from obstructed circulation; but where the cold is more intense, the redness is usually preceded by pallor of the skin. The pain experienced is much aggravated by a too rapid change of temperature. The application of frozen mercury, or solid carbonic acid gas, is immediately followed by a sensation as if the part had been burnt; the pallor supervenes, and is in turn succeeded by redness Thus we may observe that this first degree is subdivisible into three forms or stages, according to the intensity of the cold applied, or the rapidity, rather, of the abstraction of caloric. When the cold has been very intense, indeed (and its operation endured sufficiently long), the *second degree* manifests itself in the formation, sooner or later, of blis ters. "They make their appearance in the space of a few hours, or, a most, two or three days after the action of the cold. If the parts the affected be subjected to the influence of even a moderately elevated tem perature, the vesicles rise almost instantly. The tumefaction in this de gree is much more considerable, and is often accompanied by a sensation of tension and extremely sharp pain." (Vidal, vol. i., p. 91.) The symp toms of this degree, it will be observed, are those which are most nearl allied to the effects of heat; their course and termination are likewis analogous.

In the *third degree* there is actual freezing, annihilation, or suspensio of all vital action. "The life of the affected part," says M. Vidal, "i either immediately destroyed by the direct operation of the cold, or, i the first place, manifests itself under the form of a disposition to morti fication, called by some pathologists gangrenous inflammation..... It is i this degree of cold that white or gray spots make their appearance, resem bling those which characterize the third degree of burn; or where the effe has been more decided, mortification of the skin is complete..... In a extreme case, the whole thickness of a limb may be involved. There i then, reason to fear an extension of this local death." (Vol. i., p. 92 With regard to the analogy that exists between the constitutional effec of heat and cold, M. Vidal points out the coma of the one and fatal di position to sleep in the other, as holding parallel relations; but add that other nervous phenomena are not identical: we do not know what he exactly alludes. The conditions of the circulation and respiratio are the same, and the analogy of the symptoms which accompany re action present a yet more striking analogy.

In speaking of the treatment of this class of injury, our French author, who had formerly deprecated the homœopathic handling of burns, says, "No one denies the value of refrigerants in the first stage of frost-bite." Now, it is countenancing a silly error to advocate cold applications, per se, as the fitting remedy for the injurious effects of this agent: cold is merely useful to moderate the reaction which is being induced by other means; thus snow should be employed when friction is resorted to: the use of cordials or stimulants requires the same caution, and, for the same reason, as in burns.

Mortification forms the subject of an important chapter with each of our authors; and its divisions, causes, and treatment are severally treated of. We here employ the term "mortification" in the sense which its name implies, and in which it is generally, we believe, accepted; namely, as expressing generically the simple condition of death of an organized structure, without reference to the cause or its modus operandi. There are, however, divisions of this genus, which it is necessary, in specifying, to define accurately, in order to avoid that confusion into which some authors have fallen who have used the terms alluded to synonymously. Of course, any definition which may be given is, to a certain extent, purely conventional; and whether etymologically right or wrong, if only clearly understood and agreed to, would answer every useful purpose; but this is, unfortunately, not the case. By many, as Thomson, for instance (and we observe Liston follows him), the term gangrene is employed to designate a condition of parts in which vitality is not extinct, though greatly impaired, the larger vessels still conveying blood to and from the limb, and the nervous trunks still possessing sensibility; in short, a condition from which the affected organ is recoverable. To this definition M. Vidal objects, and not without some reason; urging that if the generic acceptation of the word "mortification," which we have given, be adopted (and this is Dr. Thomson's case), then "gangrene" is made to have a paradoxical signification, namely, *incomplete death*. Lassus, and others, attached a distinctive meaning, purely anatomical, to the terms "sphacelus" and "gangrene;" the former being applied by them when they were treating of total destruction of an organ or part—say, a finger or toe—and the latter where the mortification affected an organized structure partially. Both of these definitions are open to objection, and we are therefore willing to turn with Mr. Liston to "a division of more importance, into humid and dry*, or traumatic and chronic gangrene; humid or traumatic being applied to mortification produced by external injury; dry or chronic to that resulting from a constitutional cause. "Humid or traumatic gangrene," adds this author, "frequently occurs without previous inflammation; the injury being so severe as at once to deprive the part of its vitality. Dry or chronic mortification is often unpreceded by inflammatory action, or, at least, it is slight, and of very short duration." (pp. 41-2.) The alliance which exists between ulceration and mortification is simply, and, we think, justly stated by Mr. Liston: in the former, "a part which, from any cause, is unfitted to remain a portion of the living body, is only prevented from dying by absorption just as it is about to lose its vitality; whilst in mortification the part perishes too soon, or in too great quantity, to admit of absorption."

* Heissen and Kalten of Chelius.

In fact, whatever be the ultimate cause of the mischief, the dead and dying parts of an organ or texture are reduced to the same condition, and subject to the same laws, as any other extraneous or unorganized body; it can no longer be allowed to cumber the part, and it becomes the duty of the surrounding living matter to cast it off, or take it up, as best it may, into the system. In the former case we recognize the process of separation by slough, whilst ordinary ulceration, resulting from inflammation, affords examples of the latter. These two conditions or processes are, however, not unfrequently combined, that is, during the process of ulceration it may be remarked that certain parts remain sometimes unabsorbed. No doubt those textures which possess the lowest organization when healthy, are the least obnoxious to the ulcerative absorption; and it is thus we see tendinous and fascial structures cast off in shreds from an ulcerating surface, instead of being absorbed. Frequently the activity of the inflammation and progressive loss of vitality are disproportioned to the capacity of the absorbents; in such cases we have a mingled process of ulceration and gangrene. This phenomenon is, however, not so much dependent upon absolutely as relatively low organization; thus, the ready absorption of more highly-endowed surrounding tissues will leave some parts isolated, and so removed from the influence of the absorbents: hence the foul surface of a rapidly-spreading ulcer, and the still fouler surface of the yet more rapid phagedenic destruction of soft parts. But to return from this digression.—The causes of mortification are classed by Vidal under two heads, in which both the nervous and circulating systems are involved: the first includes those which operate by retarding or arresting the course of the fluids—the nervous as well as vascular (for M. Vidal expressly reasons on the hypothesis, that there is a nervous circulating fluid of some sort); accidental sources of compression, as strangulation, heat, cold, violent contusion, &c. are all referrible to this head; whilst the second order of causes resides "in the primitive alteration of the circulating apparatus," such as the various pathological changes in the heart and great vessels, which tend to retard the flow of blood, or an "alteration in the central or peripheral parts of the nervous system." Now these are almost exclusively anatomical causes operating physically, and M. Vidal has seen the necessity of adding a third subsidiary order, which comprehends those agents which indirectly tend to a similar result, by their influence over or acting upon these two great systems; they are the poison of venomous reptiles, the ergot of rye and some others. This classification would be simplified by arranging the causes under the two heads of "constitutional and local;" thus comprising all agents which operate *generally* under the former term. In commenting upon the remote or general causes of spontaneous mortification, and noticing the rigid condition of the arteries in the lower extremities of old people, from deposition of calcareous matter between the middle and internal coats as a common source of that result, Mr. Liston remarks that "an attempt has been made to connect mortification with an inflamed state of the arterial coats; but," he adds, "this opinion is not confirmed by experience. Obstruction from coagulation of their contents, and inflammation of the venous trunks, sometimes precedes death of the extreme parts in old people, and seems to act as a direct cause." (p. 42.) The principal characteristics by which

mortification may be distinguished are treated of at length by M. Vidal, who classes them under the heads of "modifications in colour, volume, consistence, sensibility, heat, and mobility." He justly remarks, at the conclusion of these observations, that a careful investigation and analysis of the above signs or symptoms cannot fail to afford a *sure* diagnosis of the real state of a part supposed to be the subject of mortification. We hope, indeed, that errors, which he speaks of as having been committed by "talented observers" are not very common, namely, that "simple contusions have been mistaken for gangrene, and that deep and extensive mortification has been treated as a light and unimportant affection." Such grave errors would, indeed, be likely to lead to serious consequences in practice. We are, however, sometimes called to act with decision in cases where we have not the means of judging by local signs or external appearances, whether gangrene is likely to succeed or has already supervened; in such cases, the value of general or constitutional symptoms becomes evident; these are, according to Mr. Liston, "great anxiety; coldness and clamminess of the face and extremities; weak, irregular, and hurried circulation; quick, short breathing; a cadaverous expression of countenance; hiccough; diarrhœa; vomiting; and in hopeless cases (more especially of traumatic gangrene), delirium and coma. . . . . In some cases, the patients are restless and unmanageable; in others, low and dejected. The disease often proceeds with a fearful rapidity to a fatal termination, the patient becoming comatose from effusion within the cranium; but in other instances, in which the vigour of the constitution is greater, and the extent of the mischief less, the system bears up under the affection, and a separation is effected between the dead and the living parts." (p. 47.)

When the line of demarcation has become established, and the process of separation has commenced, it may be a question whether the interference of the surgeon is now justified; for we need scarcely observe that all our authors deprecate any intermeddling previous to this; and we think that the rules laid down by Mr. Liston are practically correct. He says, "After the line of separation has been formed, the surgeon may assist nature in her work, by dividing the exposed bones or ligaments by which the dead parts still adhere to the living; or he may perform amputation immediately below the line of demarcation. Amputation in the sound parts cannot be recommended; for vitality is impaired throughout the system, and more especially near and above the line of demarcation, where, though the structure seems entire, the incisions are made in parts really diseased, and which would almost certainly and speedily mortify. In fact, amputation above the line of separation, in whatever way performed, is seldom if ever productive of advantage in spontaneous gangrene." (Loc. cit.) It is, however, very frequently necessary to adopt a precisely opposite plan of treatment, as the only chance of saving life, where the gangrene is not spontaneous, but the consequence of severe injury. Two contrasted cases introduced by Mr. Liston illustrate and verify the truth of this remark. The first was that of an elderly female, of robust constitution, in whom mortification of the arm succeeded exposure to wet and cold (not frost), in her occupation of gathering water-cresses: this case was allowed to take its course without local interference, and terminated favorably. The second patient was a young girl (16),

of sanguine temperament and good constitution, who suffered a compound fracture of radius and ulna a little above the wrist-joint. On the fifth day, after much suffering in the interval, mortification was found to have taken place in the limb, which was very swollen, the fingers already of a black colour, the forearm livid, and there were vesications near the elbow with fetid discharge: amputation was performed at the shoulder-joint, and the patient recovered without the occurrence of an unfavorable symptom. It should be remarked, that in gangrene from frost-bite it is not unfrequently necessary to amputate by cutting through the living tissues, for the purpose of securing a covering to the stump, where it may be of great importance, as in the foot. These cases are not obnoxious to the objection regarding the use of the knife, which applies to spontaneous gangrene; they are rather to be placed in the same category with cases of traumatic mortification. With respect to the general treatment of the various forms of mortification, the practitioner must be guided by direct or collateral indications which the individual case under his care may present. There are, however, certain general rules which should not be lost sight of; and these relate principally to the cause of death in a part, the diathesis and existing power of the patient, and the period of the affection. In acute gangrene, for instance, and in robust constitutions, as Mr. Liston observes, "when the affection arises from over-action, abstraction of blood is had recourse to with marked advantage. In some cases it may be employed, but with due caution, even after sphacelus to a slight extent had occurred; but," as this surgeon justly adds, "however strongly purging and bleeding may be indicated, it is always to be remembered that the time is not far distant when they will be totally inadmissible." That spontaneous hemorrhage exemplifies the beneficial effect of loss of blood in sloughing phagedena, is a fact which is well known and recognized. The supporting and strengthening system must be adopted and persevered in during the separating process—and this both medicinally and dietetically. The specific influence of opium (as recommended by Mr. Pott), and of bark in powder, as still employed by some surgeons, is very questionable: doubtless the former is often of great use in allaying irritation and procuring rest; but with regard to the latter, we agree with Mr. Liston in giving the preference to the more convenient and agreeable, as well as useful form of sulphate of quina, where a tonic is considered desirable.*

That form of mortification termed "hospital gangrene" is fortunately very rare—indeed, scarcely ever witnessed now amongst us; a circumstance attributable to the greater extent and improved ventilation, as well as cleanliness of our hospitals. In the congenial atmosphere of a crowded ward, where filth was rife and fresh air scarce, and especially in close and damp weather, this frightful disease has been so prevalent, that no wound, whether natural, or the result of accident, or the employment of the knife, has escaped its contagious influence. Delpech (who had ample opportunity of witnessing this peculiar form of gangrene amongst the soldiers at the Montpellier Hospital of St. Eloi), defined the disease as "a particular disorganization of soft parts, by the effect of

* An interesting case of spontaneous mortification is reported by Mr. Solly in the Fourth volume (Second Series) of the Med. Chir. Trans., p. 263. The subject was a young child, and the loss involved a great part of three out of the four extremities.

which they disappear without leaving any trace of their primitive tissue, which becomes converted into a putrid and homogeneous viscous mass (gluten)." Mr. Liston's description is, perhaps, more to the purpose: "The wound becomes painful and swollen, and loses its healthy florid aspect; the granulations are flabby, and appear as if distended with air; vesicles form, containing serum or a bloody fluid; the pain is stinging; the secretions are suspended; and the wound is either altogether dry, or covered with slimy, tenacious, and peculiarly offensive matter." (p. 233.) This is succeeded by erysipelas of the surrounding integuments: in fact, many circumstances connected with their coincident appearances and causes, indicate that these two diseases are nearly allied; but Mr. Liston points out an interesting distinction between them: "both," he says, "are accompanied with great constitutional disturbance; but in erysipelas this generally precedes, whilst in hospital gangrene it follows the appearance of the malady." The hemorrhage from the large vessels exposed is sometimes very extensive. A modification of this affection, and of an uncontagious character, we not unfrequently see under peculiar circumstances and in degenerate conditions of body; we allude to phagedenic sloughing and ulceration, under which extensive surfaces are at times destroyed, without any form of treatment, whether general or local, having the slightest effect in checking it. The ravages of this destructive disease are occasionally frightful when it attacks the genitals or their vicinity; we have repeatedly seen the male organ partially or totally destroyed, as likewise the labia in the female, the sloughing process subsequently extending widely and deeply on the nates or groin, even to the exposure, for a considerable extent, of the femoral vessels; but even these cases are now more rare than formerly, whilst the disreputably famous Swan-alley and similar resorts of low prostitutes still existed; this affection of the organ of generation seems to be independent of any specific venereal taint, but frequently appears to be connected with extensive and promiscuous sexual intercourse, especially after disease has been established. The direct application of strong nitric acid to the surface of such sores we have found the most, indeed frequently the only, efficacious remedy in arresting the destructive process; and this we have found it necessary to apply two or three times before the desired effect of cleansing the surface and inducing a new action was procured. Constitutional treatment in these affections is, of course, of the highest importance, though we are informed by Dupuytren and other French surgeons who have had extensive opportunities of witnessing and treating the *true* hospital gangrene, that they placed far more reliance on topical applications, and have repeatedly proved the inefficacy of attempting to arrest the disease by constitutional means alone. We may add, in conclusion, that some observers have positively asserted that specific sores are never attacked by this formidable affection: but Drs. Hennen, Thomson, and others cite instances to disprove the universality of this peculiarity, though they admit that cancerous, venereal, and similar affections not unfrequently escape: this fact would seem to favour the idea of hospital gangrene itself being a disease of a specific or poisonous nature.

Erysipelas. Of all the diseases which come under the province of the surgeon there is perhaps no one which deserves his more serious attention

than *erysipelas*: whether viewed as a local or constitutional affection, that is, in relation to its topical or general effects; whether appearing in an idiopathic form, or as a sequence of injury, there are few diseases which require more accuracy of discrimination in the employment of general remedial means, or promptitude and decision in the adoption of the necessary local treatment for arresting the destructive ravages which almost inevitably follow the unchecked progress of the affection in its severer forms. We shall therefore devote the space which remains to us in our present article, to discussing and contrasting the relative merits of the various plans of treatment, local as well as general, which are advocated and adopted by our authors, and also by others, in this disease: but first of all we must define what we mean by erysipelas. The difficulty of precisely defining this disease appears to arise principally from the question whether all idiopathic forms of simple diffused inflammation of the skin should be included under the generic title of erysipelas, or whether this term should not be limited to spreading inflammation of the skin and subcutaneous cellular membrane. The definition which Mr. Cooper gives is that which is ordinarily accepted, namely, that "erysipelas is a cutaneous inflammation attended with redness, which disappears and leaves a white spot for a short time after being touched with the end of the finger; and the affection, which is irregularly circumscribed by a defined line, is characterized by a remarkable propensity to spread."*

It is thus that the modification termed erythema, or as we may call it, simple idiopathic cutitis, is included in the above definition (which it is clear will apply equally to both affections), an arrangement that is objectionable on account of the difference of circumstances under which each form arises, the accompanying constitutional derangement in true erysipelas, and the several tendencies of the two complaints. With this preliminary remark we shall be content to follow our authors in the divisions which they adopt. Vidal alone gives a separate short section to the consideration of erythema. The various species of the genus erysipelas are differently given by different authors; indeed the classifications are nearly as numerous as writers: thus Pearson has three forms, phlegmonous, œdematous, and gangrenous; Lawrence describes erythema, simple, phlegmonous, and œdematous erysipelas; Rust and Chelius again have but two divisions, namely, true and false; whilst Rayer has no less than seven forms. It is of very little import which of these classifications is followed, but if we were disposed to give a preference it would be to one based on different principles to any of those above enumerated, and having reference to the exciting cause rather than to the actual condition of the affected structures; as, for instance, the idiopathic or primitive, the symptomatic or secondary, and the accidental; this distinction has been recognized by some authors. The *causes* of erysipelas may be referred either to the predisposition and existing condition of the constitution, or to the infliction of some local injury; the latter may however be said to involve the former, as we may recognize in the occurrence of this form of spreading inflammation after a wound, an evidence that there existed a predisposition to the disease,

* Dr. Willan includes erysipelas under the head of "Bullæ," on account of the appearance of vesications in many instances: it is more correctly classed with the Exanthemata, by Rayer.

resulting generally from irregular living, or a coincident morbid condition of system which favoured its development. The immediate exciting cause of traumatic erysipelas may, however, be of a simpler nature, such as the employment of improper dressings to wounds, or mechanical irritation, but the abuse of good living is the most rife of remote or predisposing causes: in no class of persons is this disease so much to be dreaded as amongst our brewers' labourers in town: it almost invariably takes on its severest form in these gross subjects, destroying limbs and frequently life. The influence of certain moral affections is also enumerated amongst the exciting causes of the disease, and M. Vidal cites an amusing example of a lady whose choleric days were always marked by erysipelas of the nose. Amongst the more rare forms of this affection, we may mention the erratic and metastatic, the latter not unfrequently proving fatal by attacking the brain or its membranes. Derangement of the uterine functions, either in the form of dysmenorrhœa or amenorrhœa, is sometimes connected with erysipelas, but probably the most uncommon type is that of *general* inflammation of the whole body. Rayer quotes a case from Renauldin, where "the whole skin of the trunk and of the limbs, slightly swollen, presented a very intense erysipelatous blush; the face was the part least affected; the patient, who felt as if consumed by fire, was soon restored by the use of aperients, and tepid baths frequently repeated."* Mr. Cooper also mentions a case of which he was informed "where the attack was both periodical and universal, effecting a lady several times, at intervals of two years."

The most usual termination of erysipelas is by resolution and subsequent desquamation, sometimes in suppuration, and more rarely in gangrene; when either of the two latter supervenes it is an evidence of functional derangement of important organs, or of a debilitated and vitiated constitution. Circumscribed collections of matter are comparatively rare, but there is occasionally a partial and trivial suppuration in the subcutaneous cells after simple erysipelas, which must not be confounded with the more extensive and diffused formation of pus succeeding the severer form of phlegmonous inflammation of the cellular membrane: this latter has been named by Mr. Liston, "diffuse cellular infiltration," the sloughing which often succeeds being ascribed by him to "the infiltration of acrid sanious matter into the subcutaneous cellular tissue round a wound or sore;" the destruction of surface in these cases is generally very rapid, and rarely to be arrested even by prompt treatment. On the disputed question of the contagious nature of erysipelas we have little to say; indeed we believe the contagionist party is but small in the present day. In his very interesting and practical paper on erysipelas, in the Med. Chir. Trans. vol. xiv., Mr. Lawrence expressed himself in favour of its contagious character; an opinion formerly advocated by Drs. Wells and Stevenson, and more recently and strenuously by Dr. Williams, of St. Thomas's Hospital; whilst Rayer and others very positively deny it. We are not disposed to attach much importance to the experiment mentioned by Dr. Williams in his work on Cutaneous diseases, namely, "that if a person be inoculated with the fluid contained in the phlyctenæ of a genuine erysipelas, a red painful diffusive swelling is produced." Certain it is that the disease will attack many

* Willis's Rayer, p. 128.

patients at the same time, or in succession, spreading from ward to ward in an hospital; and whatever theory he may adopt, the practical surgeon knows too well the danger of operating under these circumstances, where his patient would be exposed to the influence of this justly-dreaded disease; but we believe that in these instances, it is rather to be regarded as epidemic than contagious; and its restriction to some particular locality may be explained by casualties which at the time might elude detection.

The *treatment* of erysipelas has been very various: we can only notice some of the more prominent plans. In the milder forms of erysipelas, there is room for little dispute as to the proper course of treatment to be pursued; indeed, in many cases, nature is best able to manage for herself without interference, or with simple attention to diet, and the state of the bowels. Where there is much disorder of the digestive organs, and of the liver in particular, both Chelius and Liston recommend the early exhibition of emetics; "they are productive of but little good," the latter observes, "in the more advanced stage, and their place is advantageously supplied by nauseating doses of antimony, combined or not with purgatives." This surgeon further adds that he has found great benefit from the combination of gray powder and Dover's powder, when the tongue is dry and covered with a brown crust; and that saline purges are sometimes desirable. Rayer advocates the employment of mild diluents in the slighter cases, and adds that lotions with tepid or cold water, or with any mucilaginous decoction, will in general suffice to arrest the complaint. Where the inflammation is quite confined to the surface, Mr. Liston thinks the employment of lunar caustic admissible, and even desirable, as diminishing the local uneasiness, and sometimes arresting the extension of the disease; free puncturing with a fine lancet, not reaching beyond the vascular layer, is also practised by him in these cases, as a means of relieving the over-distended vessels, and giving vent to the serous effusion, if there be any: this plan is peculiarly appropriate where larger wounds would leave unseemly scars, as in erysipelas of the face and head; the relief afforded to the patient is, moreover, very considerable where these parts are affected.

Before passing on to the very important consideration of the general and local treatment of the severe forms of erysipelas, including phlegmon, it may be well to clear the way a little by pointing attention to the different theories and explanations of phenomena which have induced practitioners to arrive at such very opposite conclusions in their treatment of this disease. The adoption of any particular plan of treatment, and its indiscriminate application in all cases of even the most uniform complaint, cannot be too much deprecated; the systematic aiming at specifics as an exclusive object in medical practice cannot but mislead enthusiasts into the bye-paths of quackery, and retard the advancement of medical science; there are other, and we conceive higher objects of generalization in the treatment of disease than the discovery of nostrums; and these will be found in the establishment of leading principles to which *symptoms* may be referred, and on which they may be treated: not that we mean to deny that we must all contend, to a certain extent, with disease on empirical principles; but we mean to assert that the specific treatment of symptoms classified in the

way to which we have alluded, is not only more philosophical and successful, but is much more likely to improve the art, as well as elevate the science of medicine. Our present subject offers an apt illustration of the inutility as well as danger of attempting to lay down certain fixed principles for the cure of a formidable and frequently fatal disease; what is the practical result of these theoretical systems, and what are the rules determined by those who advocate them? One party says that erysipelas is an acutely inflammatory complaint, and must accordingly be treated by extensive depletion; whilst another steps forward and claims the title of this complaint to be classed with morbid poisons, and therefore that precisely the opposite treatment should be adopted; that to bleed is to destroy your patient. We say to both these classes of practitioners alike: attend to symptoms; attend to the predisposing and exciting causes of disease; attend to the powers and general diathesis of your patient, to the locality and other circumstances of the attack, and then decide on your plan of treatment; instead of coming to the bedside of your patient, disclaiming all the while the influence of preconceived hypothesis, with the determination of treating him on the antiphlogistic or stimulant plan; then, and not till then, may you expect that success which the careful and patient observation and treatment of symptoms alone can justify you in anticipating. Amongst the strenuous advocates of the antiphlogistic treatment we may class Dupuytren and Lawrence, their practice being founded on the opinion that erysipelas is a simple inflammatory affection, and is therefore to be handled like other inflammations, the symptoms, causes, and effects of which are analogous or identical; thus, venesection, local bloodletting, purging, and low diet are the first measures to which saline and diaphoretic medicines may be afterwards added: "the earlier," says Mr. Lawrence, "these measures are used the better; vigorous treatment in the beginning seems most calculated to shorten the attack, and prevent the disease from spreading beyond its original seat." We now turn for a moment to the supporters of the tonic and stimulant plan of treatment, before we pass on to an analysis of the opinions contained in the works which form the more immediate subject of this review; Dr. Williams of St. Thomas's Hospital will afford us the means of exemplifying the theory (not quite peculiar to him) on which this practice is founded. The fairest way of proceeding will be to allow the author to speak for himself:*

"Inflammation may be divided into two great classes, or into those which depend on mechanical or chemical causes, and into those which depend on the agency of morbid poisons. Now a long experience, as well as many experiments, has shown that these different classes of inflammation, require the most opposite modes of treatment. The first class which embraces the phlegmasiæ, very constantly requires that the powers of the constitution be brought down to an equilibrium with the state of the part; so that large bleedings are sometimes necessary to induce the inflammation to terminate and resolve, or the wound to heal. The second class embraces a great variety of diseases, as paludal, typhus, and exanthematous fevers, &c., and also erysipelas. In these cases the constitution has very generally received a considerable shock or depression before the tissue, the immediate seat of inflammation, is affected; and consequently the state of the constitution is often at once reduced either to

* The succeeding extract is taken from the St. Thomas's Hospital Report, vol. i. p. 323. Clinical Observations on Idiopathic Erysipelas, by Dr. Williams.

t be meliorated by the depletion." Again, "The exhibition of the ex-
ıct of aconite in this and other inflammatory affections is often followed
great abatement of vascular excitement, so that the necessity for the
straction of blood is done away with. It may be given in doses of half
grain in substance, or dissolved in pure water, repeated every third or
ırth hour." M. Vidal places the greatest reliance on ipecacuanha, by
ıich, he says, he has arrested the disease where even the cerebral symp-
ns had become very decided, and that, too, without having recourse
the lancet; but he admits that he has seen the most marked success
low the abstraction of blood frequently repeated, as practised by
. Bouillard, and advocated also by M. Rayer. This latter physician
kes the buffy state of the blood as a guide for repetition of the bleed-
g, and remarks, that when "erysipelas is complicated with phlebitis,
e practice ought to be even more energetic." M. Rayer has also found
at bathing the parts with cold mucilaginous lotions has been grateful
ıd beneficial, and that "local bleeding, practised at a certain distance
om the limits of the inflammation, secures the good effects of the gene-
l depletion." (p. 131.) On the other hand Mr. Liston remarks, "In
ry many cases, the strength is from the first to be supported by all pos-
ble means, by nourishing diet, by the exhibition of wine, quinine, and
her tonics—more particularly in old people, in constitutions debili-
ted by disease, in unhealthy situations, and when the fever is of the
phoid kind." The practice of Chelius is likewise founded on the just
rinciples of treating symptoms; freely employing leeches, and even
eneral bloodletting, when the case is urgent: as a local application in
is "true" erysipelas, he prefers, in opposition to Rayer, dry warmth.
leeding by leeches Mr. Liston objects to, on account of the leech-bites
roving a source of irritation, and being liable to suppurate—a difficulty
a great measure obviated by M. Rayer's plan of placing them beyond
e limit of the inflamed surface. In quitting the subject of the general
reatment, we must notice a remark of an experienced and practical sur-
eon (Mr. Pearson,) who considers that cases very rarely occur in large
owns in which the practice of bleeding is admissible, and that the repe-
ition of the abstraction of blood should never be had recourse to except
nder urgent circumstances.* We perceive that Mr. Cooper questions
e propriety of making this distinction; it is true that we ought sooner
be guided by "the violence and extent of the inflammation, the state
f the pulse and other symptoms, never forgetting the patient's age,
rength, and other important considerations," yet we are much disposed
agree with Mr. Pearson, ascribing the different susceptibility and
owers of the poor in town and country rather to the influence of their
bits and relative use (or rather abuse) of intoxicating liquors, than to
cality. We still believe, nay, we are satisfied, that very active deple-
on has alone sufficed to combat the destructive tendency of this disease
en in the most crowded parts of our metropolis.

We have already noticed the employment of warm and cold applica-
ons to a part affected with erysipelas, and we cannot but coincide with
r. Liston in expressing our opinion that the use of spirituous and eva-
rating lotions, though they may, in many cases, afford temporary re-

* Principles of Surgery, p. 211.

lief, "is fraught with the utmost danger; for their direct tendency is to produce metastasis, and if that be to an internal organ of importance, the result is too generally fatal. In case," he adds, "of the translation of erysipelas to any important part, blisters may be applied to the surface which it has left, or to any other in the neighbourhood, with the view of recalling the disease to its original and less dangerous situation." (p. 65.) The topical employment of nitrate of silver is a remedy which has been strongly recommended by many surgeons, especially Mr. Higginbottom: by some the application is confined to the healthy skin immediately around the affected part, by others the whole surface is pencilled over; by the former plan the spread of the inflammation is sometimes arrested, and in the latter, as we have already remarked, it is admissible so long as the inflammation is simply erythematous; but Mr. Liston again warns us against the use of this topical application for the same reason that he eschews cold lotions, namely, the risk of metastasis; and, moreover, he justly remarks, that the inflammation may be extending deeply without our possessing the same facility of detecting its progress when the surface is altered by the action of the caustic. M. Vidal seems adverse to the employment of these local remedies; for he says he has employed, or seen employed, actual cautery, caustic, and pure creosote, without witnessing the beneficial effects described by others. M. Velpeau has proposed to treat erysipelas by compression with bandages, but it does not appear to have been attended with much success in the hands of other practitioners; for we find Chelius, Vidal, Cooper, Lawrence, and others, speaking doubtfully or disparagingly of its effects. The last topical form of treatment we shall notice is that of relief with the lancet or knife; the former consisting in simply puncturing the affected part in numerous places with a fine lancet, the latter having for its object the liberation of distended tissues in the more advanced and acuter forms of the disease: these plans were severally originated and recommended by Sir R. Dobson and Mr. Copland Hutchinson. So much has been said upon the relative merits of the simple puncture and free incision, that we are loath to enter the lists again in deprecating either system exclusively, and in pointing out the propriety of attending to all the symptoms which may be present before any determination is arrived at regarding the proper course to be pursued. So long as the object is solely to relieve the tension of parts from the abundant serous effusion, or to abstract blood locally, puncturing will be found not only sufficient, but quite as efficacious as larger incised wounds; for the free communication between the cells of the cellular membrane allows of the ready escape of fluid from punctured apertures: but where suppuration and sloughing of the cellular tissue have ensued, as is not unfrequent in the severer forms of phlegmonous inflammation, no doubt can exist regarding the propriety of freely incising the part, although not quite to the extent of sending the patient, as Mr. Cooper remarks, "out of the world in a very sudden manner, sometimes from the shock of an enormous wound on the constitution in its disturbed state, sometimes from profuse hemorrhage." We say, then, that the actual condition of the affected parts, as ascertained from a history of the progress of the disease and the existing symptoms, must be the chief guide in determining the proper local treat-

ent; and here again we find Mr. Liston's remarks so very practical d apposite that we cannot forbear quoting them:

"It should be borne in mind that the surface of the skin only may be affected that and the subjacent tissue may be involved, gorged with serous, lymphatic, purulent infiltration—there may exist great tension of the parts, with a ughy state of the cellular tissue, established in addition to suppuration; and ain, there may be infiltration of the subfascial and intermuscular tissues, ding ultimately to exposure and exfoliation of bones or disease of articulans. From inattention to these circumstances, the treatment being often dicted to the name of the disease, great discrepancy of opinion, as to the most oper local management, has arisen."

Mr. Liston adopts free incisions where the tension is great, and there "a degree of bogginess" marking the disorganized state of the sues; the relief he ascribes to "abatement of the tension, the unding of the over-distended blood-vessels of the part, and the acce-ation of the suppurative process, which is often critical." The experi-ce of both Chelius and Vidal leads them to confirm this practice, and e latter does not hesitate to recommend the further division of fasciæ en necessary; a step which is sometimes necessary, as we have our-lves proved, when the suppuration is subfascial. Rayer approves of the actice of making incisions, deeming it advisable, in some instances, for e relief of tension, even where there is no suspicion of suppuration hav-g taken place.

We now conclude for the present, designing to avail ourselves of an rly opportunity of again meeting our authors and confronting them in e discussion of other divisions of their works, such as the "diseases d injuries of blood-vessels, bones, and joints," which will afford us nple material for a future article.

Of the relative merits of the books themselves we need add but little; r the passing opinion we have at different times expressed, together ith the quotations selected, may suffice to put our readers in possession f a fair idea of their comparative value. The work of M. Vidal certainly troduces us to an author of no ordinary merit; as a surgical treatise is perhaps to be regarded as the most complete of those before us, but r practical value to the student we cannot but award the preference to e German and English Manuals. On Mr. Liston's work we cannot ell bestow more commendation than it deserves; it bears the impress 'a practical mind which has long been habituated to accurate observa-on, and the formation of just and independent conclusions, unbiassed y prejudice or favorite theory, and unencumbered by hypothetical asoning. The soundness of practice inculcated, and the simple and rse style in which it is set forth, are such as must win the attention and struct the mind, whilst they tend to infuse self-confidence into the arner.

Art. III.

On the Nature and Treatment of Stomach and Urinary Diseases; being an Inquiry into the Connexion of Diabetes, Calculus, and other affections of the Kidney and Bladder with Indigestion. By William Prout, M.D. F.R.S. &c. *Third Edition.—London*, 1840. 8vo, pp. 594.

We felt very considerable anxiety to become acquainted with this new version of a treatise which has long held a prominent place on the shelves of medical libraries.

The work is rewritten, and in size it greatly exceeds its predecessors; but its materials are arranged on the principles connected with Dr. Prout's name. We shall not limit our notice to any actual novelty the present edition of the treatise may contain, but endeavour by giving as full a statement as we can of its leading doctrines and their applications, whether now first broached or already made public, to enable our readers to form a just estimate of its scope and merits.

The volume opens with an introduction, comprising an outline of the general physiology and pathology of assimilation, and of the secretion of the bile and urine. Having stated his classification of alimentary products into the aqueous, the saccharine, the albuminous, and the oleaginous, Dr. Prout proceeds to offer some general remarks on each of these. Saccharine bodies, he reminds us, are of two kinds, crystallizable, and uncrystallizable or organized; to the former belong sugar and vinegar, to the latter the different forms of the starchy or amylaceous principle, of lignin or the woody, and of gum or the mucilaginous principle. The only crystallizable product used to any extent as food is vinegar, and this is readily digested by healthy stomachs. Our author, however, referring to certain states of disease in which this organ appears to lose the power of assimilating this principle, expresses his belief that "on the whole it is doubtful if mankind have been the gainers, except in convenience, by employing it in a form which is the furthest possible removed from organization and life," a denunciation levelled at this principle more particularly from its effects in diabetes.

Of the characters of these primary alimentary principles, of which it is unnecessary to rehearse the species or familiar properties, none is more remarkable than the capability they possess of assuming an infinite variety of forms, without undergoing any change of their essential composition, as illustrated in the mutation of sugar into vinegar or acetic acid. But this is not the only change,—they have the faculty of readily passing into each other under the influence of a *converting agency* existing in the animal organization into which they are introduced. Nay, still more, these principles are all susceptible of transmutation according to ascertained laws into totally new compounds; for example, the saccharine is readily convertible into oxalic acid or into alcohol, itself a version, if we may so speak, of the oleaginous principle. But such modifications of the primary or *staminal* principles, as Dr. Prout names them, though endlessly diversified, bear, as he justly points out, but a very limited proportion to the original radicles supplying them. They are either the product of glandular secretion, or excrementitious, or extra-vascular; or, in still more general terms, they form no part of the

nimals in which they are developed. They are however often attached o these, as for instance in the form of a shell to the Mollusca.

Dr. Prout, having laid down the law that these proximate principles upon which animals subsist are, with the exception of the saccharine, identical in their essential characters with the components of the tissues in the superior animals, draws the inferences that, first, such animals are spared the labour of forming their proximate principles from their elements, (a task devolving on inferior animals and on plants;)* and secondly, that an appropriate diet should contain more or less of all these principles. That the latter position is correct, is alleged to be further established by two additional facts of different character, the composition of milk, and the impossibility of supporting animal life upon a single proximate principle. It is true that milk, the only material distinctly and exclusively produced by nature for food, and which may therefore be regarded as the type of what aliment should be, contains the four staminal compounds; but the other fact, which has been repeatedly ascertained by direct experiment, may be and has been explained in different modes from that adopted by our author.

Such then are the materials from which animal organisms derive their means of existence, and perpetuate the admirable machine constituting them. The processes by which they appropriate these materials are comprised under the general title of assimilative. Assimilation is therefore a compound action, or rather series of actions, divided by our author for the purposes of description into two classes: the first embracing all organic manifestations of activity tending to and terminating in the formation of blood, in other words, digestion and sanguification; the second, the formation of tissue and the redissolution of this with its subsequent removal from the system. In both of these species of operation water, that essential constituent (whether chemically combined or mechanically united with them) of all organized bodies, plays a very prominent part. In the first place, by the variation of the proportion in which it enters into the composition of bodies, these are altered materially in their character as regards their alimentary nature and power. Thus, when it is combined in small quantity with any body, this is usually of a firm or stable character; when in a high ratio, this is distinguished by the opposite properties. Now, as the mutual interchange of these conditions, of strength of principle into weakness, and *vice versa*, constitutes one of the most frequent and important species of chemical processes taking place in organized bodies, and is therefore the subject of constant allusion, it is well to devise terms to express it concisely and significantly; this Dr. Prout has done by applying the word *reduction* to the change of a strong into a weak principle, *completion* to the converse process.

The process of change undergone by the food in the stomach is of the first kind; Dr. Prout considers that the semiliquefaction of the ingesta probably consists essentially in this combination with water, in other words, their *reduction*. Secondly, as the stomach forms identical chyle from food of various descriptions, it follows that it possesses the faculty

* "Plants," it has been said, "seem interposed between the soil and animal life, as laboratories for combining the elements of inert matter into substances capable of being assimilated in the digestion of animals."

of changing into one another the different simple alimentary pri or, as our author designates it, a *converting* power. Both these pr it is clear, are purely chemical. But, as Dr. Prout conceives, t not the only powers of the stomach; he maintains that it pos *vitalizing* or *organizing* faculty, whereby it is enabled to fit th aliment for contact with living structure. The possession on the the stomach of this vitalizing power is not by any means demo in the pages before us; and why the vivifying influence should n the second stage of assimilation, the most probable notion as it us for various reasons, is not attempted to be shown. Of the r the peculiar actions by which the combination of alimentary su with water is effected, Dr. Prout laments our ignorance; but culiar juice developed in the stomach during digestion must of c the *reducing* agent. This juice is made the subject of examin our author, and the question of the source of chlorine or rathe chloric acid, forming one of its most essential ingredients, discuss conceives it quite unnecessary to suppose this generated *de nov* tains that the common salt existing in the blood must be its sou inclines, as of old, to the notion that the separation is effected by so fication of electricity. But as the acid of the preexisting muriat is supposed to be thus disposed of, it remains to be enquired comes of the base from which it has been separated. Dr. Prout is in assigning the alkali its *rôle:* "the soda remains behind or is into the mass of blood, and a portion of it, no doubt, is requisit serve the weak alkaline condition essential to the fluidity of th But the larger part of this soda is probably directed to the live elicited with the bile in the duodenum, when it is thus again into union with the acid which had been separated from the blo stomach."

Our author professes what may be called, in regard of admit trine, a heterodox opinion on the lactic or acetic acid foun stomach during the process of digestion. Instead of regarding t is generally considered, as an ingredient in the composition of th juice, second only in importance to the muriatic acid, he believ is rather to be looked on as the result of unnatural irritation, by disease, indigestible aliments, &c., than as a healthy product n to the digestive process. But for this extreme opinion no reasons are given, and its probability seems to be materially sl not altogether destroyed, by the experiments of Tiedemann and and still more distinctly so by the analysis of the gastric juice by Dr. Beaumont from the stomach of St. Martin. That th butyric, and carbonic acids are unhealthy products, when occ the stomach, we of course find no difficulty in admitting author.*

Dr. Prout next explains his views on the converting powe stomach; according to these views, under ordinary circumsta essential effects of their action are the conversion of the sacch albuminous and oleaginous, of albuminous into oleaginous, and c nous into albuminous principles. The first of these changes h as by far the most important, for saccharine aliment cannot in it

* Dr. Prout enters into no enquiry upon the nature, properties, or act digestive element *pepsin.*

tate form a constituent of any part of the living frame, yet so essential to its ingestion and conversion seem to the perpetuation of even the lowest forms of life, that they are performed even by vegetables. This power of appropriating the saccharine principle may be looked on as the lowest element in the process of renovation, and is probably the last that ceases to exist in an animal, often persisting long after the faculty of assimilating oil and albumen has been lost. Of the temporary and partial suspension of this power, occurring in and constituting one of the most important disturbed states in diabetes, we shall have occasion to speak at length hereafter. The mode in which the essential changes of alimentary principles are effected in the stomach are probably purely chemical; thus it is likely that the conversion of sugar into oil is brought about precisely by the series of changes spontaneously developed during its conversion out of the body into alcohol, which is nothing more than a weak oil. With respect to other changes, we are less capable of tracing their mechanism; the origin of the azote is not yet fully cleared up, though it would appear to be commonly derived from some external source. "The azote may, in some instances," says Dr. Prout, "be derived from the air, or *generated.* But my belief is, that, under ordinary circumstances, the azote is principally furnished by a highly azotised substance [organized urea?] secreted from the blood, either into the stomach or duodenum, or into both these localities, and that the portion of the blood thus deprived of its azote is separated from the general mass of blood by the liver, as one of the constituents of the bile; which secretion, as a whole, is remarkably deficient in azote." But it must not be supposed that the capabilities of the stomach are, under all conditions of existence, limited to the production of these changes: when animals are accidentally obliged to subsist upon a diet of wholly unusual character; when they are placed under what Dr. Prout calls "extraordinary" circumstances, "the changes out of the ordinary course are," to use his language, "altogether astonishing, and such as defy our utmost calculation. The assimilating organs appear even to decompose principles which are still considered as elementary, nay, to *form* azote or carbon." The vastness of this hypothesis is absolutely startling, for it ascribes to the animal tissues the power of producing something out of nothing—in fact, of *creating:* in the existing state of the natural sciences, it is almost needless to say that such doctrine cannot be healthfully entertained. Possibly the explanation of this is, that Dr. Prout does not consider the elementary character of these bodies established.

So much for the *converting* function; the *completing* and organizing processes to which the chyle is subjected in the lacteal system receive little novel illustration at the hands of Dr. Prout: we pass on, therefore, to his views on secondary assimilation. Under this head are comprised, according to his arrangement, first, the *formative* assimilation of the materials of the blood into the tissues and the products of the secretions; and secondly, the *destructive* assimilations (or secondary digestion) of the tissues, with their conversion into new principles designed for further use, or into mere excretions.

The general principle on which these processes are conducted is explained by Dr. Prout as follows: The elements composing the albumen of the blood, for instance, when this is destined to undergo a change of constitution, are subjected to such modified arrangement as ends either

in the production of a single new principle with different sensible properties, or the elements are so combined as to form two or more principles, each of which is the complementary albuminous principle of the other; in different words, added to that other will produce albumen. This latter mode of decomposition, which is infinitely more common than the other, is of two kinds. By the first, a substance is converted into an excrementitious principle, and into one available for ulterior purposes in the economy; for example, albumen is changed into gelatine and hydrated carbon, capable of becoming carbonic acid on exposure to air in the lungs. By the second, the change is into two principles, both designed for ulterior offices, or both excrementitious. The first belongs, according to our author, to healthy action; the second, to disease. As an instance of the latter, we are given the mutation of gelatine into some modification of the saccharine principle and urea.

These laws, however, only apply to the essential elements* of organisms; of those bearing upon mineral elements *incidentally* present therein, Dr. Prout has only to state the inefficiency of existing knowledge. He agrees, however, with Berzelius in thinking that such incidental matters usually exist in their elementary condition in organized products, and not as binary compounds; and that the latter form is only assumed as a consequence of the destruction of the organized principle with which they may be associated. Hence, Dr. Prout would draw the important inference in pathology, that the appearance of an incidental binary mineral compound involves the idea of mal-assimilation or destruction of some organized tissue; and further, that as certain incidental mineral elements are always peculiar to and therefore characteristic of certain tissues (as phosphorus of the nervous, &c.), the tissue thus morbidly engaged may be predicated from the chemical composition of the incidental binary compound.

It is unnecessary for us to analyze closely Dr. Prout's statements on the formation of the gelatinous, albuminous, and fibrinous tissues, or, as he terms them, the processes of gelatification, albumification, and fibrification. When we have stated the hypothesis, that "when albumen is converted into gelatine, carbon is eliminated, which carbon (partly perhaps in a hydrated, partly in an oxygenated form) remains associated with the venous blood till its arrival in the lungs; when, by combining with the oxygen of the atmosphere, it becomes fully oxygenated, and is converted into carbonic acid gas, and in this form makes its escape from the body," we have extracted all the novelty this section affords. For the arguments supporting the view here brought forward, the author refers his readers to his Bridgewater Treatise.

Dr. Prout is rather chary of statements respecting the character of the ulterior changes belonging to the class of destructive assimilation undergone by the three essential proximate principles. He is, however,—persuaded for reasons which, as he alleges, the practical nature of the book before us prevents him from entering upon,—of opinion that one mode in which the *gelatinous* tissues become effete is by their conversion into two classes of complementary principles, of which urea, or its equivalent, is

* The term *essential* element is applied by Dr. Prout to oxygen, hydrogen, carbon, and nitrogen; on the remaining elementary substances of animal organisms (amounting to some fifteen or sixteen) he confers the name of *incidental* element.

ne, and the saccharine principle, most commonly in the form of lactic cid, is the other. And, again, he believes that one of the crystallizable principles produced from effete *albumen* is lithic acid, most usually in he state of lithate of ammonia; the presumed complementary substances of which will be alluded to hereafter. On the ulterior changes of the oleaginous principle no theory is hazarded.

In the enquiry into the general pathology of the primary and secondary assimilating processes, to which the volume next introduces us, the aberrations of each of these from the healthy state are slightly sketched. An idea of the character of deduction drawn by Dr. Prout from his investigations on this branch of the subject may be had from considering the following train of hypothesis. When the appetite has been over-indulged, and more food swallowed than the wants of the system require, a deposit is commonly, as all the world knows, observed in the urine. Dr. Prout conceives that in this case the *reduction* and *conversion* of the surplus quantity of food takes place, but that the stomach fails in *vitalizing* it, and that this non-vitalized portion is permitted to pass through the kidney in the form of lithate of ammonia. But this mode of relief requires the kidneys to be healthy; now Dr. Prout supposes that the necessary state of health does not exist in certain individuals during youth, and that either from this cause, or from original weakness of the assimilating organs, the relief referred to is not obtained under the supposed circumstances of surfeit. In such subjects "the imperfectly assimilated chyle," continues our author, "either does not undergo the necessary changes in passing through the lacteal system, by which chyle is converted into blood, or is malconverted into the comparatively insoluble pseudo-albuminous matter of struma, which, in passing through the lungs, lays the foundation (perhaps at first mechanically) of tuberculous deposition and future accretion." This urinary theory of phthisis might, to a certain extent, be made a question of direct experience; at least, it would be necessary, *in limine*, to ascertain whether the urine of scrofulous subjects, after such indulgence as would lead to lithic deposit in healthy subjects, is free from abnormal deposition of that salt. But our author does not appear to have considered the point in this light, and devotes a note to some further speculation, supported by such lame arguments as appear in the following quotation: "Strumous, lithic acid, and gouty diseases are all results of mal-assimilation of the albuminous principle, either primary or secondary, and often gradually run into each other. Thus gout and struma are frequently, if not always associated; and the gouty chalk-stones of old age may be considered as little more than modifications of the scrofulous tubercle of youth, both being alike formed from mal-assimilation of the albuminous principle." The unproved allegation, that the offspring of gouty persons are more subject than others, *cæteris paribus*, to phthisis, and the fact that tophi are often accompanied with renal disease, are the only statements, wearing the semblance of argument, adduced in support of these curious hypotheses. The notion that scrofula, gout, and phthisis are mere modifications of each other has been sustained by an equally unsatisfactory style of argument by Sebastian of Groningen.*

A distinction between the danger of secondary diseases depending upon

* Brit. and For. Med. Rev., vol. VIII., p. 242.

primary or upon secondary mal-assimilation is ingeniously pointed out and illustrated by Dr. Prout. Those originating in the primary aberration are, "in general," of much less formidable character than those of the other class. Thus the appearance of lithate of ammonia in the urine (a phenomenon often pressed into the service of illustration) is one of the most common effects of slight error in diet; but that salt is deposited in some cases when no food has been taken into the stomach;—now the latter occurs in certain fevers, and other severe constitutional diseases.

A chapter on the general characteristics of the blood, the bile, and the urine next follows. These cannot be said to contain much not already made public, either by the author himself or by others; one or two points, however, require notice. Of these is the modification of the opinion formerly advocated by the author respecting the cause of the deposition of free lithic acid in the urine. Dr. Prout now believes the free acid, or precipitating agent, to be the lactic; in some instances, it is true, the mineral [sulphuric, muriatic] or other acids may be the remote cause of the precipitation; [how so if these acids are present only in saline combination and the phosphoric alone as a *super*-salt?] but this is only from their separating the lactic acid from the base with which it is combined, and so allowing it to be the immediate organ of precipitation. Dr. Prout, however, thinks that in the greater number of cases of lithic-acid gravel, lactic acid is secreted in excess, either separately, which is comparatively rare, or combined with urea, which seems to be the rule. Here he appears to reject by implication the opinion of MM. Cap and Henry, that urea always exists with urine in combination with the acid in question.

It will be perceived, as an inference from the train of argument just stated, that Dr. Prout holds unchanged his former notions respecting the combined condition of the lithic acid in the urine. In favour of these he reiterates the evidence long familiar to the profession, without even alluding to the important microscopical observations of Donné and Quevenne on the subject; and, judging from the incomplete view given of them, appears to have become acquainted with the doctrines of Duvernoy and Wetzler rather from a recent work on urinary complaints than from the writings of those persons themselves. Dr. Prout assumes to himself merit for "avoiding all controversial points," but he means the discussion of these; so far from avoiding such points (unfortunately we know not the branch of medicine where such system of conduct could be pursued,) he settles them invariably *ex cathedrâ*, without the least apparent misgivings as to the rectitude of his decisions. However grateful it may be to some readers to meet with a conviction so strong that it almost displays itself in dogmatism, we, for our parts, feel persuaded that it diminishes the real usefulness of the volume.

Dr. Prout, like M. Rayer, has never been able to satisfy himself that casein may, under any circumstances, become a constituent of the urine. In commenting upon the statement of Liebig, that xanthic oxide differs from the lithic acid only by containing one proportion less of oxygen, the author seems inclined to doubt its accuracy, because, in the first place, he knows of no apparatus or means of operating capable, where azote is concerned, of unequivocally deciding upon the presence of one proportion of hydrogen, or even of oxygen in a complicated body;

secondly, because the foreign chemists are usually in the habit of following the calculation of Berzelius, as respects the combining weight of carbon, and therefore of making it considerably higher than six, its real value, as is now recognized both at home and abroad.

It is affirmed in these pages that the existence of the prostatic secretion in the urine may be detected by certain particular appearances. We were the more anxious to learn what these particular appearances might be, because M. Rayer has recorded his inability to point out any essential characteristic of such impregnation; but Dr. Prout singularly enough makes no attempt to impart his knowledge to his readers, on the ground that "the appearance in question can hardly be so described as to be made intelligible to those who have not attended to urinary phenomena." But surely there are some persons at least who have made these a study; why is the information withheld from them?

If a general view be taken of the constituents of the bile and urine, one of the most striking points of distinction between them will be found to be the comparatively great number of oxidized and acidified principles existing in the latter. The character of the function of the kidneys, of which the acidity in question is evidently the result, is by Dr. Prout termed positive; that of the liver negative. The liver depurates the system of unassimilated and superfluous oleaginous matters, and of those portions of blood deprived of azote and of vitality; the kidneys, on the other hand, exercise the same action with respect to the albuminous principle and the mineral matters incidental to them. And it is further pointed out that the changes produced by the liver on the principles it separates, are in some measure of an organizing kind; that is, the principles separated retain some of their vitality for ulterior purposes; whereas the function of the kidneys is of a disorganizing kind, all materials passing through those glands being perfectly deprived of vitality. In certain states of disease, as we shall hereafter see, the character of these functions is in the instance of each organ changed.

We have now made the reader acquainted with the more important part of Dr. Prout's preliminary matter; and analyzed this with sufficient closeness to convey a general impression of the character of the declared laws and of the inferences upon which the doctrines broached in the body of the volume are based.

The main part of the treatise is divided into two books, devoted severally to the history of "functional," and of "mechanical" diseases. The division is, however, most imperfectly retained in the sequel, for organic lesions of the most profound character (such as those known under the title of "Bright's disease,") are considered in connexion with those of functional character. But we have no space for a discussion upon errors of classification. Under the head of functional diseases are described "diseases arising from the deranged operations and less obvious lesions of the assimilating and secreting organs." Now, as the assimilative processes bear on four alimentary principles, it follows that there are four classes of functional diseases, of which those dependent on derangement of aqueous assimilation are first examined. In commenting upon the distinctive characters of the *urina potûs* (or of assimilation, as he proposes to term it,) and the *urina sanguinis*, the author insists upon the necessity—and that a truth so obvious should be habi-

tually disregarded in practice is a sufficient proof of the imperfect use to which we put the means of diagnosis nature places within our reach,—of examining in cases of disease a specimen of each of these varieties. Of the derangements connected by Dr. Prout with morbid states of aqueous assimilation, being simply excess or deficiency of urine, he very correctly abstains from speaking at this stage of his progress, inasmuch as these conditions are simply symptomatic of, and do not in themselves constitute, disease.

The views developed in the next section on the pathology of saccharine assimilation were some time since made known in outline to the profession.* The labours of the intervening years have, in the estimation of the author, verified and extended the applications of the doctrines then broached. Dr. Prout's argument runs thus: as the blood in health contains no sugar, and as the latter principle forms an ordinary part of human food, it follows that by the operation of the primary assimilating functions it must be converted into some of the constituent principles of the blood. And if the function of *sugar-conversion* (if the phrase be admissible) be allowed to exist, the reality of aberrations from the healthy state by derangement or suspension of this function is implied. Experience proves what reason points to,—the *suspension* of saccharine conversion is unequivocally shown to exist by the presence of sugar in the blood of diabetic subjects; its *derangement* made manifest by the occasional existence of oxalic acid in the urine of persons who had not ingested any of that substance with their food. The process in respect of diabetes is this: First, it must be remembered that the reduction of all forms of the saccharine principle appears to be accompanied with the development of a low sugar, easily convertible in the healthy stomach into albuminous and oleaginous matters. This change takes place speedily; the presence of sugar in the healthy organ is therefore with difficulty detected. In the diabetic stomach, on the contrary, where the reducing function is morbidly active, while the converting is suspended or paralysed, sugar is discoverable in abundance. "The first step in the derangements producing the disease called diabetes does not consist, as some have supposed, in the development of sugar in the stomach, which is a natural process, but in the greater or less destruction of the converting, and consequently of the still more important organizing functions of the assimilating organs." Upon this statement of Dr. Prout we would, in the first place, observe that the phrase "as some have supposed" is inapplicable to the existing state of belief. Mr. Mac Gregor's experiments clearly showed that sugar is contained in the healthy stomach after an ordinary meal of animal and vegetable food, and therefore, as it has been correctly put by Dr. Willis, that "diabetes is actually disease only because the degree of a natural process is surpassed." There are two principal questions to be put with respect to Dr. Prout's theory: is it necessary, is it involved by the established nature of things? secondly, if not, is it adequate? We can find no difficulty in answering the first query in the negative; there is no known fact demonstrating the impossibility of, or even rendering improbable, the conversion, under the supposed circumstances, of gelatinous and albu-

* Gulstonian Lectures; Medical Gazette, vol. viii. 1831.

minous principles into the saccharine. How are we justified in affirming that a modification of the *class* of principle may not be effected in a morbid state of the digesting agent, or of the system to which it belongs, when an interchange of principles of the same class is in health so easily accomplished? Surely the possible assumption of such power of conversion on the part of the stomach can, with no colour of consistency, be denied by a physiologist who, as already mentioned, believes that that organ can, under "extraordinary circumstances," take upon itself the office of a creator and form elements. The doctrine, in this point of view, evidently presupposes the existing power of fathoming the operations of vital chemistry to be infinitely more vast than stubborn facts, encountered on every side, will allow us room for a moment to dream. But, secondly, is the theory adequate; will it explain all the phenomena of the disordered chemistry under consideration? We think not; and for this reason. Mr. Mac Gregor, after having produced vomiting and purging in two individuals, one healthy and the other diabetic, fed both for three successive days on beef and water; the product of vomiting at the end of that period, and three hours after the last meal, furnished evidence, by fermentation with yeast, of the presence of sugar in the case of the diabetic subject, none in that of the healthy. Now, the theory of non-conversion cannot stand here, for no saccharine or vegetable matter was swallowed; hence we have positive proof that the stomach acquires the power, under certain conditions, of evolving sugar from organic non-saccharine combinations. While, therefore, we fully admit the fact of the non-conversion of the sugar produced in the stomach, we cannot, with Dr. Prout, suppose *all* the unnatural sugar therein contained to be the result of non-conversion of the quantity of sugar which would in health be developed from the same materials: in other words, the fault is a double one; *active*, in the power the stomach acquires of producing sugar from materials which would not supply it in health; *passive*, in the loss of its normal share of converting faculty. Besides, a point of no mean importance, which appears to be generally forgotten, is, that the appearance of the sugar in the stomach, however induced, is but a symptom, and cannot constitute the essence of the disease. Whatever be the change in the tissues of the stomach leading to it, whether microscopical or otherwise (for that such exists cannot, in our minds, be made matter of doubt), Dr. Prout seems to think it of not the remotest consequence, for no allusion is made to the point in his chapter.

The general causes of saccharine mal-assimilation are next examined. The internal or predisposing are most generally innate or inherited, according to Dr. Prout. The external exciting (in every instance subservient to the predisposing) are cold, moisture, and malaria. Although Dr. Prout is unwilling to pronounce diabetes a malarious disease, he has no hesitation in expressing his belief that almost all forms of disease connected with the development by the secondary assimilating processes of oxalic, lactic, and other abnormal acids, are more frequently excited by malarious than other external influences. His attention was first directed to this point by seeing in quick succession several well-marked cases of oxalic-acid diathesis from a malarious district. The inference drawn from this circumstance was confirmed by the occurrence of the appearances characteristic of oxalic instead of lithic acid in persons af-

fected with indigestion or "cold" after the cholera; a disease which our author believes to have been of malarious origin from certain facts, for which we must refer to the original. Dr. Prout has sometimes imagined that the urine has not completely recovered (1838) its former condition since the occurrence of that epidemic.—A most important order of causes is diet. Under this head nothing novel, however, is communicated, except a diatribe against the use of tobacco in every form, as if not distinctly inducing the development of oxalic acid, at least that "of some analogous and equally poisonous principle." We confess we cannot consider Dr. Prout's reasoning strong here, though we are equal haters of the weed with himself; as, however, he has *known* "inveterate snuff-taking" lead to "malignant disease of the stomach and liver," there is every excuse, of course, for his warmth, although it be not warranted by mathematical evidence.

The confusion arising from the application of the word diabetes to all affections attended with considerable discharge of urine is justly animadverted on in these pages: their author proposes to restrict it to cases in which the urine is saccharine. The increased flow of urine is the circumstance which commonly directs the patient's attention to the urinary organs; and, as Dr. Prout remarks, "a saccharine condition of the urine exists in gouty and dyspeptic individuals much oftener than is supposed, and hundreds, who are quite unaware of it, pass many years of their lives with this symptom more or less constantly present." We know this to be a fact ascertained by other close observers of the urinary organs also. It follows from this that the physician must always experience much difficulty in determining the period of origin of diabetic attacks; Dr. Prout, however, conceives that, "by enquiring minutely as to the period when the urine was last observed to be turbid," he has several times traced attacks very nearly to their origin. He believes it probable that, at the time the urine becomes clear, after such turbidity, in patients labouring under diabetes, its saccharine condition may be considered to have set in, or, at least, to have become confirmed.

Dr. Prout remarks, "Diabetes very frequently (as far as my personal experience goes, *always*) accompanies carbuncles and malignant boils or abscesses allied to carbuncles;" a fact mentioned by Cheselden* and others of the older writers. These cellular inflammations, however, he is not quite prepared to consider as causes of the affection; and, if he were permitted to draw a general inference from his own observations, he would say "that diabetes usually *follows* cutaneous affections, and accompanies (perhaps *precedes*) the affections of the cellular tissue." The subject is curious, and has, at least in an allied form, engaged the recent attention of a distinguished continental surgeon.†

The symptoms of the affection are lucidly described: novelty is, of course, not to be expected. The absence of any important change in the kidneys is affirmed.

Our author enters very fully into the question of treatment, commencing with the important topic of diet. Admitting that an animal diet—as, indeed, follows from his own principles—ought to form an essential principle in the treatment, he considers a certain proportion of

* Anatomy, p. 139; fifth edit.
† M. Civiale.—See Brit. and For. Med. Rev., vol. X., p. 574.

farinaceous matter proper. This recommendation is founded upon the position already stated, that the assimilation of the saccharine principle is one of the last functions that becomes extinct in animals, and the most essential for the continuation of their existence. This may be very true, but what appears to us much more important is the experiment of Mr. Mac Gregor, proving directly, as might be supposed à priori, that the quantity of sugar produced in the stomach is much reduced under abstinence from vegetable products. However, we do not mean to contest the secondary importance of Dr. Prout's plan; the craving after farinaceous matter, such as we have ourselves witnessed it, becomes so irresistible in individuals submitted for a length of time to purely animal sustenance, that it seems flying in the face of nature to withhold it altogether. Yet, on the other hand, we may dearly pay for yielding to these importunate solicitations, however natural they may appear: long since, Rollo observed that a few mouthfuls of biscuit reinduced the worst symptoms in a subject whose condition had been materially improved by the animal diet. The hint thrown out by Dr. Willis, of mixing perfectly indigestible vegetable matter with strongly nutritive animal diet, founded on the well-known fact of the unfitness of very concentrated aliment for the sustenance of man, seems to us not to be despised. Every crystallizable variety of the saccharine principle is absolutely inadmissible: Dr. Prout has known "the use of a few saccharine pears undo in a few hours all that he had been labouring for months to accomplish."

It must be considered matter of regret that Dr. Prout has, throughout his volume, neglected to notice the remarkable announcement of Schwann and Müller, that artificial digestive fluid, though it perfectly dissolves starch, will not convert this principle into sugar unless saliva be added. Those who believe that the gastric error in diabetes consists, either wholly or in part, in abnormally forming sugar may feasibly make trial of the effect of non-insalivation of the food. The only objection to this seems the difficulty of accomplishing it, for the use of an œsophagus tube is a sad drawback; and, besides, the absence of saliva might, and probably would, entail some new description of disorder. The suggestion, which originated with and displays the practical shrewdness of Dr. Willis, nevertheless, deserves consideration.

Dr. Prout continues, by enforcing the importance of moderation in food. He states that he has found more relief follow the temperate use of and more support given by porter than by any other means whatever; he recommends fluids to be taken tepid. He has no confidence in any medicines recommended as specifics; prescribes venesection under the circumstances in which others have found it beneficial, or leeches to the epigastrium, if there be tenderness there; has seen no permanent benefit derived from the use of purgatives; regards the exhibition of diaphoretics as important, but does not lay the stress upon the use of the vapour bath, which well-ascertained facts warrant; acknowledges the value of opium as a sedative, but deprives it of the shadow of a claim to be regarded as capable of removing the saccharine condition of the urine, and points out that its continued use has the effect, in the end, of making patients confirmed opium-eaters; believes a combination of sedatives with astringents and tonics occasionally useful, and is disposed to think very favorably of phosphate of iron particularly, as the representative

of one class of these medicines. In commenting on the treatment of the complications of the affection, and among these of hepatic disorders, the learned author takes occasion to add very materially to the value of his volume, by introducing some admirable strictures upon the abuse of mercury in such treatment. We agree to the fullest point in the spirit of these remarks, and only regret that their length prevents us from finding room for them in full. The following short extract will suffice to direct attention to them: "I can only say that a large proportion of the most inveterate and dyspeptic and urinary diseases which I have seen have been distinctly referrible to the use of mercury." Well may the physician, who has had experience to this effect, stigmatise those who, "to save themselves trouble, and, at the same time, to gain the doubtful reputation of being decisive and quick in their practice, resort to mercury, without due regard to its remote consequences."

When the urine of an individual is transparent and remarkably free from sediments, of a pale citron-yellow or greenish hue, and of a specific gravity oscillating about and near to 10·20, there exists a concourse of circumstances sufficient to lead the experienced observer to suspect the existence of the oxalic-acid diathesis. Unless corroborated by the presence of such gastric and general symptoms as usually attend that diathesis, it is however more likely that the characters described depend on the temporary influence of certain articles of diet, &c. And caution in forming our judgment is the more necessary, as in the statement of symptoms given by Dr. Prout, we have scarcely been able to detect a single particular which might not exist under very different conditions of the system. Of those apparently most characteristic "a tendency to periodical discharges of dark-coloured blood, both from the rectum and bladder," is the most remarkable; we mean, of course, of those occurring before the period when the presence of a calculus in the kidney has become matter, if not of certainty, yet of strongest presumption. It has long been known that hemorrhage is more frequently produced by oxalate of lime calculi than by others, an explanation being found in the roughness of its mulberry-like surface; but Dr. Prout is disposed to place it more especially in the diathesis itself. We have already noticed the belief of this writer respecting the influence of malaria on the diathesis in question; now at the time of the cholera a greater number of cases of hemorrhage from the urinary organs occurred to him than in any previous period of equal duration; at the time, these cases were taken for examples of calculous or malignant disease, but the bleeding ceased under circumstances precluding a belief in the existence of either of these forms of disease: hence the inference that it depended on the oxalic-acid diathesis.

The non-assimilation of oxalic acid taken as food (in sorrel, tomatos rhubarb-stalks, &c.) or the real assimilations of saccharine aliments, and in extreme cases, perhaps, of the albuminous and oleaginous principles may be regarded as the proximate causes of the disease.

The diet applicable in this diathesis is closely similar to that suited to diabetic patients. Porter in small quantity or dry sherry, even hock and claret in similar moderation, may be taken. The quality of the water used is of the last importance; it is obvious that persons labouring under this diathesis, who drink *hard* water containing lime in solution,

are exceedingly liable to have an oxalate of lime calculus. By adopting a careful diet, and taking a course of mineral acid (muriatic,) three or four times a year, on each occasion for about a month; (or, more accurately speaking, until lithate of ammonia or lithic acid begin on each occasion to appear in the urine) Dr. Prout has seen the diathesis gradually subdued and at length removed altogether.

With the following section, devoted to the subject of abnormal development of lactic and allied acids in the stomach, we shall not long delay.* The development of this acid in excess, by the *primary* assimilating processes, is stated to constitute one of the most common and troublesome forms of dyspepsia; by the *secondary*, to give occasion to many serious and most painful affections, of which rheumatism and neuralgia are the most remarkable. The last portion of the chapter is so intricately hypothetical that it would require some pages to explain what, as we conceive, is by no means worth the space. The practical part of the section is good, and there is valuable truth in the considerations on the exhibition of alkalis.

The derangements of albuminous assimilation next engage Dr. Prout's attention; and as these are most prominently marked, or at least best identified by the changes they induce in the urinary secretion, he makes them the basis of his description and arrangement. These derangements are of four kinds, according as they are accompanied by (*a*) excess or deficiency of urea; (*b*) by the presence of albuminous matters in the urine; (*c*) by that of lithic acid and its compounds; (*d*) by that of cystic oxide.

Diuresis with excess of urea is, by Dr. Prout, regarded as a rare affection; for one such case he has himself seen twenty of true diabetes; nevertheless he suggests that the disease may, in some cases, pass unnoticed from the patient's not applying for advice, until it has merged into diabetes or some other formidable affection.

Dr. Prout retains his former division of albuminous urine into the chylo-serous and serous. Of the remarkable affection characterized by a discharge of milky-looking urine, he has now had an opportunity of seeing more or less of thirteen cases, but he does not appear to have ascertained any new facts on the subject since the publication of the lectures already referred to; and the important parts of these have, in various forms, been brought before the public. In seeking for the proximate cause of the affection, he is led to suppose "that the chyle from some derangement in the process of assimilation is not raised to the blood standard; and consequently, being unfit for the purposes of the economy, is, agreeably to a law of the economy, ejected through the kidneys; but these organs, instead of disorganizing or reducing it to the crystallized state as usual, (that is to say, instead of changing it into lithate of ammonia,) permit it to pass through them unchanged. That this is a just view of the matter cannot, I think, be doubted; for if the chyle was properly converted into blood, not chyle but blood ought to

* Does not Dr. Prout contradict a previous opinion in affirming here, that the presence of muriatic and *lactic* acids appears to be necessary to the accomplishment of the reducing function of the stomach; for at page xxvi. we read, "that lactic acid is rather to be considered as the result of unnatural irritation, than as a healthy product *necessary* (the italics are the author's) to the digestive process?"

be thrown off by the kidneys." Of the essential proof of the presence of chyle, namely, that of the chyle-globule, Dr. Prout says nothing. Respecting the treatment, his experience furnishes him with no suggestion of value; counter-irritation in the form of seton may do good at first, but the complaint returns after the system has become familiarized to the stimulus of this; the same may be said of mineral acids and of opium. It is consolatory to know that the general symptoms are frequently of trifling severity, that such indication of the existence of the affection may be altogether absent, and that temporary spontaneous disappearance of the complaint is far from uncommon. The narrative of the case of a woman, commenced in the former edition of the work, is brought to a conclusion in the present; she died in an emaciated state in 1836, after having laboured under the affection for nearly twenty years.

Pass we now, under the guidance of Dr. Prout, once more to the subject of albuminuria, a morbid state so fully discussed of late in the pages of this Journal that our task will be now limited to an exposition of the original matter, speculative or otherwise, of the author. Diseases of this kind are by him considered "as of two principal kinds or species only; one of which may in general terms be said to be of an *acute*, the other of a *chronic* character." The former includes two varieties, the latter eight; all ten pass into each other by imperceptible degrees; the only distinguishing feature in the urine being that, in the two acute, that fluid lets fall lithate of ammonia on cooling, while in four of the chronic species this phenomenon never occurs. Before entering upon the particular history of these varieties, Dr. Prout delivers his opinion upon an important question of general pathology. It is a mistake, he conceives, to consider, as French authors more particularly do, inflammation as almost the only cause of disorganization. That inflammation is the immediate cause of death in most of the diseases of the kidney distinguished by the suffix *itis*, he does not deny; but that even when qualified by the epithet chronic, the word rightly designates that comparatively quiescent state of the kidneys which immediately preceded the fatal inflammatory attack, he refuses to believe. All organic affections may, he maintains, "be supposed to arise from two distinct causes, degeneration and inflammation." By the former is meant that slow and gradual change inseparable from advancing age, and therefore normal; though it may be induced abnormally by inherited and innate, or by acquired weakness. The causes of the latter may be slowly acting, such as continued errors of diet, or acute, such as inflammation, severe accidents, &c. From this view Dr. Prout draws five inferences; four of these are so clearly involved by the premises, that there is no necessity for stating them. The fifth proclaims that "the appearances presented after death under these circumstances are quite useless in a pathological point of view, because it is impossible to distinguish what is due to degeneration and what to inflammation." Finally, Dr. Prout has not seen this doctrine, though it may so exist, exposed *todidem verbis*; he, however, confesses his reading on such subjects has been limited, as in consequence of the neglect of these and other important distinctions by authors, "their works present a confused and unphilosophical jumble." From this flattering compliment to the existing tribe of pathologists and patholo-

gical anatomists, let us turn to the theory, the conception of which has demonstrated to Dr. Prout the worthlessness of all existing pathological information. It may be stated, first, that to the term degeneration there is this strong objection, that it has been, and still is, used by numerous writers in a completely different sense. Next we would ask, does it not bespeak an unwarrantable degree of self-confidence in an author to state a doctrine presumedly subversive of pre-existing knowledge, without attempting to support it by an elaborate series of arguments, and above all of facts? The question is simply this: do the characters of what the experience of all countries has united in terming chronic inflammation, exist in certain cases? Secondly, if so, are these characters phenomena which arise, and only arise, from a previous condition of tissue, known conventionally as inflammation? Dr. Prout avers the negative of both positions; but his intimate conviction of this seems to be the proofs with which we must satisfy ourselves of its correctness. It would be less unfortunate that much of Dr. Prout's chapter turns upon this mode of classing organic changes, did he tell us what this degeneration is, what the characters by which it may be recognized: but nothing of the kind is distinctly done. The subject of serous urine is, in pursuance of these views, presented by Dr. Prout in the following manner:

Species *a*, serous urine; the kidney in a state of health.	Var. 1. Quiescent. Var. 2. Inflamed.
Species *b*, serous urine; the kidney in a state of degeneration.	Var. 1. Quiescent. Var. 2. Inflamed.

We shall, with as much brevity as possible, state the leading characters assigned to these varieties.

. Spec. *a*. var. 1. Here we have the healthy kidney quiescent; is albumen ever present in the urine under such condition of these organs? Our author admits that it is, for example, as a consequence of admixture with blood, or with that species of pseudo-serum thrown out during inflammation of the mucous surfaces. Again, a suspension of the healthy disorganizing function of the kidney (that whereby it converts albumen into lithate of ammonia) may take place when some condition of the kidney approaching to the inflammatory, coexists with certain derangements of the system at large. Such is supposed to be the state of things when albumen appears in the urine in consequence of eating cheese, of using cantharides, &c. Further, as albuminuria is not always produced in all subjects by such causes, he infers that "in the person liable to be so affected, there exists a sort of latent predisposition (incipient degeneration) to kidney disorder." Functional derangements, productive of albuminuria, are thus admitted; but it is advanced as "not improbable, that such functional derangements may partake of the character of incipient disease." It will be perceived, that so far as Dr. Prout states the result of observation, his statements agree with the views taken in the articles on Renal Pathology in this Journal; that he should differ from us, when he wanders into the region of speculation, we can really feel no concern.

Spec. *a*. var. 2. Here we have the healthy (that is, the non-degenerated) kidney seized with inflammation: the urine is albuminous, of deep colour, below the natural standard in quantity, varies in specific

gravity from 1018 to 1030, or more; sometimes contains blood and deposits a deep brownish-red sediment of lithate of ammonia. This state of the urine is accompanied with a tendency to a species of anasarcous œdema, known as inflammatory dropsy. We need go no further to show that Dr. Prout here has in view the first stage of the *néphrite albumineuse* of Rayer, in other words of Bright's disease; but as there is no formal description of the anatomical characters of the disease, how are we to understand that Dr. Prout has personally ascertained the reality of an inflammatory condition; a point hitherto distinctly contended for by Rayer alone? The proximate cause of the affection our author places in an inflammatory state of the system generally, but involving the kidneys in particular; an hypothesis conceived in order to explain the difference in the train of symptoms induced by this disease and by simple acute nephritis. Persons who do not die during the acute stage of the disease "generally perish sooner or later with all the symptoms of degeneration of the kidneys and serous urine, in their worst forms:" the affection is thus further identified with the first stage of Bright's disease. "Allied to it," says Dr. Prout, "is the anasarca following the exanthemata." Bleeding, the exhibition of diaphoretics, and after awhile of mild diuretics, constitute the treatment recommended. When the disease has originated after scarlet fever, it has occasionally, as we are reminded, been treated with large doses of calomel, "which no doubt contributed its share towards the production of the chronic forms of the disease under which the patient laboured."

In species *b*, the kidney is degenerated. Upon the character of the degeneration particular ages appear, it is alleged, to exercise the most fundamental and important influence. When renal disease occurs before the age of forty, it is almost always, we are told, to be considered as acquired *de novo*, or as resulting from inherited predisposition or struma; when after that age, partly from long-continued and slowly-acting causes giving occasion to gout, &c., and partly from natural decay of the vital powers. These facts, as they are called, supply the inference that *anæmotrophy* will constitute the leading feature of the diseases of the kidney in early life and *hæmotrophy*, "from the plethora produced by the daily use of a generous and stimulating diet, in advanced years."* It is not however contended that hæmotrophy does not exist in early life and anæmotrophy in old age; but these conditions are then coupled with disorganization. These two states of defective nourishment are, in Dr. Prout's view, so practically important, (little does it bespeak of perfection in our science and our art, when that can be pronounced practically important which is essentially hypothetical,) that he makes it the basis of his arrangement of renal affections connected with serous urine. Let the reader bear in mind that Dr. Prout does not by any means appear to have, or affirm that he has, observed, with such frequency as to exclude the idea of accidental coincidence, the fact of these anatomical conditions being more or less peculiar to a given age; but that imagining from such *à priori* considerations as are stated above that such *should* be the case, he finds no difficulty in endeavour-

* Anæmotrophy and hæmotrophy mean respectively excess and deficiency of sanguineous nourishment, and therefore differ both from anæmia and hyperæmia, and from atrophy and hypertrophy.

ing to persuade his readers that it is so. Here is his arrangement based on this bipartite division of renal nourishment:

<table>
<tr><td rowspan="4">Species δ. Kidney degenerated.</td><td rowspan="2">Sect. I. In a state of Anæmotrophy.</td><td>Subspecies α. The kidney in a state of *organic change*, but without any *visible* derangement of its ultimate structure.</td><td>Var. 1. Quiescent.
Var. 2. Inflamed.</td></tr>
<tr><td>Subspecies β. The kidney in a state of disorganization, i.e., having its ultimate structure more or less visibly destroyed.</td><td>Var. 3. Quiescent.
Var. 4. Inflamed.</td></tr>
<tr><td rowspan="2">Sect. II. In a state of Hæmotrophy.</td><td>Subspecies γ. The kidney in a state of *organic change*, &c., as in α.</td><td>Var. 5. Quiescent.
Var. 6. Inflamed.</td></tr>
<tr><td>Subspecies δ. The kidney in a state of disorganization, &c., as in β.</td><td>Var. 7. Quiescent.
Var. 8. Inflamed.</td></tr>
</table>

Considerable confusion arises from Dr. Prout's manner of examining the phenomena of these different forms of disease; we must, however, follow the order he has adopted, in such observations as we feel called upon to make. He first considers the state of the urine, then that of the kidneys, (even this very order of priority exhibits the author's inclination to elevate the importance of symptoms at the expense of lesions, and thus, *pro tanto*, make causes subservient to their effects,) together with the causes of the two quiescent varieties 1 and 3. He then follows the same plan with regard to the other quiescent varieties 5 and 7; and, lastly, considers the diagnosis, prognosis, and treatment of the four collectively. A different order altogether is observed in examining the inflamed varieties 2, 4, 6, and 8.

Spec. *b*. Sect. I. The kidney is anæmotrophous and quiescent. The diseases comprehended in this section may exist under a variety of forms, but for all practical purposes may be reduced to the two subspecies α and β. In the former the urine is not habitually, but from slight causes becomes more or less serous, proving "that the kidneys *must* be in some incipient condition of organic change, though their ultimate structure be not visibly deranged." If advanced by a pathologist of the organic school, the clause quoted would not involve a *petitio principii*, provided the fundamental dogma of that school be supposed to be granted; but Dr. Prout, in thus expressing himself, begs the question at issue most completely; and besides, shows how easily we may persuade ourselves of the reality of the doubtful, when we suffer theory instead of observation to be our guide. In truth, what Dr. Prout had but a few pages before hesitatingly proposed as "not improbable," is now boldly announced as a necessary truth.

Subspec. α. Var. 1. Our author's experience "assisted by analogy," leads to the following very unsatisfactory exposition of the state of the kidneys in this variety. Their size is equal to, greater or less than that of the healthy organs; they are "usually" paler than natural; "the ultimate structure is not sensibly changed, yet often presents an appearance indefinably unnatural; the texture is sometimes irregularly firm or *lumpy*, more rarely softer than natural . . . . . and generally the kidney may be said to partake of that want of natural appearance and development which can hardly be verbally expressed." We confess this mode of description appears to us most extraordinary. It would, in

truth, be impossible to draw from it a single definite idea as to the anatomical condition in view. How any object can appear unnatural, and yet not be sensibly changed from the natural state, we are at a loss to understand; and, above all, the idea of drawing up anatomical descriptions upon "analogy" (analogy of what?) is a novelty for which we were not prepared.

Subspec. β. Var. 3. In illustration of the state of the organ here, the author "limits his attention" to the "granulated kidney" of Dr. Bright, believing this condition the only one susceptible of description in the present state of knowledge. Hence it is to be inferred that there are other changes besides those made known by Dr. Bright referrible to the present head; but what these are, Dr. Prout himself is unable to inform us.

The constitutional symptoms of both these varieties are carefully considered by Dr. Prout; with those attending Var. 3, he joins the morbid states described as "secondary disorders" by Dr. Christison, from whose recent volume the greater part of the details on these appears to be derived. Hereto is appended an announcement by our author of his acquiescence in the notion already advanced by the Scotch writer in nearly similar terms, that individuals labouring under "granular degeneration" are "peculiarly liable to the invasion of some epidemic diseases." Not the semblance of a proof is adduced in support of this most important doctrine. Neither the necessity for, nor the difficulty of obtaining proof appear to have flashed across our author's mind. He has not reflected that the following steps would require to be taken in order to establish the assumed position: 1. To learn accurately what proportion of persons out of a given number affected with "granular" disease were attacked with a prevalent epidemic. 2. To ascertain what number of individuals out of a similar amount of population, unaffected with such renal disease, suffered from the epidemic. 3. To have evidence that both series of individuals, thus compared, were subjected to the same, or as nearly as possible the same, physical and social conditions. 4. Admitting that there were an excess of victims to epidemic disease among the sufferers from the renal complaint, to discover, by learning the amount and extent of organic disease of other kinds among the subjects belonging to the category free from "granular degeneration," whether the observed excess might or might not be attributed to the influence of the affection of the kidney simply as *organic*, and not specially as *renal*, disease. 5. Admitting that the result were in this instance also favorable to Dr. Prout's notion, it would be necessary for the substantiation of this, that the excess on the part of persons with the renal disease should be far from inconsiderable. 6. And, further, the point could not be considered to be demonstrated by the existence of even a notable excess of the kind during a *single* epidemic. Several successive epidemics should be observed with similar results, as these might in the first instance be the effect of hazard. All this may be troublesome enough; few are wont to prefer the enduring toil such mode of examining a subject requires to the comfortable beatitude of an easy chair, a well-nibbed pen, and a little of the imaginative faculty; of him, thus triply armed, conclusions are ever ready at the beck.

Spec. *b*. Here the kidney is hæmotrophous, and may be invisibly (subsp. γ) or visibly (subsp. δ) deranged. The quiescent varieties are

therefore two, 5 and 7. In var. 5 the urine is characterized by being transparent when passed, becoming turbid after a while from copious separation of lithate of ammonia, and again clear occasionally from subsidence of that salt; such subsidence is, however, rarely complete; but the fluid may be rendered perfectly transparent by a very gentle heat, and frequently again made turbid from the deposition of albuminous matter, by raising the heat to 130° or 140°. The kidney is normal or not so in point of size, of deeper colour than natural, and "generally the kidneys seem to partake of that unnatural character, and forced or excessive development, *which hardly admits of description*, but which may *readily be supposed* [!] to be produced by long continued full diet, and the free use of vinous liquors upon the system in general, and upon the congested abdominal viscera in particular."

Var. 7. The disease is now established. The urine is almost always albuminous, "particularly after meals." In the more advanced stages it becomes impossible to lay down rules respecting the other properties; such as specific gravity of the fluid, as the original affection is liable to become complicated with other diseases of the kidney which modify them materially. The presence of these complications is asserted to be no bad diagnostic mark of the peculiar condition of the kidney now under consideration, as more immediately contrasted with the granulated form of disease, "in which," according to Dr. Prout's observation, as well as that of others, "actual calculous depositions, and even abscesses, may be said to be rare." The size of the kidney increases with the advance of the malady; its colour is dark, as if from vascularity; and the texture of the organ is softened. The section of the cortical substance is almost always broader than in health, and may present a distinct granular appearance, "though this is often entirely wanting, and the whole mass assumes a uniform homogeneous appearance, like that of the brain, or occasionally like that of degenerated fatty liver." Obliteration of the natural structure follows, and the cortical and tubular substances may eventually be so completely absorbed, that the once enlarged organ is reduced to a small flabby irregular capsule. Such is Dr. Prout's account of the quiescent hæmotrophous disorganized kidney. However familiar we and our readers may be with the individual appearances thus loosely described, we feel persuaded the concatenation of these must be new to them as it is to us. That an enlarged congested kidney should, in the natural order of things, become infiltrated with encephaloid matter (for this is the only lesion with which we are acquainted whereunto the phrase "possessing an appearance like that of the brain" is at all applicable) is in itself a startling affirmation; but the implied notion that it is a matter of indifference whether the original congestive enlargement pass into a state of such infiltration, or into a condition having the outward aspect of fatty liver, we should have thought the *ne plus ultra* of "analogical" morbid anatomy, were we not next told that "in the last stages" of the disease, the whole organ (previously changed into, it may be, encephaloid matter) is sometimes converted into a small cyst or capsule! With all our strong respect for Dr. Prout, we cannot consent to hold as feasible, notions flying in the face of all accepted doctrine, and can only express our very sincere regret that so truly gifted a man should have fallen into the sad fallacy of supposing

that closet speculation, however profound, however varied, and however sustained, may supply the place of that essential knowledge furnished by the scalpel alone.

The discussion upon diagnosis is prefaced by some remarks upon the state of the blood in these affections; these are, however, principally a summary of the more important of the facts elicited by the researches of Dr. Christison. The diagnostic discussion itself bears upon the distinctive symptoms of the anæmotrophy of early life, and the hæmotrophy of middle and advanced age: the recapitulation of these exhibits a very striking amount of difference between the effects of the two species of lesion, as Dr. Prout has conceived them.

The prognosis of the four quiescent varieties is considered at some length. The blood, the urine, and the concomitant affections are examined as indicating the character of this. In respect of the former, we are told that when the position and relative proportions of the ingredients of that fluid are not materially altered, or altered in so far only as may depend upon an inflammatory condition, an early and controllable state of disease is indicated. But it is naturally added, that deficiency of hæmatosin, unless this can be referred to previous depletion—the influence of which on the colouring matter has been very clearly shown by Dr. Christison—as well as general diminution of the solid constituents of the blood, announce an advanced and generally irremediable state of disease (anæmotrophy.) As respects the urine, high specific gravity, deep colour, moderate quantity, *abundant* albuminous impregnation, (here Dr. Prout agrees with Dr. Christison in opposition to certain pathologists, who maintain that the quantity of albumen increases with the advance of the disease,)* and the presence of lithate of ammonia denote an incipient stage of disease: the opposite conditions, a form of anæmotrophy, from which recovery is impossible. It must be remembered, however, that Dr. Christison's experience is not to the same effect exactly in respect of the albumen, for he has observed the quantity very great in the worst stages of the disease, when fresh reaction may have occurred.

In examining the question of the compatibility of renal disease with life, Dr. Prout points out the important fact, that this depends mainly on our success in averting acute intercurrent inflammation of the affected organs; that hence upon the habits of life of the subject, upon the care with which he avoids exposure to cold and intemperance, mental anxiety, and severe bodily exertion, must our prognosis be mainly built. "I am decidedly of opinion," says our author, "that a very large proportion of those who labour under renal disease may live for many years and enjoy tolerable health; indeed better health and more comfort than are compatible with many other chronic and incurable diseases."

The remarks on treatment display Dr. Prout's practical skill in the management of these diseases in a very favorable light. Nevertheless, if satisfactory in this respect, their perusal is far from encouraging, for they show very distinctly that the highest degree of such skill coupled with great opportunity for its exercise, is unlikely, in the present state at least of *scientific* acquirement on the subject, to lead to any marked

* See Brit. and For. Med. Rev., vol. X. p. 312. Oct. 1840.

wer of controlling, never of curing, these maladies. The three great ints to be constantly borne in mind in the conduct of these cases, are ention to diet and regimen, the guarding against the influence of ld and moisture, and the avoiding all violent remedies. The diet ould be light and nutritious, and free from stimulating matters; flannel ould be constantly worn next the skin throughout the year, and, during winter and spring, the addition of leather is advisable. Moderate ercise, always short of fatigue, and if circumstances permit, easy avelling by land or sea are beneficial. Copaiba, mercury, cubebs, rpentine, and similar medicines are to be avoided as destructive. eches or cupping-glasses to the loins may be indicated in the outset, d mild but efficient purgatives, if the hepatic system be congested. e gastric symptoms, acidity, flatulence, and nausea, are best relieved prussic acid, sometimes united with small doses of the fixed alkalis, utiously administered. The light bitters and tonics, such as the *infus. uli*, or *cinchon.*, prepared with *cold distilled water*, are occasionally eful. If there be irritability of the bladder, the *uva ursi*, *hyoscyamus*, the *diosma* and *pareira brava*, either alone or conjoined with acids, the properties of the urine may indicate, are to be recommended.

Of the disease affecting individuals of advanced years the treatment somewhat different. If there be an acute attack attended with hepatic mptoms, venesection, the judicious use of mercurials, and of the more tive diuretics are advisable. Active mercurial treatment is however be carefully eschewed, but to mild alterative doses of this remedy, ther alone, or, if gouty irritation exists in the system, conjoined with e acetous extract of colchicum, Dr. Prout admits there can be no obction. When the active symptoms have abated, the diuretic effect of e remedies may be occasionally assisted by the use of effervescing aters; and if the patient have been accustomed to wine or spirits, a tle sound sherry or hock, or a little hollands may be added. The ine gradually regains its natural appearance and transparency under is mode of treatment, and if then examined, will be found in many ses [?] to have lost its albuminous character; whence Dr. Prout infers at formidable renal disease has not existed. Very similar conduct is commended in the advanced and confirmed stages of the disease. The steady use of some diaphoretics" is declared to be now often beficial, "provided their action be at the same time directed to the lneys, or at least be not calculated to unduly suppress the action of se organs. One of the best I know is the citrate of ammonia, which ay be variously associated, either with ipecacuanha and the syrup of ppies, or with Dover's powder, the extract of sarsaparilla, &c." An ue or a seton in the renal region is recommended for its palliative ect. We cannot, we think, close our notice of this subject better an by laying before the reader the following emphatic injunction of a actitioner whose experience entitles all his practical observations to ious consideration: "it should be constantly kept in view, that in all ms and stages of renal affections, and particularly in the advanced ges, *it is much easier to do mischief than to do good*; and that the eat principles to be kept in view are to prevent evil as much as posble on the one hand, and on the other to meet promptly symptoms as ey arise, particularly when of an inflammatory character."

We find little calling for particular notice in the few pages devoted to the four inflamed varieties, 2, 4, 6, and 8. The writings of Dr. Bright, and more especially the Atlas of M. Rayer, are referred to as containing delineations of everything that has been observed. The reference seems to us to come rather singularly from our author; but the reason of the recommendation is that, "as by far the greater proportion of kidney diseases prove immediately fatal by the supervention of inflammatory action on their degenerated structure, the appearances, described by authors as occurring after death, present in general faithful pictures of the phenomena now under consideration." Why, we had thought that they jumbled together the effects of "degeneration" and of inflammation. But be this as it may, we wish it to be understood in respect of Dr. Prout's hæmotrophy and anæmotrophy, their relation to age, &c., that we do not take upon ourselves to deny them all foundation in truth and observation; the subject demands special enquiry. Still we unhesitatingly affirm that he has utterly failed to make out even the semblance of a case.

We need only notice some isolated points in the chapter on lithic acid deposition. The different forms in which this occurs are stated to be remarkably under the influence of age: in the earlier periods of life the tendency is to the deposition of amorphous sediments and crystallized gravel; in middle age the amorphous concrete mass commonly occurs, giving occasion to the formation of renal and vesical calculi; and lastly, in more advanced age there is occasionally a disposition to the development of numerous small lithic calculi (pisiform concretions) around minute nuclei. Although exceptions to these general laws occur, yet they are of sufficient constancy, according to Dr. Prout, to allow of the age and constitutional symptoms of a patient being predicated from a simple knowledge of the characters of the lithic matter passed by him.

The causes of lithic sediments are discussed at much length. Among the predisposing, Dr. Prout ranks hereditary influence, citing as proof of the reality of such influence, that "he has repeatedly seen the children and grandchildren of individuals who had suffered from lithic acid concretions liable to the same depositions." This affords a presumption in favour of the alleged fact, but does not prove it. Lithic deposition is perhaps so common a urinary derangement, that its occasional occurrence in successive generations is likely to be observed in the natural course of things, as a mere matter of accidental coincidence. To *demonstrate* the reality of the etiological influence in question is a more serious matter than Dr. Prout seems to imagine; but the present is not the place to discuss the subject. The existing causes are classed under the three heads of diet, exercise and atmospheric influences. Of exercise it is remarked, that if taken so as to interfere with the processes of assimilation, as for instance immediately after a meal, it is very prone to induce lithic deposition. In like manner, certain exercises affecting "the dorsal system," hard riding for instance, have been observed to produce urinary sediments in those unaccustomed to such motion. On the other hand, the want of exercise after a certain stage of the digestive process is completed is usually followed by similar consequences; indeed, of the influence of full living and bodily inaction none can entertain a doubt. A theory of the *proximate* causes of the affection is next traced by Dr.

out in the following manner: 1. Lithic acid and its salts are, as he ssumes, and as has already been stated in this article, principally de-ed from the albuminous principles of the chyle, the blood, and the uminous textures of the body; as urea and lactic acid are conceived be produced from the gelatinous tissues. Now when on account of perfect assimilation the chyle is not raised to the blood standard, the dney, as Dr. Prout supposes, possesses the power of selecting and dis-ganizing such imperfect matters, and of converting them into the lithate ammonia; hence the *yellow* amorphous sediments occurring from ght errors in diet. 2. During feverish affections, especially when the er is involved, the lithate is supposed to be derived from the two urces above mentioned, and also from the deranged *secondary* assimi-tion of the albuminous tissues. The salt now appears more especially der the forms of the *red* and pink amorphous sediments, and is dis-nguished by the large quantity of colouring matter formed with it. The massive forms of lithic acid originate from the same sources as e above, but when thus deposited the acid is secreted either in con-xion with other acids, which set it free by combining with its ammonia, in connexion with other bases, as soda, which compounds are of very ferior solubility to the lithate of ammonia. With regard to the general thological relations of lithic acid and its compounds, Dr. Prout hazards e hypothesis, "that the lactic and lithic acids, considered with re-rence to rheumatism and gout, may be regarded somewhat in the light f *materies morborum*." Having developed this notion within certain mits, he adds that "he does not consider it worth while to illustrate ese views further, much less to enter into a formal defence of them." e can scarcely expect others to examine with any care doctrines which ren the partiality of authorship cannot persuade him to be worthy of efence; the only puzzle to us is, that such imperfectly elaborated notions ould be considered deserving of publication.

The great point in the prevention of the yellow amorphous sediment is ttention to diet; and here Dr. Prout regards *quantity* as of much more nportance than *quality*. That "any stomach may digest a *little* of *nything*, but no stomach a *great* deal of anything," is a maxim to which e attaches the extremest value. However, it need scarcely be told that ren a minute quantity of articles of food, confessedly indigestible, should e carefully avoided. When the yellow sediments are very pale coloured nd liable to follow slight atmospheric changes and errors of diet, they ommonly denote a feverish state of the system, resembling that which ccompanies phosphatic deposition. The treatment then merges into at of this more dangerous variety of urinary affection. The more luable part of the section on the treatment of pulverulent or massive thic-acid deposits is contained in the following summary. Dr. Prout ems to settle the disputed point of the comparative appropriateness of nimal and vegetable diet, by giving the palm to neither. Certain spe-mens of either class, such as salted and dried meats on the one hand, nd potatoes and subacid fruits* on the other, will, in some subjects,

* How does Dr. Prout account for the observed fact, that subjects tortured with gout d lithic deposition frequently experience notable relief, during the season, from free dulgence in such fruits? Here is evidently not a case of *habitual* use of the presumed leterious agent.

produce abundant lithic deposition; while ordinary food of botl
if taken in moderation, is innocuous in this respect. The disti
animal and vegetable origin is probably quite out of the questi
instances of disagreement above referred to. Similar notions
pressed on the subject of varieties of wine. Persons accustom
"strong, brandied wines," usually consumed in this country, ar
sure to suffer from a gravelly attack, if they indulge in weak a
wines, such as the inferior rhenish and champagne; yet individ
customed all their lives to such beverage,* or to the free use of
perry, rarely suffer from gravel. As regards ordinary diluents, D
conceives weak tea to be the best, reprobates the use of malt l
every kind, and milk, and dwells upon the importance of us
water, and of taking a moderate share of exercise, on foot espe
the patient have the use of his limbs. On the exhibition of alk
preventives of deposition, as solvents of concretions already forn
as constitutional alteratives, Dr. Prout severally speaks. They
prevent the development of, but only remove already existing
are therefore best prescribed about four hours after eating, (tl
riments of Schwann seem to us to throw some doubt upon the
admissible at this period,) generally in the form of from 10 to
of carbonate of potass; and not, as is usual, in company witl
though the separate use of both these classes of medicine may b
beneficial: thus the author gives alkalies after, and acids as toni
and between meals, "with the best effects." On the question
solvent power, Dr. Prout "has no doubt that in some constitutio
doses of alkalies can be taken for a great length of time with a
impunity, and so as not only to affect the urine, but even to act
acid calculi lodged in the kidney and bladder;" yet he cannot
the practice in general as a safe one, and that its application is
instances impossible, is well known. When thus used, they
taken dissolved in a large quantity of water, as indicated by t
dition in which they exist in the Vichy and other mineral waters
class. Of the *intolerance* of alkalies, sometimes witnessed, Dr
refers to a striking example; in this case the excitement produ
their ingestion bordered on delirium or mania, and though ev
strongly indicated their use, they were in vain tried in every form
persisted in would probably have poisoned the patient. Dr. Pr
not comprehend the meaning of the charge urged against alk
Huxham, Dr. Copland, Magendie, and a host of other experime
that they "attenuate the blood," and therefore cannot speak
subject. The age of the patient, inasmuch as it modifies the c
of the lithic deposition, the general condition of the urinary p
and of the tone of the digestive organs, &c., exercises a modif
fluence on the treatment; this part of the subject the reader
very usefully illustrated in Dr. Prout's volume.

The notice of cystic oxide deposition is as brief as the extent o
ledge respecting it is limited. The peculiar composition of this su

* We have heard it stated by medical men on the spot, that calculous comp
unknown on the banks of the Moselle, while we have not heard such immunit
for those of the Rhine. The inferior Moselle wines, such as are consumed by
santry and boatmen, are of the most harsh and pungent acidity.

nd particularly the sulphur entering into its constitution, are the grounds or classing it with principles of albuminous origin. These conditions 'appear to show that its formation results either from an imperfect assimilation of the albuminous principle, or most probably from the future subsequent] action of the kidney on such imperfectly developed albuminous principle." Among remedies, alkalies may be sometimes proper, but acids are much more frequently indicated. Of the latter, Dr. Prout has generally preferred the nitro-muriatic, which he was first induced to employ from observing that it removed the peculiar odour, apparently closely allied to that produced by the cystic oxide, occasionally connected with the urine. This acid will remove the foreign principle, while its administration is persevered in, but when given up, the urine returns to its former state. The prognosis is generally unfavorable. With the notice of this substance closes the chapter on the pathology of albuminous assimilation.

In the third chapter are discussed the morbid conditions of oleaginous assimilation. The affections thus arising, in Dr. Prout's arrangement, are first those dependent on excess or deficiency of the oleaginous principle (obesity, leanness); secondly, those originating in change in the qualities of the principle, as an illustration of which the formation of biliary concretions is adduced. The different conditions which are presumed to have an influence on the development of fat, or the contrary, such as inherited tendency, climate and locality, diet and exercise, are each considered. The mode of fattening geese, by securing them immoveably in a high temperature and cramming them immoderately with food, is cited in exemplification of the effects of high feeding, elevated temperature, and rest. Dr. Prout, in justly stigmatising the barbarous cruelty committed in the production of that epicurean morsel, the *pâté de foie gras*, observes, and there is satisfaction, we had almost said, in hoping that such is the fact, "it is probable that in this and many similar instances, the poor animals sometimes *have their revenge*. Indolent and dyspeptic individuals, who partake of these diseased and poisonous productions, can scarcely be supposed, in all instances, to assimilate them; and consequently run considerable risk in inoculating and converting their own livers or other organs into a similar mass of disease." Among the causes of leanness, Dr. Prout omits to notice shortness of the intestinal canal which, in at least one well-authenticated instance of such state, combined with monstrous bulimia, appeared to afford a plausible explanation of the phenomena. After all, idiosyncrasy seems to have as great an influence on the matter as anything else; there are individuals who would, as is their own expression, grow fat on bread and water. Respecting the functions of fat in the animal frame, Dr. Prout remarks that though, as is commonly admitted, it partakes less of the character of a living organized substance than the other fundamental principles, yet that it, or some nearly allied principle, admits of the highest degree of organization of which matter is perhaps capable, appears from the large proportion in which it enters into the composition of the cerebral and nervous tissues, in association with phosphorus and other incidental mineral matters.

Matter of more attractive character than anything contained in the section on biliary calculi induces us to pass over this subject with the

statement, which throws out a hint for future enquirers, that while Dr. Prout has noticed cholesteric concretions to be most usually associated with lithic acid in the urine, (the common belief is that the two things rarely coexist), he fancies he has found the other kinds of concretion, and especially the inspissated-bile calculus originate in cases where the tendency to the formation of oxalic acid and to "malignant disease," more especially of the liver, prevails.

In his fourth chapter, Dr. Prout enquires into the pathology of the incidental matters of organized products; the latter are of two kinds, soluble and insoluble, hence the formation of two subspecies. The affections connected with the insoluble compounds, the triple phosphate, and the phosphate of lime, (the latter of exceeding rarity), are first considered. The phenomena attending the separate deposition of these salts are passed in review, but no actually novel facts are brought forward. Of the proximate cause of the deposition of the triple phosphate, our author conceives that we know but little. Yet he is persuaded from all its pathological relations, that it is connected with the inordinate consumption or malassimilation of some tissue, "intimately connected with the organic system of nerves." He asserts that when the triple phosphate is deposited in large quantity, the material of the tissue referred to (probably in a modified form) is often likewise present in the urine. This material, he conceives, is usually confounded with mucus, though perfectly distinct from that principle, and assures us that he has been long familiar with its characters; but what these characters are, he makes no effort to teach his readers. The exact nature of the tissue, during the malassimilation or destruction of which the phosphate of lime is eliminated, Dr. Prout does not pretend to define; but has no hesitation in saying, that it is quite different from that engaged in the production of the triple phosphate, and seems to have more relation to the dermoid variety of the gelatinous tissues.

A much more common derangement of the urinary secretion than the separate excess of either of these salts is the combined overplus of both. Deposition of the mixed phosphates is not often, according to Dr. Prout, an idiopathic disease, but usually depends on local affections of the urinary organs. Of this we are perfectly persuaded; but we feel surprised that our author does not distinctly mention disease of the kidney itself as among the causes of such derangement. We conceive a sufficiently strong case has been made out by M. Rayer, in favour of the opinion that phosphatic urine almost always depends, in cases where it has been the habit to ascribe its occurrence to a particular diathesis, upon chronic nephritis, to merit the careful consideration of the practitioner in the position of Dr. Prout. Nor do we think our author happy in eschewing all recognition of such inflammation as an intermediate condition between "injury of the back," and the appearance of phosphates in the urine,—in all probability the real and essential cause of such appearance.

Urine, containing the soluble incidental matters, soda, potass, and ammonia in excess, is next described. It is alkalescent when passed, of ammoniacal and peculiar odour, pale coloured or reddish, opaque from the presence of pus or mucus, usually contains a less proportion of phosphates than the healthy fluid, and as the alkaline bases are combined

with carbonic acid, effervesces strongly on the addition of an acid, is deficient in urea, contains a large proportion of lithate of ammonia and soda, varies in quantity and in specific gravity, is accompanied with constitutional symptoms like those attending phosphatic deposition, and originates in virtue of the same predisposing and exciting causes.

The diet of subjects discharging phosphatic urine is a point of primary consequence. Dr. Prout's experience proclaims loudly the superiority of nutritious *animal* food; M. Rayer's, as we stated on a former occasion, is to the same effect: the question may therefore be considered to be decided. But respecting the use of acids, whether medicinal or in the form of ascescent wines or fruits, we do not find the same harmony of opinion. Dr. Prout, as is well known, recommends the use of vegetable and of mineral acids; and when wine has been an ordinary article of consumption, Rhenish, Moselle, or Bucellas wines. Yet we must affirm that the aggregate of unprejudiced experience is opposed to the doctrine implied by such recommendation. Rayer well remarks, that acids rarely produce the desired effect on the urine, and that when their exhibition is persevered in for any length of time, they derange the digestive function, the free exercise of which is so essential a point to preserve so long as the progress of the malady itself will permit. We believe that Sherry or Madeira, of good quality, is better adapted for these patients than the class of wines spoken of by Dr. Prout. But we agree to the full with our author in the statement, of which, in our minds, it is impossible to exaggerate the importance that "absence from care, the exhilirating air of the country and such occupations (in moderation) as are consistent with the patient's peculiar condition, will perhaps, *more than anything else*, contribute to the cure; particularly in those slighter and induced cases, in which the affection is not complicated with local injury." Upon the value and mode of exhibition of sedatives and tonics we find nothing new. To the use of the shower-bath, especially of sea-water, as recommended here, we are accustomed to offer no objection, provided at first the trial of this powerful agent be very cautiously conducted. Dr. Prout, admitting the deposition of the mixed phosphates to be rare as an idiopathic affection, adduces three alleged examples of the fact in the form of cases. But instead of proving the point they are designed to illustrate, so far as the meagerness of detail and want of precision in the narrative permit us to judge, the unnatural abundance of those salts was in all three induced by organic disease of the urinary passages. It is a melancholy fact that the genius of many of our medical writers apparently induces them to despise the task of case collecting; of this we are certain that, judging from results, it incapacitates too many among them for entering upon this fundamental part, without which all others are as mere dross, of the investigation of disease with a view to the establishment of truth.

Some observations upon the phenomena attending the transition of different diatheses to the phosphatic close the present division of the work. One of the first changes usually detected by our author during the conversion of the oxalic into the phosphatic diathesis, was the secretion of an excess of (carbonate of) lime; he observed too that as the quantity of lime becomes greater, the proportion of the oxalic acid is decreased, while that of the phosphoric is increased, until at length phosphate of

lime is deposited in nearly a pure state. The urine during these changes is frequently observed to let fall the triple phosphate also. The first circumstances denoting a change from the lithic to the phosphatic diathesis are the paleness and sometimes the increased quantity of the urine. There is also a great tendency to precipitation of pale amorphous sediments, generally with more or less of the phosphates. Though ascescent when voided, an irridescent pellicle of phosphates soon forms on the surface. On account of the rarity of the cystic oxide, Dr. Prout has had few opportunities of observing its transition into the phosphates.

We have now presented the reader with a tolerably full account of the contents of Dr. Prout's first book; the second hardly calls for equally complete notice, as it embraces topics both more familiar to practitioners and more familiarly treated by the author. Here are examined "mechanical diseases, comprehending the description and treatment of diseases arising from obvious lesions of the kidney and bladder; and particularly from the presence of concretions in those organs." The book is divided into seven chapters: three of these referring particularly to the growth and symptoms of calculi in the kidneys and bladder, and the modes of removing them, with remarks on the operations of lithotomy and lithotrity, we defer noticing for the present; of the others we shall limit ourselves to a very condensed abstract.

Dr. Prout commences his brief history of acute idiopathic nephritis with an affirmation that it is, at least in this country, a very rare disease. We have in another volume of this journal stated our reasons for not acceding to this notion, and find no motive in the pages before us for swerving from our then recorded impression. Dr. Prout may have "only seen two or three well-marked instances of the disease," and may not "be able to speak with *much precision* of more than one case," but these facts may be otherwise accounted for than by the presumed rarity of the occurrence. But admitting that such rarity exists, and that consequently little or nothing is generally known of the anatomical characters of nephritis, it surely was incumbent upon Dr. Prout accurately to describe the condition of the organ, when good fortune furnished him with the opportunity of doing so. And yet Dr. Prout can only speak with "much precision" of one of these cases. And what is the description given of this? A minute exposition of the condition of the arterial and venous vessels, of the colour, of the size, of the weight, of the consistence of the organ, of its fibrous, subfibrous, and mucous membranes, of the absence or presence of pus, of plastic lymph, or of gravel? No such thing; the pathological world are at once given the credit of perfect acquaintance with what but a moment before they are declared to have scarcely ever seen (and of which decidedly there is no good description in the English language); and instead of an account which might enlighten general ignorance, we find a simple assertion of the fact: we are simply told that the "kidneys were both much enlarged, and in a most intense state of inflammation throughout their whole substance." It is unnecessary to follow our author in his description of the disease, as confessedly it is not derived from experience, and falls consequently far short of the information already laid in different forms before the profession. We feel ourselves called upon however to notice some general statements, which to us appear to involve practical error. It is affirmed, for instance,

that "what may be and is usually called chronic nephritis, is most commonly connected with a gouty diathesis and the formation of lithic-acid gravel or concretions." On the contrary, the researches of M. Rayer seem to prove that chronic nephritis is frequently independent of such diathesis, and is, when so independent, most commonly, as already mentioned, productive of phosphatic gravel. Speaking of rheumatic nephritis, Dr. Prout states "that the nearest approaches to what he has considered this affection have occurred in some of the milder cases of anasarca, accompanied by serous urine and produced by exposure to cold. In such cases the anasarcous swellings are often tender to the touch, and shift about without reference to the laws of gravity, very like rheumatic œdema." Here our pathologist becomes a mere symptomatic nosologist, in forming species from supposed differences in symptoms. M. Rayer has formally admitted a rheumatic nephritis, but upon the more judicious ground of his having detected a difference in the anatomical characters of the disease, when developed in rheumatic subjects. But Dr. Prout thinks meanly of such evidence as this, and prefers indulging in hypothesis as follows: "even acute anasarca itself might not be unreasonably referred to the universal inflammation of the same tissues which are usually the seat of common rheumatism."

The author alludes to the disease described by Rayer under the name of pyelitis—inflammation of the mucous membrane of the pelvis and calices,—but seems inclined to deprive the French observer of the merit of originality, by affirming that the "existence of the disease has long been suspected or rather known;" an affirmation which, as we are satisfied, would not stand before an enquiry into the evidence of the fact. Nor are the symptoms correctly stated: "the affection is accompanied by increased discharge of mucus and epithelium," says Dr. Prout; but he should have added, to do justice to Rayer whom he avowedly follows, that pus is poured out in more or less abundance. The French observer in truth lays great stress upon the alleged fact, that in cases of lumbar tumour and coexisting purulent discharge with the urine, the disease is really that now referred to, and not, as has been habitually supposed, chronic suppurative inflammation of the renal substance itself.

Dr. Prout supplies, in an enquiry into the diagnostic value of "pains in the back," some useful information as to the mode of distinguishing the different conditions of the kidneys to which such pains may be owing. The cases put by him all imply that the kidney is really the *seat* of the disease; its *nature* therefore is the only point to be established: as far as they go, Dr. Prout's observations may be referred to as of very great practical value; they are unfortunately too long for extraction.

As respects the treatment of nephritis, the author recommends venesection, the warm bath, and the exhibition of active doses of calomel conjoined with opium, henbane, and colchicum, &c.; (in gouty subjects mustard poultices to the feet), and when the disease has begun to yield under these measures, blisters; emollient clysters with or without opium should be administered, and strict antiphlogistic regimen insisted on.

In proceeding to discuss the subject of vesical diseases, Dr. Prout promises not to infringe on the province of the surgeon, and indeed there is no reason why he should, for assuredly their general pathology, diagnosis, and medical treatment, would in themselves supply abundant ma-

terials for an *ex professo* treatise. In the account of chronic inflammation of the mucous membrane of the bladder, cystorrhœa or catarrhus vesicæ, we find little to detain us. The ropy muciform matter which is now almost universally recognized both at home and abroad to be in reality pus, modified in property by the action of alkalies developed in the bladder, is still described by Dr. Prout as consisting of mucus. At the very outset of the complaint he has ascertained that the urine is acid, but the mucus itself always neutral, if not alkaline. As the disease advances however, the urine becomes alkaline; it has now a strong ammoniacal smell, and, as is well known, effervesces with an acid; according to the author there is "almost always in this case an excess of the carbonate of potash or of soda present; which are derived from the serum of the blood exuded from the ulcerated inner surface of the bladder." Towards the close, however, he has remarked that while the urine grows scanty and very high coloured, it occasionally reassumes an acid reaction, and the mucus or pus gradually diminish or almost disappear. The importance, as regards the prognosis of this change to acidity will become extreme, should the fact now announced meet with general confirmation. "I do not remember," says Dr. Prout, "to have ever seen a person recover when the urine has rather suddenly become acid, in long protracted and severe affections of the bladder usually accompanied by alkaline urine. After death the urine, though alkaline in the bladder, is often found acid in the kidney, provided that organ be not diseased."

Dr. Prout's account of irritable bladder is in some measure novel. The disease may depend either on functional or organic disease of the urinary organs, or upon nervous affections of a remote and constitutional origin. He first considers those cases in which the complaint depends upon functional derangements of the kidneys, and well points out that here the irritability directly depends on unnatural properties of the urine. Whenever that fluid varies in composition from its normal state, it becomes a source of *feeling*, a state which never arises in health except from repletion of that organ. In this mode the bladder may become occasionally irritable in persons of every age, but in young and healthy persons such irritation is only temporary. Dr. Prout appears to regard dyspepsia as the cause of this derangement, and conceives that in those whose assimilation is permanently bad, the tissues of the bladder may ultimately become organically diseased.

A much more fertile and more serious source of irritability of the bladder is disease of the kidney, a fact commonly known. Dr. Prout describes at some length, and as type of a class, an affection of those organs which he appears to consider especially connected with such derangement of the vesical functions. Respecting the anatomical nature of this renal affection we are left completely in the dark, unless the statement that "some of the affections formerly described as connected with anæmotrophy are so nearly related to, or so imperceptibly graduate into those now to be described, that it becomes impossible to draw the line between them," be accepted as conveying available information. Of the symptomatic and other characteristics of the disease as here stated, the following abstract comprises the more important particulars. It is principally confined to early and middle life, connected with a composite cachexia "*strumous*," "*remote syphilitic*," and "*malignant*;" hence

often connected with the oxalic-acid diathesis and anæmiotrophy of the system, never with decided lithic-acid diathesis; it is usually very slow, almost imperceptible to the patient in its earlier progress. The urine is generally acid, of a pale greenish whey-like colour, opalescent, below 1020 in specific gravity, often serous, rarely bloody, sometimes deposits the phosphates when heated, rarely a grayish ash-coloured sediment of the lithate of ammonia on cooling, and becomes clear by standing. The desire to make water is frequent and urgent, the period varying from one to three hours, and the quantity from one to three ounces; micturition is attended with scalding pain along the urethra, particularly behind the scrotum. There is no mechanical obstruction to the discharge, and in the early stages perfect ease is enjoyed after micturition, until the patient is again called upon to empty the bladder. With the advance of the disease all the symptoms increase, the calls are more frequent and urgent, the unnatural properties of the urine become more marked, the patient grows weak, emaciated, and irritable, and more than ever under the influence of atmospheric changes; he complains of no pain in the loins, but if pressed will sometimes admit the existence of a dull aching sensation there; the pulse gradually becomes quick and feeble, sickness occurs after eating, the urine diminishes in quantity, the patient complains of nothing, becomes drowsy, and at length goes off in a comatose state. Occasionally, however, the fatal event is hastened by an accidental exposure to cold or otherwise. The affection occurs in females as well as in males, and is occasionally attended with an increased vascularity and tumefaction about the orifice of the urethra, which part appears to the patient the chief seat of her distress. As respects the condition of the organs, Dr. Prout presumes that the whole secreting apparatus of the kidney is more or less involved, and that the mucous membrane of these organs takes on a diseased action capable of being propagated to the rest of the mucous surface as far as the bladder, in which viscus it appears even to commence sometimes, the symptoms varying in intensity according as the fundus or neck are attacked. Dr. Prout can say but little about the nature of this lesion; it may, he fancies, "in common language be called inflammation, but to his mind it is rather a species of degeneration." We would willingly draw from this some definite inference as to the morbid changes in the disease described, but from such data any other than a hypothetical conclusion cannot by possibility be derived.

Having thus described what "may be considered as the fundamental character of organic affections of the bladder, originating in kidney disease;" the author briefly notices some of "those rarer complications," which are liable to produce an impression of the existence of calculus. Under this head, polypous excrescences, elongations of the mucous membrane, fungus hæmatodes, &c. are ranged. The former affections are said to be usually in themselves harmless, and to cause inconvenience by their mechanical effects alone, unless they be complicated with the cachexia spoken of; and it is further affirmed, that fungus hæmatodes is "a tumour rendered malignant by being complicated with that cachexia." The nature of the structure composing morbid growths appears, in Dr. Prout's estimation, to exercise no influence on the phenomena induced by their presence, a notion totally opposed to the doctrines of the sagest pathologists. Be this as it may, however, fungus hæmatodes of the blad-

der is well known to become frequently more or less encrusted with phosphate of lime, and under these circumstances to have not unfrequently deceived surgeons into the belief of the presence of an ordinary calculus. Upon the question of diagnosis in these cases, the observations of Dr. Prout will have thrown some light, should it prove as he alleges it to be, a constant fact that the red particles of the blood discharged in the earlier stages of "fungoid disease" have a remarkable and characteristic appearance, seeming to the eye larger than natural, so that when they have subsided to the bottom of the urine, they at first sight somewhat resemble grains of lithic-acid gravel, and like that substance may be distinctly seen, when the vessel is inclined on one side, to roll along the bottom.

In the irritable states of the bladder which are commonly considered to depend upon nervousness, Dr. Prout has discovered that the properties of the urine are decidedly changed. Allied to this subject is that of the changes, real or simulated, which take place in the urine and urinary organs of hysterical females. Dr. Prout is too experienced a practitioner not to have encountered many cases of the flagrant deception which this order of females delight to play off upon those around them; deceptions the extent, occasionally disgusting character, and pertinacity of which appear in some instances to be only intelligible by supposing them dictated by some low form of insanity. His cautions to the young practitioner called upon to investigate one of these most difficult cases are valuable.

The summing-up remarks upon the diagnosis of these different affections of the bladder are most practical and judicious; they are really clinical, and as such we very strongly recommend them to the study of our younger readers. The subject of treatment is also very ably and comprehensively handled, and we regret that the near exhaustion of our space obliges us to confine our particular notice to the management of the affection described by Dr. Prout himself. In the early stages "if there be anything like activity," cupping the loins or leeching the perinæum are advisable, and the citrate of ammonia with mild purgatives is to be given internally. When instead of activity there is quiescence, the *uva ursi*, the *lythrum salicaria*, the *pareira brava*, and even small doses of the chalybeates, as the *tinct. ferr. muriat.*, are sometimes useful. In the more advanced stages, when the urine has become decidedly *albuminous*, and with a tendency to alkalescence, the citrate of ammonia either alone or combined with the fluid extract of sarsaparilla should be administered. If the disease be quite passive, the *infusum diosmæ* with sarsaparilla or muriatic acid may be given; or the *tinct. benz. co.*, the infusion of wild carrot seed or of sassafras, prescribed in such weak doses as never to excite. An issue or seton may be applied over the kidneys: this is not however often advisable. In the last stages of the disease, sedatives are the practitioner's only refuge, and of these *hyoscyamus*, *muriate* or *meconate of morphia* and *conium* are the best.

We shall on a future occasion examine the contents of Dr. Prout's chapters on hæmaturia and incontinence of urine.

Our general estimate of Dr. Prout's treatise may be gathered from the analysis just concluded, and the strictures scattered through it. We acknowledge and have pride in bearing testimony to the high qualifications of our countryman in the branch of pathological enquiry based

ıpon chemical facts; we recognize the comprehensive sagacity of his spe-
ulations, and have respect for the patient zeal with which he has toiled
o erect upon these a stable system. But we fear the time for such sys-
ematizing has not yet come; and although all speculations on the subject
re seductive in themselves, and doubly so when emanating from an in-
ividual of Dr. Prout's eminent skill in the department of chemical phy-
iology, it cannot, we think, be denied that in the existing unformed and
acillating state of organic chemistry, they sin essentially in being estab-
ished on a most unsound basis. Nor can we avoid entertaining some
olicitude as to the results of their propagation, which to us appears
ikely to betray minds of inferior order into mere extravagances. For
hese, however, Dr. Prout is not fairly answerable; and should his doc-
rines—when the frail embryo science on which they are based, has
eached healthy maturity—be recognized as true, he must almost take
ank with those highest intelligences, whose energy has outrun the sci-
ntific apprehension of their times. But meanwhile Dr. Prout has neither
lone his doctrines, himself, nor his readers justice, in not explicitly stat-
ng the foundation for and manner of verifying (so far as he is ac-
uainted with these himself) his presumed results. The day has passed
ever to return, when the authority of name could at will supply the
place of demonstration.* Should Dr. Prout intend favouring the public
with further volumes, he would do well to ponder on these admirable
remarks of a natural philosopher, of an eminence not inferior to his own:
"Il ne suffit donc pas de dire qu'on a vu telle chose. Ce n'est rien dire,
si, en même temps, on n'indique comment on l'a vue; si on ne met pas
es lecteurs en état de juger de la manière dont les faits qu'on rapporte
ont été observés."†

There are some defects in the manner of treating the subject, which
can scarcely be passed over without comment. No illustrations are here
drawn from micrography and microchemistry, which from their recent
successful application have been shown to be indispensable in these
investigations. On the inefficient and sometimes, as we think, positively
erroneous notions on pathological anatomy put forward in the volume,
we have already animadverted. The classification of diseases into func-
tional and mechanical, in itself bad, is lost sight of totally in the exami-
nation of details. By what laxity of phraseology can simple idiopathic
nephritis be termed a mechanical affection, as it is here made to appear
from the manner in which it is classified? Will it be believed again, by
those who have not read the essay, that the severest and most advanced
forms of Bright's disease, of encephaloid infiltration and of total de-
struction of the renal tissues, actually figure therein as so many "func-
ional diseases"? There is, in truth, a tendency in the whole tenor of the
work to establish a symptomatic medicine, to dispossess lesions of their pre-
minence, and invest symptoms with the importance of causes. The very

* Dr. Prout excuses himself from the statement of proof by the "practical" character
f his book; this is the worst species of extenuation, as we think, which he could have
lopted. What is not proved may be admitted in a merely theoretical work, intended to
tercise the mind of closet speculators, because it can then do no direct and actual harm;
it to extend to practice modes of acting founded altogether upon undemonstrated fan-
es, may, and is likely to become mischievous positively and physically.

† Trembley, Histoire d'un Genre de Polype, &c.

title of the volume is an illustration of this; of "stomach diseases," except on the title-page, we in reality hear nothing: of certain forms of dyspepsia much is no doubt said, but of the organic causes and essence of these not a word is breathed.

But we have done with this most irksome duty of exposing what we believe to be errors of no mean importance. We can most unaffectedly assure Dr. Prout, that it would have been infinitely more grateful to our feelings to lavish unmixed praise; but this we could not do. Were we to lend our voice to the establishment of doctrines such as those we have just taken leave of, we should break the vows we made on assuming the critical chair, to suffer no dogma which, in our conscience and to the best of our judgment, we believed to be wrong, to pass unscathed through our hands.

ART. IV.

1. *Statistical Reports on the Sickness, Mortality, and Invaliding among the Troops in Western Africa, St. Helena, the Cape of Good Hope, and the Mauritius; prepared from the Records of the Army Medical Department and War-office Returns. Presented to both Houses of Parliament by Command of her Majesty.—London*, 1840. Folio.

2. *Traité des Maladies des Européens dans les Pays chauds, et spécialement au Sénégal; ou Essai Statistique Médical et Hygiènique sur le Sol, le Climat, et les Maladies de cette partie de l'Afrique.* Par J. P. F. THEVENOT, Chirurgien de première Classe de la Marine, &c. *Publié par Ordre de M. le Ministre de la Marine et des Colonies.—Paris*, 1840. 8vo, pp. 399.

Treatise on the Diseases of Europeans in Warm Countries, and especially in Senegal; or a Statistical, Medical, and Hygienic Essay on the Soil, the Climate, and the Diseases of that part of Africa. By J. P. F. THEVENOT, Surgeon of the First Class of the Navy, &c. *Published by Order of the Minister of the Navy and the Colonies.—Paris*, 1840.

3. *Practical Medico-Historical Account of the Western Coast of Africa; embracing a Topographical Description of its Shores, Rivers, and Settlements, with the Causes, Symptoms, and Treatment of the Fevers of Western Africa. A similar Account respecting the other Diseases which prevail there.* By JAMES BOYLE, M.C.S.L., Colonial Surgeon to Sierra Leone, Surgeon in the Royal Navy.—*London*, 1831. 8vo, pp. 423.

WE have pleasure in presenting our readers with another valuable Report from the pen of Major Tulloch. The volumes which we have associated with it relate to the climate and diseases of the district forming the subject of the first part of this Report, the western coast of Africa; and as they are both of them the works of medical men, they constitute, as might be expected, valuable supplements to the more strictly statistical work of the gallant Major.

The British settlements in Western Africa are Sierra Leone, Gambia, the Isles de Loss, Cape Coast Castle, and Accra. They are scattered over

a line of coast, which, from St. Mary's on the Gambia to Accra, is nearly 1600 miles in extent, and, consequently, presents considerable diversity of climate, soil, surface, and geological structure, but everywhere exhibits the same remarkable hostility to the European constitution. The coast is generally low; its elevation from Senegal to Sierra Leone, a distance of 100 miles, being only a few feet above the ocean; its rivers are sluggish and flooded during rains, when the mud they deposit and the moisture they supply give rise to an interminable wilderness of forest and brushwood, among which lies rotting the decayed vegetation of many centuries.

In the Sierra Leone command are comprised the stations of Sierra Leone, the Isles de Loss, and the Gambia. The peninsula of Sierra Leone occupies an intermediate position in our settlements along this coast, being about 500 miles to the south of the Gambia, and 1100 to the north of Accra, and comprehends a tract of land extending about eighteen miles from north to south, and twelve from east to west, consisting principally of a range of conical mountains from 2000 to 3000 feet in height, surrounded by a belt of level ground from one to five miles in breadth.

From the noxious agencies likely to be generated in such a tract as has been described as extending from Senegal to this colony, it is sheltered by the mountain ranges which form the boundary in that direction, while on the south it is washed by the Atlantic, and on the north by an estuary terminating in the Sierra Leone river; so that it seems protected by nature from all extraneous sources of disease, except such as may originate on the opposite side of the river, called the Bulam shore. There is nothing in the nature of the soil of the colony, as described by Mr. Boyle, which, according to received opinion, is calculated to render it prejudicial to health. He says, "in the direction of the mountains it is generally thin, arid, and pebbly, with occasional large granite rocks projecting; and in the lower parts it is ordinarily either light unproductive sand, or a superficial and equally barren stratum of dark loam." Free Town, the capital of the colony, is so built as to comprise nothing in its structure noxious to health. The streets are fifty or sixty feet in width; the principal houses have rooms of good dimensions, lofty, and of a large area; the kitchens and other domestic offices are detached from the residence, and their whole arrangement seems well fitted for ventilation and comfort. The inferior dwellings, called frame-houses, are mostly detached, with yards or small gardens, and so placed as to afford free ventilation. The Bulam shore opposite to it has a soil of a ferruginous clayey loam, apt to form marshes during the rainy season, owing to the surface being almost level; but this shore is distant about seven miles. Mangrove bushes, which formerly covered the banks of the river with a dense barrier, and were supposed to be a fertile source of disease, have been cleared to a considerable extent, without, however, effecting any manifest improvement in salubrity. The barracks for the troops are stated to be good, ample, and commodious; they are situated on the top of a conical hill, 400 feet high, and nearly in the centre of the elevated range which surrounds Free Town. It would appear then, that, independent of a hot and an excessively wet climate, the acknowledged sources of disease do not abound immediately at Sierra Leone. Neither

would they appear to do so at the Isles de Loss, for Crawford's island, where the troops for the defence of these isles are stationed, is a granite rock, elevated 250 feet above the level of the sea, and is said to be

"…. entirely exempt from that exuberance of vegetation which prevails on the main land, and in no part of it, nor in any of the other islands, are either pools or marshes to be found; so that if the insalubrity of the command arose from the combined influence of these agencies, it ought not to have extended to this station, unless we can suppose the miasma capable of being conveyed from the main land with undiminished virulence across several miles of ocean.

"The principal settlement on the Gambia is at the island of St. Mary's, about 500 miles N. W. of Sierra Leone. It lies in lat. 13° 25″ N., long. 16° 38″ W., is five miles in length and one in breadth, and consists merely of a sand-bank formed by the confluence of the tides at the mouth of the river. It is consequently, in many places, under high-water mark, which renders it during the rainy season one complete marsh. The soil is too light to be well fitted for agriculture, but where intersected by creeks, and aided by the alluvial deposits brought down by the river, gives birth to dense masses of mangroves, underwood, and every species of rank vegetation, which, particularly during the hot season, create most offensive effluvia throughout the whole island. The country along the banks of the river, to the distance of fifty or sixty miles from its mouth, is so low as to be nearly on a level with the water, is covered with impenetrable mangrove bushes, reeds, and brushwood, and at ebb-tide innumerable shoals of mud and masses of decayed vegetation are left exposed to the action of a tropical sun." (Tulloch's Western Africa, p. 4.)

The principal characteristic of the climate of the western coast of Africa, we are informed, is its extreme humidity. As an illustration of this, Major Tulloch furnishes us with a table of rain which fell at Sierra Leone during the months of June, July, and August, 1828; it amounted to the enormous quantity of 313·4 inches: but this was an unusually wet season; and we are presented with another table, according to which 144·54 inches fell during six months (from July to December) of 1819. The rain does not fall with any degree of uniformity throughout the year; for instance, whilst July has above forty-five and August above forty-six inches of rain, there are but five inches in November and six in January. The wet season, we are informed, extends at Sierra Leone and the Isles de Loss from May to November, and at the Gambia from June to September or October, and is always ushered in and carried off by tornados.

In note A. of the Appendix to this Report, Major Tulloch adduces evidence to show the moisture of the atmosphere of Western Africa from what may be called natural hygrometers, which appear to be of a very conclusive kind. He remarks very justly, that

"The moisture of the atmosphere depends in part on the quantity of rain which falls, and partly upon circumstances which retard evaporation. In the torrid zone the quantity of vapour contained in the air is much nearer the point of saturation than in the temperate zone; in consequence of which the evaporation is much less than might be supposed from the high temperature. Various instruments," he adds, "have been invented to measure the degree of moisture in the atmosphere, but, in practice, these instruments seem to indicate the amount of evaporation rather than the absolute moisture. In the absence of a more accurate standard, some idea may be formed of the extent to which the latter exists in tropical climates by its influence on inorganic bodies. For example: 1. The rapid oxygenation of iron, by which tinned vessels are

:d with rust, and iron fastenings, unless of considerable strength, duced to a state of powder; 2, the effect on common salt, which, fully excluded from the atmosphere, speedily dissolves; 3, the effect l paste, which soon lose their tenacious qualities—for articles of furhich the former has been used fall to pieces, and paper, though well mes deteriorated and unfit for use; 4, the destruction of the texture of cloth—for some kinds, particularly woollens, are soon covered and, unless frequently dried, become rotten; 5, the mouldiness of r, during the wet season, in some tropical climates, boots and shoes vered with mould in one night; 6, the rapid destruction of soft h, if left underground, will rot and fall to powder within a year; id putrefaction of animal and rapid fermentation of vegetable sub- ll these indications of extreme moisture are particularly observable ate of Western Africa during the rainy season." (Tulloch, Note A x to Report on Western Africa.)

perature, as stated in the table for 1820, published in the re- iefly remarkable, both at Sierra Leone and Gambia, for its ormity. In the former colony we find the annual range of the ter to be but 12°, the extremes being 87° and 75°; whilst at is shown to be but 14°, the extremes being 89° and 75°. A owever, of Major Tulloch's leads us to conclude that there e been a peculiarity in 1820 as to temperature; for he says th the exception of Sierra Leone, where the diurnal range of ometer rarely exceeds 10°, sudden transitions from heat to the general characteristic of the whole west coast of Africa, ly at the Gambia, where the thermometer has sometimes fallen 62° in the morning during October, November, and December, risen to 80° in the course of a few hours."

rrison of Sierra Leone has consisted, since 1829, chiefly of ps, with a slight intermixture of white non-commissioned offi- atality of the climate to the European constitution having been ne so clearly demonstrated as to induce the government to alter e of garrisoning the colony. For, though the mortality at Sierra s less enormous than at the Gambia, which, as Major Tulloch it, "proved the grave of almost every Englishman sent thither," too considerable to justify any government in retaining the such a price. The admissions at Sierra Leone were 2978, and leaths 483 per thousand of the strength annually; in other ery soldier was thrice under treatment, and nearly half of the shed every year. Some corrections of this estimate required cumstance of many of the deaths in 1825 and 1826 having e at the Gambia reduce the mortality to 350 per thousand, or more than a third of the mean strength.

ace, the Gambia, is, as might be expected from the noxious in- n operation there, the most pernicious to European life of our n this coast. The statement on this hand in the report is as , at the same time, as forcible as possible. A detachment of arrived in the colony in the latter end of May, 1825, just as commenced. Between that date and the 21st of September of year, there died of remittent fever seventy-four, of other dis- teen, and there remained alive at the end of four months e. Another detachment, consisting of ninety-one, at this pe- d towards the end of September, room being provided for them

in the barracks by the death of four-fifths of their comrades. Of the force thus raised to 112, there died between that time and the 21st of December, of fever sixty-one, and of other diseases twelve, forming a total of seventy-three, and there remained alive thirty-nine. Another body of 200 Europeans was sent out to recruit the force thus reduced; it arrived, as before, at the commencement of the rainy season. The deaths reported from the 21st of June to 21st of September, 1826, were, from fever eighty-five, and from other causes thirteen, forming a total of ninety-eight. Eighteen more of the force, reduced by the previous mortality to 108, perished between the 21st of September and the 21st of December. Thirty-three of the survivors, who were suffering under chronic affections of the liver, spleen, and other viscera, were removed to head-quarters, and in June of the following year all the white troops were withdrawn from the station.

The following is a summary of the mortality at the Gambia. Between the end of May, 1825, and December, 1826, a period of only nineteen months, 279 perished out of a force, of which the number on shore seldom exceeded 120, and was sometimes even as low as 40. We are informed that, during the whole of this dreadful mortality, a detachment of from forty to fifty black soldiers of the second West India regiment lost only one man, and had seldom any in hospital.

We have already mentioned the circumstances in the geological formation and nature of the soil of this station, which gave it a character for salubrity which, unfortunately, was by no means justified by experience. Of a detachment of 103 men which landed there in the early part of 1825, in eighteen months sixty-two were dead and twenty-one invalided to England, consequently there remained of the original force but twenty; and such of these as survived were withdrawn at the close of the year, scarcely any of them being fit for duty. Major Tulloch makes the following estimate of the relative mortality at the respective stations during the years 1825 and 1826. At the Gambia there died 500 per thousand of the force employed, at Sierra Leone 650, and at the Isle de Loss 600; so that this quarter, though possessing some advantage over the other stations, still appears to labour under the ban apparently directed against the whole territory set apart for the descendants of Ham, that of being uninhabitable by the "genus Iapeti."

Fever is, as would be supposed, the cause of this frightful mortality, and remittent fever is at once its most prevalent and fatal form. A few extracts from the tables will show this. At Sierra Leone, the admissions from fever during eighteen years were 2600, and the deaths from the same cause 756; whilst the total admissions from all diseases (fevers included) were 5489, and the total deaths but 890; or, to state the ratio of deaths to the mean strength, whilst this is 410·2 per thousand from fevers only, it is but 483 per thousand from all diseases, fevers included. Then again, whilst of these 2,600 cases of fever 948 are intermittents, of which eleven are fatal, and sixty-one are common continued, of which six are fatal; 1601 are cases of remittent fevers, from which a mortality of 739 results; or, to state the fatality proportionally, intermittents kill one in eighty-six of those attacked, common continued fever one in eight and a half, and remittents one in two.

The enormous mortality from remittents of course produces a doubt as

to the accuracy of the name given to a disease so fatal. Major Tulloch remarks that, except in two points which he mentions, there is little difference in the character of the disease from that which generally marks the course of yellow fever of the worst type in other colonies. These two points are, that ulcers, which are very common among soldiers who indulge in intemperance, seemed to act as a safeguard against remittent fever so long as they continued open and discharged freely, but whenever they ceased to do so that it assailed the patient, and generally proved fatal; and that, in 1837, it was observed that, in many of the worst cases, an exudation of blood took place from the tongue, gums, nose, and anus, and that, whenever leeches were applied, the tendency to hemorrhage was so great as to render it almost impossible to stop the effusion. With regard to the first point, we would remark that this prophylactic power of ulcers over tropical fevers has been noticed in other localities—the West Indies, for instance; and we cannot regard it as at all decisive of the character or correct designation of the fever presented. As respects the second point, we would observe that hemorrhage is one of the characteristics of yellow fever, as is shown in our review of Louis's account of the Gibraltar Epidemic; and, although it seems to have been more general and severe in the African disease, it should be observed that the African, however it is to be designated, seems to have been altogether a more malignant form of fever than that at Gibraltar. Major Tulloch remarks that the absence of all mention of black vomit in the reports between 1824 and 1829, when the disease raged in its most aggravated form along the whole coast for several months each year, may induce a doubt whether the disease was yellow fever or the endemical remittent of the country. In the first place, great accuracy in reports of symptoms could scarcely be expected under the circumstances in which medical officers would certainly be placed on the western coast of Africa; the surprise is, that there were any reports at all. Again, it was well ascertained during the Gibraltar epidemic of 1828 that yellow fever often exists, and is sometimes fatal, without black vomit.

Mr. Boyle, in his account of the fever at Freetown in 1829, leaves no doubt of *his* opinion of its nature, for he calls it the epidemic, Bulam, or *yellow* fever; and in his really interesting description of the progress of this fever through the town, black vomit is frequently mentioned among the symptoms, and yellowness of the skin is very generally noticed. We regret, but we are not surprised, that none of the writers favour us with autopsies, for we should have been glad to observe how far M. Louis's pathognomic distinction, the anæmious condition of the liver, was present or otherwise.

M. Thévenot seems to have no doubt regarding the nature of the fever we are considering; for he says,

"Yellow fever, which we may justly suppose to be endemic on the neighbouring shore, nevertheless does not pass certain bounds. *Almost permanent at Sierra Leone*, it is rare near the Senegal; and its having twice appeared on the arid sands of Goree cannot be ascribed to the ordinary circumstances of the soil and climate. The marshy countries which border on the Great Desert are the cradle of intermittent and remittent fevers, whilst yellow fever has its origin in the uncultivated marshes near the equator. Thus, the sands of Sahara, the action of the east wind, and the long continuance of dry weather, tend incessantly to diminish the action of the miasma exhaled from the soil, whilst long-

children, in 1826, 578, decrease 553; Maroons from Jamaica, landed in 1800, arrived 550 alive, including children, in 1826, 636, increase 86; or, in about thirty years, a population of 19,564 suffered a diminution of 7634. This diminution may have arisen in part from emigration, and there is reason to believe that some of the liberated Africans have been again kidnapped and reduced to slavery; but, after every allowance for these possible contingencies, the reporter considers the climate nearly as unfavorable to the civil as the military portion of the negro population.

We regard this evidence of the insalubrity of this portion of the western coast of Africa to the negro race as conclusive; but we find by the returns that very little of the mortality which has effected this diminution of population has arisen from fevers. The annual ratio of admissions from these diseases has been but 54, and the deaths but 2·4 per 1000 of mean strength; or, as is stated in the report, the attacks have been fewer, and the deaths have not materially exceeded the proportion among an equal number of white troops in the United Kingdom, or other temperate climates. Though fevers are much more frequent and fatal, it is added, among the whites than in the West Indies, the reverse is the case with the blacks; for of black troops there died annually from fevers per 1000, in the Windward and Leeward Command, 4·6; in Jamaica, 8·2; Honduras, 4·4; Bahamas, 5·6; and in Sierra Leone, 2·4; so that it seems established that the susceptibility of the negro to febrile agency increases when removed from his native land.

Continuing to trace the mortality on this coast, and the influence of fever in contributing to it, we proceed to the second portion of the report or that relating to Cape Coast Command. Cape Coast Castle is 1000 miles to the E. and S. of Sierra Leone; it is built on a rock fifty feet high jutting into the sea, which is described as having a salt pond about a mile distant, but no swamps or marshes in the vicinity, nor any river nearer than five or six miles. The climate, as to humidity resembles that of Sierra Leone and Gambia. In this Command, two thirds of the white troops died annually, and the deaths from fevers amounted to 382·6 per 1000 of the mean strength annually.

A report of sickness and mortality among the officers serving in Western Africa, though from circumstances less full and accurate than those relating to the privates, tends to show that from fevers this class has suffered nearly in the same proportion as the soldiers, or, where the advantage has appeared to be on their side, this has arisen from the opportunity enjoyed by most of the officers of returning home, when their constitutions had become impaired by attacks of this disease. With regard to diseases of the bowels, a subject to which we shall subsequently advert, officers, as in the West Indies, enjoyed a comparative exemption; thus, as Major Tulloch remarks, inducing the inference that they must have arisen from some other cause than climatorial influence.

In the Fourth Section Major Tulloch presents his deductions from the Reports on Western Africa, and, examining the different causes to which this fever, whether correctly designated as remittent or yellow, has been ascribed by different investigators, regards successively excessive moisture, high temperature, variations in atmospheric pressure, and

the prevalence of particular winds as unconnected with its origin. With regard to the effluvia of marshes he says:

"The hypothesis that this fever originates from the miasma of marshes in the immediate vicinity of the station, as elsewhere it has been supposed to do, is directly opposed to the fact of the Isles de Loss, Accra, and the peninsula of Sierra Leone itself, being so subject to it, though all are in a certain degree, remote from the operation of any such agency. If it be referred to similar exhalations wafted to the distance of several miles, how is its prevalence to be accounted for at Fernando Po, a mountainous region and bordering on a main land still more so, and where, so far as can be ascertained, no such agency is in operation? Instances of the disease having raged with the same violence on the rocky Isles de Loss and the sandy wastes of Senegal, as in those parts of the coast where vegetation is most dense, preclude the likelihood of it originating in a superabundance of that agency. In every description of situation along the coast has this scourge of Europeans been found to prevail. The low swampy Gambia, the barren Isles de Loss, the beautifully diversified features of Sierra Leone, the open and park-like territory around Accra, the low jungle-covered hills of Cape Coast Castle, and the rugged mountainous island of Fernando Po, however different in aspect, have all exhibited the same remarkable uniformity in giving birth to the disease." (1. Report, p. 26.)

This passage contains ample reasons for further research, by showing us that such familiar words as malaria or marshy miasma, if not hypothetical, are yet far from furnishing that absolute explanation of the origin of these and similar fevers which they have often been supposed to do. Still the general tenor of this report and information derived from other sources regarding the territory under consideration tend to show that this fever, though existing in various and diversified localities, is most prevalent and malignant where the character of the country and its vegetation would lead the believer in marshy effluvia to apprehend its assaults. In "the beautiful and diversified Sierra Leone," the deaths in two years were 650 per 1000 of the force employed; in "the barren Isles de Loss," 600 per 1000; but on "the low and swampy Gambia they were 1500 per 1000. "The sandy wastes of Senegal," it should be observed, too, are not without their marshes. "Remittent and intermittent fevers," says M. Thévenot, "dysentery, and nervous colic, these are the most formidable enemies of Europeans (in Senegal). The marshy soil of Senegal does not differ from that of Sologne, of Bresse, and of Aunis. The same causes produce the same effects. In both cases there is decomposition of organic substances, and poisoning of the air by marshy emanations, and the action of this air on men takes place in virtue of the same laws; but if the direct causes of the endemic maladies are in the two cases identically the same, it is not so with the subsidiary causes. There is at least such a difference in the intensity of their action, that patients receive from it an impression which often cannot be mistaken." (pp. 232-3).

We have no disposition to undervalue the opinion of Major Tulloch, formed, as is manifested by this and all his reports, from a survey at once minute and extensive of facts, and a careful deduction from them, and we think that he has shown that marshes, in the ordinary meaning of this term, are by no means essential to the production of the fevers in question, but he has not shown that they do not produce them, or that of all soils they are not the most calculated to produce them. When he quotes

situations exempt from marsh as engendering these fevers, it will still be urged that vegetable decomposition is there possible and even probable, under a tropical sun and a humid atmosphere, and that such situations differ in their effects from marshes only in degree, and that this difference in degree is shown in their respective effects as set forth in his own pages. It may be urged that this argument is hypothetical, there being no proof of the decomposition of organized matter in such situations except from their influence on the human frame, and that to reason thus is reasoning in a circle. We admit this, yet does the hypothesis derive much probability from the effect in almost all climates of manifest marsh, and we think it one very difficult to disprove.

Before transferring our attention from this pestilential coast to climates more congenial to the European constitution, we shall briefly sketch the mortality from other diseases besides fever, observing merely that the class of intermittents there displays its usual characteristic, that of producing much inefficiency among the troops, and but little mortality. We find the total admissions for eighteen years to have been 948, and the deaths but eleven, or 1 in 86.

From diseases of the lungs there have been admitted but 56 per 1000 of mean strength, of which only 4·9 per 1000 were fatal, being in both cases a smaller proportion than in the United Kingdom, where the admissions are 148 and the deaths 7·7. The most marked exemption, the reporter observes, in this class is from inflammation of the lungs, by which 8 per 1000 have been attacked annually, though the usual proportion in most other colonies is from 30 to 40 per 1000. Consumption has also been comparatively rare, there having been admitted during the 18 years comprehended in the reports but 7 cases, of which 3 were fatal. In reference to this we are supplied with the *consolatory* reflection that the limited duration of human life on this coast would not perhaps in some instances afford time for so lingering a disease to become fully developed.

From diseases of the liver, described under the terms acute and chronic inflammation and jaundice, there were 150 admissions and 11 deaths, the proportion of deaths to admissions being 1 in 14, and the admissions being 82 and the deaths 6 per 1000 of mean strength, this class of diseases having been nearly four times more prevalent and fatal among the white troops in this station than in any other command of which the statistical details have yet been investigated.

From diseases of the stomach and bowels, the total admissions in the Sierra Leone command were 929, and the deaths 76, being in the proportion of 1 in 12; whilst the annual ratio per 1000 of mean strength was 504 admissions and 41·3 deaths. The Sierra Leone commissioners being of opinion that the large proportion of salt rations had mainly contributed to this sickness and mortality, an alteration was made in this respect, when, though the admissions remained the same, the mortality from these diseases was reduced to one tenth of its former amount—certainly a surprising reduction.

These diseases are much more fatal on the Cape Coast command than at Sierra Leone, more than one fourth of the troops having perished from them. The deaths from dysentery were at the former station 135, from diarrhœa 4, forming a total of 139 from these diseases of an entire morta-

lity of 421, and being in the ratio of 220·6 of a mortality of 668·3 per 1000 of the mean strength.

We expected to observe some notice of diseases of the spleen in these reports from Western Africa, knowing from personal observation that they are very frequent sequels of the fevers both remittent and intermittent which prevail there. They are often, however, combined with diseases of the liver, which may account for their not appearing in the returns, but we have seen them uncomplicated in this country in patients who had suffered from fever on that coast.

We pass to the sickness and mortality among troops serving in the island of St. Helena.

This island is situated in the southern Atlantic, about 2000 miles from the American and 1200 from the African continent, between 15° and 16° of south latitude, and 5° and 6° of west longitude. It is about 10½ miles in length, 6¾ in breadth, and has a superficial extent of above 30,000 acres.

When viewed from a distance, it presents only a mass of abrupt rugged rocks, towering to the height of 2700 feet, apparently divested of vegetation, and in several parts broken into immense chasms. These rocks, however, are intersected by valleys of various extent, but all extremely contracted. At the extreme of one of these valleys, on the north-west or leeward side of the island, is James Town, the capital. Near this the country is wild and rugged, and presents but little vegetation; but in proportion to its distance from the sea it becomes less so, and the luxuriance of vegetation increases; most of the uplands are covered with verdure, and at an elevation of nearly 1700 feet are two level tracts, called Longwood and Francis Plains, well-wooded, and affording good pasture. Various small streams intersect the island, but there is no marshy ground.

Though within 15 degrees of the line, the climate is not unhealthy, nor is it found debilitating to Europeans. The south-east trade-wind affords a steady breeze, which in these latitudes is rarely disturbed by storms and gales, and brings with it a canopy of clouds sufficient to afford shelter from the vertical rays of the sun. The temperature varies considerably according to the nature of the locality and different degrees of elevation. In James Valley, for instance, where the principal part of the troops are quartered, the thermometer during the summer months sometimes rises as high as 85°, and is generally about 80°; but at Plantation House, which enjoys an elevation of 1783 feet, the average at that season is much the same as in Great Britain. The average annual temperature in the former situation we find to be 75½°, and the annual range to be 15°. In the latter situation we find the mean annual temperature to be 69°, and the range to be 16¼. This is a very uniform temperature, but this equality is *said* to be counterbalanced by the circumstance that, owing to the nature of the localities, an individual may pass in a few minutes from the heat of the tropics to that of a temperate region. The difference between the high and low grounds with regard to moisture is still more remarkable. At James Town in the valley, in 1826, the number of rainy days during the year was 46, and 54 inches of rain fell; whilst at Plantation House, on the height, there were 178 rainy days, and 281·5 inches of rain fell.

Major Tulloch remarks that it could scarcely have been anticipated that, under the tropics, a situation could be found where the mortality among the white and black population did not, on the average of a long series of years, exceed that of their respective countries. He has shown, however, that such a situation has been found in St. Helena. Of a population amounting (exclusive of military) to an average of 3600, there died in 22 years 1627, being annually 74. According to this estimate the annual mortality has averaged one in 48½, even including the class termed strangers, many of whom landed in the last stage of disease; in the United Kingdom it averages about 1 in 47½ of the population; consequently St. Helena must be healthier than Britain.

According to the war-office returns, the deaths among the whole force in the island were 33 per 1000 annually, or, including 12 deaths at sea among invalids sent home for diseases contracted at St. Helena, 35 per 1000; but the medical returns, including troops of the line only, show the mortality in this force to have been but 25·4 per 1000 of the main strength annually. The admissions were fewer than among the most select force in the United Kingdom, in the proportion of 738 to 929; the diseases, however, were of a more dangerous character.

Above one third of the admissions and more than one half of the deaths arose from diseases of the stomach and bowels, which occurred in the proportion of 268, and were fatal in that of 13·9 per 1000 of mean strength annually. Of the admissions from this class nearly one half were from acute dysentery, and of the deaths three fourths arose from the same disease, which was fatal to one in 10⅓ of those attacked. The prevalence of bowel complaints generally and dysentery in particular is referred to the cause so often mentioned, salt rations.

Fevers are unfrequent and mild, the admissions of this class being but 78, and the deaths but 2·2 per 1000 of mean strength. Major Tulloch remarks, that there can be no better proof that this class of diseases may be comparatively rare, even within the tropics, than that the admissions have been fewer, in the proportion of 71 to 75, than among an equal force in the United Kingdom. Three fourths of the cases of this class are returned under the name of common continued fever.

As regards diseases of the lungs St. Helena is also remarkably healthy, the admissions from the whole of this class being but 61, and the deaths but 3·4 per 1000 annually; the admissions and deaths among the military being not half so high as in the United Kingdom or Mediterranean stations. The same feature is manifested in the fatal diseases of the population generally, in whom diseases of the lungs are fatal in the proportion of 3⅓ per 1000 annually. The exemption in this class is especially from inflammation of the lungs, which is the more remarkable, as a large proportion of the inhabitants are of the negro race, who in other climates manifest a great predisposition to this disease.

The admissions from liver disease have been 29 and the deaths 4 per 1000 annually of mean strength. To the total deaths which occur in the island, those from these diseases are in the proportion of 1 to 34½, whereas in England they are in the proportion of 1 to 78. This otherwise healthy climate, then, seems favorable to the production of diseases of the liver, resembling in this respect that of Western Africa. They occur, as the reporter remarks, even more frequently, and are of a graver

haracter than in the West Indies, though the temperature is lower and nore uniform, and though other diseases are more rare.

This seems the only real exception to the healthiness of this climate, so ingular in a tropical latitude; for the prevalence of dysentery and other ffections of the bowels does not appear attributable to climatorial nfluence.

The author considers the diseases of the Cape of Good Hope under wo divisions, those of Cape Town and its vicinity, called the Cape disricts, and those of the eastern frontier of the colony. We shall first give an abstract of his account of the former.

The Cape of Good Hope lies in lat. 34° 22′ south, long. 18° 24′ east. Cape Town is situated on a gravelly plain at the west side of Table Bay, having a gentle ascent to the foot of three barren precipitous mountains, which, stretching from the north-west to the north-east, form an amphitheatre, having its front to the bay. The soil is sandy, the under stratum rocky, and there is no marsh in the district. The maximum temperature as indicated by the thermometer is 86° and the minimum 56°, being a range of 30° throughout the year. This account, however, gives but an imperfect idea of the real temperature, which is very much augmented by the reflection of the sun's rays from the surface, composed principally of sandstone of the adjacent mountains already described. This reflection has raised a thermometer, which stood at 86° in the shade, to 136°. The average number of rainy days is 75, and the inches of rain are 41 throughout the year.

These are circumstances which, with the exception of the great heat, appear to promise health, and the promise is fulfilled. The admissions into hospital are numerous, being in the ratio of 991 per 1000 of mean strength; but two thirds of these are on account of diseases which seldom prove fatal, and the deaths are only in the proportion of 13·7 per 1000 of mean strength. The latter ratio, however, is exclusive of accidental deaths out of hospital. When these are added the mortality is found to be 15½ per 1000, or nearly the same as among dragoon guards and dragoons in the United Kingdom. The fevers appear to be mild, for though 88 per 1000 are admitted annually, of these 1 only in 45 is fatal, the proportion being little more than in the United Kingdom. By far the greater number of cases are returned under the head of common continued fever. Of diseases of the lungs there are 98 per 1000 admitted, of which 1 in 25 is fatal, or 3·9 per 1000 annually, a proportion much below the average of our colonies, those of the Mediterranean included. Diseases of the liver send 22 per 1000 to hospital, of which 1 in 20 is fatal. Of diseases of the stomach and bowels 126 per 1000 are admitted, and 3·1 per 1000 fatal. The ratio of admissions is lower than in British America, but with this difference, that dysentery is the prevailing form at the Cape, one half of the admissions arising from it, and whilst in the acute form it is fatal to one in 48½ of those attacked, a proportion pass into the chronic form, which is fatal to 1 in 4½. Diseases of the brain attack 10 per 1000 of the mean strength, and are fatal to 1 in 7·18 of those attacked, a degree of prevalence and severity nearly the same as in British America: a large proportion of the cases is said to arise from intemperance; but at the Cape, brain affection takes the form of delirium tremens in the proportion of only one tenth of the degree in which it pre-

vails in North America. On this fact Major Tulloch makes the remark, that " If the relative prevalence of delirium tremens throughout all the colonies is investigated, it will be found rare wherever wine is procurable at a moderate rate, compared with stations where spirits form the principal intoxicating medium, a circumstance which should lead to the latter being placed under more rigid regulation than the former." (Cape Report, p. 10.)

We wish we could within our limits transfer to our pages many of the very curious details respecting the soil and climate of the eastern frontier district, and the connexion between circumstances ordinarily supposed to be the sources of insalubrity, and a high degree of healthfulness. The following is among the most striking of these details:

"On the Keiskamma and Great Fish Rivers, the thermometer about noon has been frequently known to range for several weeks from 105° to 110° in the shade, and from 135° to 140° in the sun, and even during several months it has seldom been under 95° at that hour. This portion of the colony, however, is throughout the whole year subject to very sudden transitions of temperature; the thermometer in summer has been known to fall from 110° to 64° in the course of a few hours; and in winter, though it is often as low as the freezing point at night, it sometimes rises to 70° or 80° at midday. The degree of heat in summer is in a great degree regulated by the quantity of rain in the preceding season; if the fall has been plentiful, the summer is comparatively cool; if scanty, the reverse. Notwithstanding the extremely high temperature of this climate, its salubrity is probably unequalled in any portion of the globe. As a proof, we may say, that in the three adjoining districts of Somerset, Albany, and Witenhage, the deaths in 1833 did not amount to more than 327 in a population of 30,000, being only 1 in 91, which is much lower than has ever been observed in the very healthiest districts of Great Britain." (Cape Report, 12 b.)

The report from the Eastern Frontier district shows an exemption from disease which we regard as unparalleled. The admissions into hospital are to those of troops in the United Kingdom as 866 to 929, and the deaths as 9·8 to 14. Diseases of the lungs are to those at home as 2·4 to 7·7, and only half as high in Malta, where the same dryness of atmosphere exists as in this singularly favoured district. The mortality from fever is 1·2 per 1000, whilst at Cape Town it is 1·9, and in the United Kingdom 1·4. No part of the world seems to enjoy such an exemption from fever, especially those of the intermittent and remittent types; and this is the case, though several of the stations are situated close to the bank of a river, which, being either dry or stagnant during summer, might be expected, under a high temperature, to give rise to exhalations such as are supposed to induce febrile diseases in the vicinity of *fumiares*, or beds of mountain-torrents in Spain, Portugal, and the Ionian Islands. Diseases of the brain are fewer than at home, though the troops are often exposed to a temperature which, during summer, generally ranges from 95° to 100° in the shade at midday.

It is remarkable that this very salubrious climate appears to agree better with the European than the native constitution, though healthful to all. The mortality among the Hottentot troops is 1·1 per 1000 higher than among the whites, being 10·9 per 1000 annually. This excess is chiefly owing to the greater tendency to pulmonary disease and affections of the bowels among the Hottentots than the white troops. Febrile diseases are exceedingly rare and mild among them; so much

, that Major Tulloch remarks that it may be doubted whether any race men in any quarter would be found to exhibit so great an exemption om them. The deaths from these diseases has been but 7 per 1000 of ean strength annually.

This singularly healthy colony manifests its character equally in the se of officers and men; the mortality in the former class being 14 per 00 of the strength annually, including accidental deaths, in no way ttributable to the climate. The ratio discharged as unfit for service from mong the soldiers has amounted to 15 per 1000 of the force annually; eing lower than in any of the other stations which have as yet been assed under review. Of those pensioned for various diseases, the proortion of persons sent home on account of pulmonic affections is much ower than from any other colony, showing that the influence of this class f diseases is especially small at the Cape.

We pass to the sickness and mortality among the troops serving in the Mauritius.

This island is of an irregular oval shape, 36 miles in length, and from 8 to 27 in breadth, with a superficial extent of nearly half a million of cres. It is situated in the Indian ocean, about 500 miles to the eastward of Madagascar, and from 70 to 80 north-east of the island of Bourbon, nd lies in lat. 20° 9′ s., long. 57° 28′ E. The land rises rapidly from he coast to the interior, where it forms three chains of mountains from 000 to 2000 feet in height. The country, except towards the summit f the mountains, is generally well wooded. The whole line of coast is ell watered, from the foot of the mountains to the sea. The soil is in any parts exceedingly rich; but in the neighbourhood of Port Louis, nd generally in the immediate vicinity of the sea, there is but a scanty overing of light friable soil over a rocky surface of coralline formation.

The maximum temperature at Port Louis is 88°, the minimum 70°; the rmer occurring in January and February, the latter in July and August: r Mauritius, lying to the southward of the equator, the seasons are there eversed. It will be understood that great variety of temperature is exerienced according to the elevation attained; so that in the high regions f the interior, fires are often necessary. On an average of seven years, 9·30 inches of rain fell annually at Port Louis; but the humidity of calities is much influenced by the vicinity of mountains. It is observed at so far as regards temperature, rain, physical aspect, and diversity climate, this island presents a striking resemblance to Jamaica.

According to an estimate, the data of which Major Tulloch states to be ot very precise, the average annual mortality of the resident white opulation is 1 in 41 of both sexes, or rather less than in Malta. The ore precise statistics of Mr. Thomas show that in the adjacent island of ourbon, the mortality of the same class does not exceed 1 in 45, which nearly the same as in the United Kingdom; a circumstance calculated confirm the opinion of the general healthfulness of the Mauritius.

The sickness and mortality among the troops, however, are more conlerable than this account of the white resident population would lead to expect, there being admitted into hospital 1249, and dying (inding deaths from violent and accidental causes) 30½ per 1000 of mean ength annually. Of these admissions and deaths, a very small propor- n indeed, considering that the climate is a tropical one, arises from

fever; the admissions being 154 per 1000 of mean strength, and the deaths being but 1·7, or 1 in 89 of those attacked; this is the average for 19 years, and that for the six preceding years exceeds it only by a very insignificant fraction. During the period of 19 years, the admissions from remittent fever were but 6, and of these 1 only died. Major Tulloch remarks on this, that Jamaica, at the same distance from the equator as the Mauritius, with a temperature identically the same, a similar mountainous elevation in the interior, little difference as to soil or moisture, and the same amount of marshy ground, has lost during the same period from this disease 5114, of a garrison larger by only about one-half than that of the former island. This is one of the numerous nuts with which Major Tulloch seems to delight in perplexing medical men.

It is in other classes of disease that we must seek for the excess in the mortality of the Mauritius, compared with the Cape. From diseases of the lungs 5·6 per 1000 die annually, or twice as many as at the Cape. This excess arises from the comparative prevalence of consumption, to which more than one half of the mortality from this class is due, and with which 7·9 of the mean strength is attacked annually; a higher proportion than in the United Kingdom, the Mediterranean, or even America—a proof that a mild and agreeable temperature and an insular situation furnish no exception from this disease. Inflammatory affections of the lungs by no means abound; another evidence, in addition to the proofs produced by Louis, that pneumonia and consumption do not abound contemporaneously, but rather present a contrast in prevalence in any locality.

Diseases of the liver attack 82, and are fatal to 4 per 1000 annually. Affections of the stomach and bowels attack 275, and destroy 10·6 per 1000 annually; and of these attacks five eighths and of the deaths seven eighths arise from dysentery. The first attack is said to be seldom fatal, examinations having shown the tissue of the bowels injured by former attacks in those who have died. The chief mortality from it is in soldiers advanced in life, the younger men suffering only to one fourth the extent of those above the age of forty. Many of the cases are connected with hepatic derangement, but in many no such connexion can be traced. Officers and white residents suffer much less from dysentery and other bowel complaints than soldiers.

Epidemic cholera appeared in this colony, as is well known to those acquainted with the history of this disease, in 1819. The admissions were 268, the deaths 32, or one in 8⅓, a much lower proportion of mortality than occurred from the same cause in the United Kingdom, and other colonies. Major Tulloch says the disease exhibited nothing of a contagious character. He of course rejects the story of importation by the Topaze frigate.

Diseases of the brain gave rise to 41 admissions, and 2·7 deaths per 1000 of the mean strength annually. Five twelfths of these admissions and five-eighths of the deaths arise from *delirium tremens*, and the returns show that it has been progressively on the increase. The extent of intemperance at home and in our various colonies being traced by the test of this disease, it would appear to have attained its maximum in the Mauritius, *delirium tremens* being there the most prevalent, the Windward and Leeward Command in the West Indies alone competing with

ı this bad preeminence. The cheapness of arrack furnishes an ex- ıation of this unfortunate distinction. Whether there is anything :ial in the effect of this form of alcohol, we do not know.

ıfter noticing the diseases of a detachment, furnished by one of the iments stationed at Port Louis, to a group of islands called the Ley- lles, but which call for no observation here, the author gives the ails of the sickness and mortality in a corps of black pioneers in the uritius, which present some points of interest. This corps is composed ıegroes either born in the Mauritius, or brought from Madagascar and ambique. These men are subject to no harsh treatment, their rations abundant and suited to their tastes and constitutions, and they are cribed as a robust and athletic race. Yet do they die at the rate of 2 per 1000 annually, that is four times as rapidly as the inhabitants the Cape, or other healthy countries at the same age, and at least ice as rapidly as the white population of the Mauritius. The whole gro population of this island is decreasing so rapidly that in five years deaths here exceeded the births by upwards of 6000 in a population 50,000. It would seem that Mauritius and the West Indies are alike uited to the negro race: but it really does not appear on which of many circumstances included in that comprehensive word, climate, unsuitability depends. In countries so situated as Mauritius and the st Indies, it can scarcely depend on deficiency of temperature rela- ely to the negro constitution.

Diseases of the liver, stomach and bowels, and brain contribute consi- ably to this high rate of mortality, but diseases of the lungs are its atest source, 12·9 per 1000 dying annually in this way, or more than ce as many as of the white troops. The following are the proportions which negro troops die of diseases of the lungs at different stations: st Coast of Africa 6$\frac{3}{10}$ per 1000, Honduras 8$\frac{1}{10}$, Bahamas 9$\frac{7}{10}$, naica 10$\frac{3}{10}$, Mauritius 12$\frac{9}{10}$, Windward and Leeward Command $\frac{5}{10}$, Gibraltar 33$\frac{5}{10}$. In his native country the negro suffers from se diseases as British troops do at home, but after quitting it they ct him in a ratio varying in different colonies in a degree not explicable the manifest circumstances of the climate, but always more consider- y than in his native country. Consumption may be fairly estimated causing two thirds of the numerous deaths from diseases of the lungs negroes, yet natives of some tropical climates are so little prone to the ease, that among 74,850 native troops serving in the Madras Presi- ıcy, the deaths by every description of disease of the lungs did not, the average of five years, exceed 1 per 1000 of the strength annually. The deaths among the black troops from liver disease, is rather in a ısiderable ratio, that of 5·7 per 1000 of the strength. As the negro s not suffer to any extent from this disease, either on his native coast in the West Indies, the reporter regards it as an evidence of some uliar but unexplained tendency in the climate. May it not be re- ded as part of a more general but equally unexplained tendency in whole climate of the eastern hemisphere to generate disease of the r?

We again take leave of Major Tulloch, and with an increase of our timents of respect. The additions which he has made to our know- ge of climates are alike honorable to his talent as a *redacteur*, and

to that department of the public service whose very precise, voluminous, and long-accumulated treasures he has turned to such valuable account. We yield to no medical writer in *esprit de corps;* our "order" shall ever find us the unflinching advocates of their rights and privileges; but we do not regard ourselves as violating this sacred principle when we rejoice that the valuable treasures of the Army Medical Board Office were made over to a lay brother to be arranged in a form suitable to the compendious taste of the public. From experience of the manner in which questions of etiology have hitherto been treated by members of our body, we cannot help apprehending that the same amount of materials intrusted to more orthodox hands might have led to more of speculation and disputation, and less of practical usefulness than in those of the gallant Major. He has carefully eschewed hypothesis, and consequently avoided ground of controversy; but in precise practical facts relative to the climates treated of, his reports are unmatched by anything in our own, or, we believe, any other language. Some deductions towards the establishment of general facts or principles he could not avoid making; but those he produces seem rather calculated to lead us to these very desirable objects by showing the doubtful foundations of some of our present opinions, and thus clearing the ground for further investigation, than destined to form part of novel theories to be engendered by the writer. He manifests a greater disposition to show us that our present structures rest on frail ground than to become himself an architect.

The work of M. Thévenot contains a great mass of very precise statistical information regarding the French colonies in Western Africa, and had we considered them as likely to interest the British reader, our extracts from it should have been more abundant. As it is, we have availed ourselves of its contents chiefly to illustrate certain points in the corresponding parts of the reports. It is a book highly creditable to the writer, and it well merits the distinction conferred upon it, that of being published by order of the French Minister of Marine and Colonies.

Mr. Boyle's book is evidently the work of an experienced tropical practitioner. It is more strictly practical and less statistical than the other two works, but in its way one of great value. His descriptions of the diseases of Africa, particularly of the fevers, are excellent, and his practical suggestions bear all the stamp of observation and experience. We can conscientiously recommend his book as a safe tropical guide, and especially to the visitor of the Western Coast of Africa.

Art. V.

Medical and Physiological Commentaries. By Martyn Paine, M.D., A.M. —*New York*, 1840. Two vols. 8vo, pp. 1531.

Although we do not subscribe to the ancient proposition in all its generality, that "a great book is a great evil," there are certainly many instances in which it is true; and we are sorry to say that we are obliged to consider the work before us as one of the number. The multiplicity of subjects with which the mind of a medical man, who attempts to keep pace with the progress of his science, is constantly occupied, renders it desirable that ideas which are intended to be of practical utility to him

ould be submitted to his grasp in the smallest compass and most de-ed form, of which they are capable. For their merits in these respects formerly bestowed high praise on Dr Holland's Medical Notes and flections; a work to which the one now before us bears some degree resemblance, being a collection of essays upon disconnected subjects, result of the author's reading and experience. But here the resem-nce stops. We compared Dr. Holland's short but lucid reviews to etches drawn by the hand of a master; and, though mere outlines, only embodying just and vivid conceptions, but evincing more power l even more learning than many an elaborate picture on which all minor resources of art have been expended. To the latter part of s comparison Dr. Paine's work exactly answers. Of the degree of boration our readers may judge, when we say that Dr. Holland's gle volume of six hundred pages contains thirty-four essays, whilst in . Paine's bulky tomes we find but nine; and yet we hesitate not in erting that any one of Dr. Holland's sketches will convey a greater mber of clear ideas to a reader of ordinary capacity than the most hly-wrought of Dr. Paine's finished compositions. The reason is ply this,—that the latter contains a mass of materials, giving evi-ice, indeed, of great learning in the collection of them, but displaying uncommon want of sagacity in their arrangement; so that the picture follow out our metaphor) is so perplexed by the fullness and dis-ntedness of its details, that it leaves no distinct impression on the nd.

A perusal of Dr. Paine's work has impressed us with a great respect the author's industry and zeal; but we fancy that his mind must be icient in one qualification, which is rather important to any one who s up as a philosopher, (a title of which we should judge, by the head-of four of his essays, that the writer is rather ambitious,) namely, capability of perceiving accurately the relations of ideas. This notion have formed from the multitude of instances of loose and incoherent soning, of contradictory statements, and of misinterpretations of the nions of others, which we have detected in his treatises. To bring a gle objection against the assertion of another—whether that assertion the statement of a fact or the enunciation of a principle—seems in mind quite sufficient to overturn it. To show that a certain law is capable of explaining all the phenomena of a given action appears be regarded by him as quite sufficiently proving that the law had no uring on the action. Now, any one who has given the slightest atten-n to the science of life—Biology—must be aware how little any phe-mena exhibited by living beings can be attributed to the undisturbed eration of any single law; and how constantly the physiologist and hologist are obliged to make allowance for circumstances which inter-e with the operation of the simplest and best understood principles. t, it may be asked, does not the author lay himself open to similar acks? Are his doctrines more securely defended than those of others? his own estimation they certainly are; but we must take leave to dif-from him. The foundation of his system is the attribution of every nomenon which cannot be *fully* explained in any other way, to the livided operation of the "vital forces;" a sort of "independent com-ny," of which the "vital principle" is the commander. Any attempt

to penetrate their ranks is useless; their shields form an impassable barrier, and their spears defy the invader. But ancient armour is of little avail against the resources of modern warfare; a few cannon-balls sweep the supports of these old-fashioned soldiers from under them, and the invaders boldly make their way into the guarded spot. To speak without metaphor, we consider that any attempt to check the course of enquiry, by thus attributing all unexplainable or unexplained phenomena to the immediate influence of that mysterious agent, the "vital force," is a decided retrogression in physiological science. For there can be no doubt that many of these phenomena are, to say the least, partly explicable on physical principles; and that those which result from the vital properties of organized bodies still take place under the government of fixed laws, which it is the legitimate province of the philosopher to investigate.

It seems to us as if Dr. Paine's mind—possessing, as it evidently does, much power, but, unfortunately, misdirected in the application of it—would have been much benefited by that training in the school of physical science, which would have given him habits of more correct reasoning and a clearer perception of the bearing of general principles. The latter with some persons is intuitive, by others it is acquired: from whatever source it arises it is one of the chief characteristics of a truly philosophic mind. We cannot better illustrate the deficiencies and errors we have laid to Dr. Paine's charge than by referring briefly to his "Appendix on the Microscope." (vol. i. p. 699.) The extended use of this instrument "threatens," in his opinion, "a subversion of physiological science." He forgets that, faithfully employed, it can only disclose *facts*. But facts are stubborn things. He quotes a number of instances in which microscopic observers have differed,—as, for example, the size and form of the globules of the blood—the structure of the nervous tubules; but he does not tell us how completely, with instruments much improved during the last few years, and with increased knowledge of the circumstances which can affect the accuracy of the observation, all good microscopists are now agreed on these as upon a multitude of other interesting and important questions. "Finally," he concludes, "our microscopical philosophers would have it, that all nature may be ultimately resolved into microscopical *insects* (!) The very rocks are composed of them—nay, the earth itself..... *On weighing one of their shells,*" he afterwards tells us, "it was found to be the 187,000,000th part of a grain;" a most unfair distortion of an *estimate* founded upon measurement of the size of these curious bodies, and the known weight of collections of them containing a number which may be calculated by their size. In upholding this ridiculous doctrine, he tells us that "Professor Hitchcock is alone in his glory in the western hemisphere." We are assured, however, on the authority of Dr. Paine, who does not mention having examined the objects for himself, that "the objects mistaken for fossil animalculæ are nothing but crystalline spiculæ of earthy or metallic substances." When Dr. Paine has taken the trouble to examine, as we and hundreds of others have done in this country, the living animalcules, and the beautifully-organized siliceous cases which accumulate as a kind of mud at the bottom of the water in which they breed, and has compared these with similar remains, which, preserved in the most complete perfection, constitute the Polierschiefer of Bilin, or the mealy sili-

ceous earth used as food by the Laplanders—he may perhaps regret that he has hazarded his credit upon so rash an assertion.

Dr. Paine's First Essay is devoted to the consideration of the subject of vitality; and in this, says our author, "we have endeavoured to show that the great question relating to the vital powers is in no respect a speculative one; and since all our other subjects revolve about it, we have made that article of unusual extent." It professes, indeed, to comprise a *full* discussion of the whole subject; but when we say that Dr. Prichard's luminous treatise is not once referred to, that Dr. Fletcher's chapters on the subject have received scarcely any attention, and that the lengthy discussion of Dr. Carpenter's views is based only on the extracts from his work which Dr. Paine found in our review of it, our readers will have an opportunity of judging whether it can be regarded as a *complete* one. He quite prevents Dr. Carpenter from complaining, however, of this kind of treatment, by the following preliminary apology. "We notice Dr. Carpenter's opinions, without the advantage of reading his work, on account of his high reputation and the encomiums of his able reviewer." We rather suspect, however, that Dr. Carpenter would have been very willing to forfeit the compliment, for the sake of having his views either fairly represented, or passed altogether without notice. On a subject of this abstract nature, where so much depends upon the particular sense in which terms are employed, and where passages taken altogether out of their connexion are so liable to be misinterpreted, a critic cannot be too careful to take an accurate survey of the whole of his author's argument, before running foul of any particular portion of it. This we have endeavoured to do in regard to Dr. Paine; and we must take the liberty of saying that if we do not give a fair representation of his views, it is because they are expressed in such vague and often contradictory phraseology, that we are not always certain of his meaning. We shall establish this claim to his indulgence, before going farther.

"Returning to our enquiry" says Dr. Paine (p. 12), "'what is life?' and to the consideration of its definition by Bichat, and the philosophers of his school, we consider the *functions* as being merely the result of peculiar forces operating upon organic matter, and that *life* virtually consists in the coexistence of these forces and that peculiar substratum. The forces are, to a certain extent, in a passive state, when not excited by their appropriate stimuli. But they are still the essence of life; and whilst they endure, whether in an active or seemingly passive condition, life is constituted." Now here, Dr. Paine evidently recognizes the existence of a "force" as something distinct from matter, on which it acts. In the previous paragraph, he speaks of "force" and "property" as synonymous; and we are thus enabled to understand the idea which *he* attaches to the following definition, contained in a subsequent part of his essay. "The foregoing considerations," he says (p. 19), "would lead us to regard life as a cause; and to define it as consisting of certain specific properties appertaining to organized matter, which are more or less capable of resisting the destructive agencies of inorganic matter, and the forces to which it is liable, and of protecting against them the matter in which they are inherent. . . . We thus," he adds with considerable self-

gratulation, "embrace the elements of a good definition; philosophical accuracy, brevity, and a peculiar universal characteristic."

We do not know when it has been our fortune to meet with a more complete jumble of ideas, than that which would be involved in the foregoing sentences, if interpreted according to the usual meaning of the words composing them. By philosophical writers on any department of science, the term *force* is only applied when an action of some kind is taking place; thus the force of gravity, that is, the attraction subsisting between the earth and the falling body, causes the clock-weight to descend; or, if checked, occasions a pressure against the impeding substance. We cannot ourselves conceive of a force as having an existence distinct from the matter which manifests it. The Creator, in giving origin to that which we term matter, by that very act created the forces by which different material bodies operate upon one another. For all forces result from the action or operation of the properties (thus differing essentially from the properties themselves) with which matter is endowed; some of these being common to all forms of matter, whilst others are restricted to certain kinds of it. Thus a magnet has the *property* of attracting iron; but no *force* exists, until a piece of that metal is brought within the sphere of its attraction. To speak of a "passive force" is therefore a contradiction in terms. What should we think of a person who should talk about a "passive force" existing in water, by which are effected the mighty operations of the steam-engine? Does any *force* exist until the water has been converted into steam by the *stimulus* of heat, and the steam by a still further addition of temperature caused to expand with violence? Assuredly not. Yet this is only carrying out Dr. Paine's doctrine in the mode he himself warrants, when he tells us (p. 10), that the Deity is *not* the immediate cause of the phenomena of nature, but that he has brought into existence a number of *forces* (which he compares in their operation on matter to the mind, and which must, therefore, be so many separate *entities*) which are the proximate causes of all phenomena. Though the advocates of the vital principle have clung to this doctrine, for the purpose of explaining more readily (as they imagined) the phenomena of life, we certainly never expected to see it revived in the nineteenth century, in its application to material operations in general, by a writer of Dr. Paine's pretensions, and in a country where novelty is so eagerly grasped at. We cannot help imagining that Dr. Paine mistook this hypothesis for a new one; but he will find it to be the one which was adopted in the very infancy of philosophy, by those who endeavoured to account for the phenomena of nature.

Having disposed of *forces*, we will turn for a short time to *properties*. And we would ask, what other notion of matter do we possess than that derived from those properties which either directly affect our senses, through the material changes they produce in our organs, or which, by the influence of different bodies on one another, give rise to forces that produce phenomena of which we in like manner take cognizance? Subtract from *our notion* of matter all those properties by which we characterize it, and what remains? Nothing. On the other hand, we may ask, *if it is possible* for the properties to be separated from the matter itself? Can they have a distinct existence? Can we conceive of the

uality of hardness, or whiteness, or smoothness, without some material ody to which we attribute it? Or, to put the question in another form, an we conceive of an adjective as having any force without a substantive, and is not the latter always understood when the former is used lone? We hope that few of our readers are so bewildered by speculative abstractions as to be unable to use their common sense in answerng these questions.

Having enlarged so much upon these preliminaries, a very brief statement of Dr. Paine's views on the subject of life will suffice, and this we shall give in his own words: "The more, therefore, we investigate the subject, the more are we satisfied that life consists in the integrity of the vital properties associated with organized matter; that the vital actions are only the results of life, or of the foregoing conditions; that the vital properties or forces are essentially distinct from organized matter itself; and that organized structure may exist entire without the properties of life, although the former is necessary to the existence of the latter." (p.29.) Moreover, at the commencement of the essay, we are given to understand that *vital principle* is employed only as "a collective term, referring to the universal cause of animal and vegetable life." Now it surpasses our comprehension to understand how that can be legitimately stated to be *a cause*, which implies the conjoint action of a number of causes. As far as we can understand Dr. Paine, all the vital properties, as we should term them, appertaining to organized matter, as, for instance, contractility and sensibility, are to be regarded as distinct existences, the combined operation of which produces the phenomena of life. Thus, we have not *one* vital principle (using that term in its ordinary sense) but many. Further, he tells us in one breath, that the "vital properties or forces are essentially distinct from organized matter itself," and that they can go away and leave this matter in a state of perfect integrity, whilst in the next he informs us that they cannot exist without it. What becomes of them, then, when they leave the organized body to shift for itself? If they have an existence so essentially distinct as to be capable of leaving it, it stands to reason that they must be capable of existing without it, yet Dr. Paine says they cannot. A writer who can so contradict himself scarcely needs to be exposed by us; and we should not think it worth while to waste more words upon this Essay, were it not that he has so strangely misunderstood and misrepresented our own views. As on this subject we formerly expressed our complete accordance with Dr. Carpenter's positions, whilst differing from him on other points, we shall in our present observations identify our own with them, more especially as Dr. Paine's criticisms are directed to both alike.

Dr. Paine accuses Dr. Carpenter of a want of consistency, in applying physical and chemical laws to the explanation of some of the phenomena exhibited by living beings, whilst he attributes others to the vital properties of their organisms. It will be hereafter seen that Dr. Paine, with most laudable zeal in defence of the principles he professes, defies the whole array of chemists to prove that *any* of the changes concerned in digestion, respiration, or the other functions of the living body, are in the least within their province. We cannot, therefore, be surprised at his accusation, but we apprehend that it will find few supporters. No physiologist who has thoroughly investigated his science can, we think, hesi-

tate in the belief, that there are many phenomena in which the law vitality are only remotely concerned, and in which the immediate o ration is dependent upon principles recognized elsewhere. To w extent we can carry such explanations is certainly a matter of specula only; but that they are applicable in general to the changes of comp tion taking place in the organism, we think that the recent progres organic chemistry (of which Dr. Paine seems but little aware) most tinctly intimates. Neither Dr. Carpenter nor ourselves ever advoca the doctrine that *organization* could be the result of any chemica physical laws; on the contrary, Dr. Carpenter most distinctly repudi any such idea, and shows that these laws can only operate in the paration of those *organizable materials*, which, until organized, do exhibit any vital properties. That the phenomena exhibited by liv beings are influenced by many concurrent causes, is a doctrine to wl no one in his senses can refuse his assent. Will Dr. Paine tell us the particles of their organisms are, as soon as they are brought u the laws of vitality, removed from the influence of gravitation? that heart forces the blood through the arteries according to any other l than those which would govern the motion of a fluid of similar con ence, when impelled by a pressure similarly applied by artificial mea Then why should we stop short, and say,—the operation of chemical l is suspended, whilst that of physical laws remains in force? In alr all sciences there are phenomena which general laws *appear* inadeq to explain; but the philosopher is not disposed to consider them as a gether beyond the pale of these laws, unless he can show that they governed by different principles; and he trusts to time and further kn ledge of the circumstances which modify their application for the solu of the difficulty. Who, for example, would attribute to a *meteorolog principle* the varied phenomena which at present baffle the philoso skill of the most profound enquirers to explain? Now we hold t upon the Newtonian axiom so universally admitted, which forbids construction of unnecessary hypotheses, it is the part of the philoso to refuse his assent to any new doctrine of *vital affinities*, or of the version of chemical affinities by vitality, or any other equally vague culation. It appears to us that, to entitle such a doctrine to assen would be necessary to prove that oxygen, hydrogen, carbon, and nitro and the other elements of organized structures, follow, in the living b laws of union so entirely different from those which control their i ganic combinations, that, when brought together under the same circ stances in respect to all those influences (for example, form, temperat state of combination, state of division, electric condition, &c.) which inorganic chemist recognizes as affecting the result, the influence of vit *directly* exercised upon one shall entirely change the product. In case, oxygen would cease to be oxygen, for it would lose those prope by which we know it, and upon which alone (as we just now endeavo to show in regard to matter in general) our notion of it is formed. Dr. Paine tells us (vol. ii, p. 120, *note*,) that the laws regulating affinities are "more clearly established than any in chemistry." would suggest to him to make an exposition of them the subject of next work, since nothing can give him a higher rank as a physiologist such a distinct statement of them as may rank with the Daltonian law

definite proportions. Until, however, the proof which we think we have a right to require is brought forward, we shall rest upon the fact, now fully established, that, by influences acting on chemical principles, one organic product (not an organized tissue) may be converted into another, as distinctly indicating that the elements of all these products are held together by no other than chemical affinities.

The doctrine which Dr. Carpenter has propounded respecting vital properties, and which is essentially the same as that upheld by Dr. Prichard, Dr. Fletcher, Mr. Roberton, and other able writers on the same side, may be concisely stated as follows: Certain forms of matter (especially oxygen, hydrogen, carbon, and nitrogen,) are endowed with properties which do not manifest themselves either in these elements when uncombined, or in those combinations of them which the chemist effects by ordinary means. But they do manifest themselves when they are united into those peculiar compounds which are known as *organic*, and when these compounds have been submitted to the process which is termed *organization*. It is possible that the first of these conditions may be imitated by the chemist, but the last can only be effected by a previously-existing organism. We assert, then, that the very act of organization causes the materials acted on to exhibit properties *quite distinct* from those ordinarily termed physical and chemical, which properties cannot be caused to manifest themselves in any other way than by the series of operations just described. We cannot see in what points this doctrine is open to objection. No one can say that the properties *do not* exist in a dormant state because they do not manifest themselves to him. The answer to such an assertion would be simply this: Put the matter in question into the conditions specified as necessary, and you will see the operation of the properties. And this is but following out the mode of reasoning which is constantly employed in physical science, for no properties of matter but those which directly affect our senses can be shown to exist in any form of it except by affording it certain conditions for its manifestation. For example, how do we know that oxygen is a supporter of combustion, except by trying it with a combustible substance? Or how do we know that magnetic properties may be made to show themselves in iron, until we have placed the metal in the necessary relations with a magnet? How, then, can we expect to find contractility or sensibility in any combination of oxygen, hydrogen, carbon, and nitrogen, until it has been converted into an organized tissue by a previously-existing organism? And have we any more right to say that "vital properties," or "vital forces," or a "vital principle," have been *superadded* in the last case, than to assert that "magnetic properties," or "magnetic forces," or a "magnetic principle," have been *superadded* in the former one?—a mode of expression which, if it is to mean anything like that which the words import, no enlightened physical philosopher would think himself justified in employing.

From this explanation it will appear that a large part of the criticism which Dr. Paine has lavished upon us for attempting to explain the phenomena of life upon physical and chemical principles is entirely misdirected; since, whatever difference of opinion there may be as to those changes of composition which are concerned in the formation of organizable materials and the products of secretion, there is none whatever

as to the fact of these materials, when organized, being endowed with properties entirely distinct from any which can be traced in inorganic matter. Whence these properties are derived is the point on which we disagree: Dr. Paine maintaining that they hover about with a kind of undefined existence, ready to enter organized tissues when ready for their reception; whilst we argue that they were as much present in the elements as any of their other properties, which only exhibit themselves in certain conditions, and that we have no more right to deny their presence there than we have to assert that a sleeping man does not possess a mind, because it does not show itself in his actions.

It would occupy by far too much of our time and space if we were to follow our author through all the weak points of his lengthy disquisition. But we would just remark that, as, according to all the rules of philosophising, the *onus probandi* rests upon those who maintain the existence of a "principle" which others declare to be unnecessary, it is for the advocates of a "vital principle," or of "vital properties distinct from organized tissues," to show that an organized body which has suffered death by the supposed departure of these but which *retains its entire organization*, can be subjected to the action of the ordinary vital stimuli, without exhibiting vital actions. We take the liberty of denying altogether the possibility of this; and assert that death can never take place without some important change in organization. Let our opponents prove the contrary. Further, we assert that in all cases in which an organized body, such as an egg or seed, exhibits a vital power of resisting the decomposing effect of external agents, that power is due to some amount of vital *action* going on in it; and it is for our opponents to prove that no such action is going on. For instance, when Hunter found that a fresh egg was longer in freezing than one whose vitality had been previously destroyed, we assert that the effect was due to the respiration which was taking place in the former, and which retarded its cooling; for, by another experiment of Hunter's (On Animals producing Heat, experiment xxvii.), we learn that, under the hen, the temperature of the living egg is two degrees higher than that of the addled one. We therefore feel no doubt that the difference in the power of resisting cold possessed by a living and a dead egg is to be accounted for on precisely the same principles as that which may be observed between an hybernating and a dead animal. The respiration is feeble; but it is in itself an important process, and indicates the continuance of other changes within the system. The power of resisting spontaneous decomposition may be accounted for on the same simple principles; and it is for those who seek for new ones to show that they are necessary. This Dr. Paine has by no means done; and we therefore now take leave of his exclusive system of vitalism until it shall come before us on some better foundation.

The vagueness of our author's views in reference to the "vital forces," and the complete inutility of them in practice, is well shown in the Second Essay on the Philosophy of Bloodletting. After quoting sundry opinions on the *modus operandi* of bloodletting as a therapeutic means, he gives us his own conclusion, that it acts by producing an "impression on the vital forces." Now we should like to know what is gained by this form of speech, for it is nothing more. We thought that at first Dr. Paine was intending to explain the nature of this impression; for he tells us

hat the loss of blood produces, as its first effect, a contraction of the blood-vessels, which is propagated by continuous sympathy towards the heart on one side and the capillaries on the other, so that the action of the one is modified and that of the latter restored to its normal condition. This doctrine, by the way, precisely corresponds with that of Dr. Macartney; and though Dr. Paine elsewhere ridicules his phraseology, he employs something so very like it in this instance that we can scarcely overlook the parallel. The "alarm felt by the extreme vessels and capillaries," according to Dr. Paine, must be tolerably like the "sense of danger" which Dr. Macartney attributes to them; and, so far as we can discern the general theory of the former through the cloud of words in which it is enveloped, it may be concisely stated in the following proposition set forth by the latter: "The intention of drawing blood from the system is to produce that kind of impression which is followed by a weaker action of the heart and a more contracted state of all the smaller arteries in the body, and by that means inflamed parts are included in the general condition which precludes the possibility of inflammation." (Macartney on Inflammation, p. 151.) But it would seem that Dr. Paine, like Dr. Macartney, really considers the general impression upon the vital powers as the immediate cause of the contraction of the vessels; for just after showing how this change is propagated by sympathy, he tells us that "a victory is obtained over the disease" (inflammation) even before the blood is expelled from them, so that we do not seem to have advanced anything; and are, after all, to rest satisfied with the "direct and efficient impression" of bloodletting "upon the *vires vitæ* of the capillary vessels," as an ultimate fact. Let those esteem it as such who think it of any use to them. And in this way, Dr. Paine endeavours to satisfy himself and his readers, in regard to the influence of particular forms of the remedy. "From the foregoing considerations, we infer that the peculiarities of leeching are owing to some specific impression exerted by this remedy upon the forces of life, which no other mode of abstracting blood can directly establish." Are we one whit the wiser after all this learned discussion than we were before?

Of the practical portion of this essay, a large part is devoted to a critical review of Dr. M. Hall's opinions on the subject, to which our author strenuously objects. To the general merit of these opinions, we have on a former occasion borne a willing testimony; and our appreciation of their value is but little affected by Dr. Paine's objections. It is well known to every practitioner that no principle of treatment is without its exceptions; and Dr. Hall is as ready as any one to admit that this is the case with regard to the rules which he lays down. Dr. Paine brings together all the evidence he can gather in favour of the position he adopts, that the lancet is not used with sufficient freedom; and he seems almost a stranger to those distressing *sequelæ* of excessive loss of blood, which few practitioners in this country do not frequently witness. Believing that, whatever may be Dr. Paine's qualifications as a philosopher, he is a man of correct and extensive observation (on subjects, at least, which his prejudices allow him to use his sight and his sense), we can only justify the very free and all but universal recourse which he advises to the use of the lancet, by supposing that his countrymen, young and

old,* have much more stamina than the original stock—beef-eating and plethoric as it has been usually accounted.

We are inclined to think that he especially needs enlightenment as to the prevailing constitution of the poor of our large cities; where deficient and unwholesome food, unmitigated exposure to a most impure atmosphere, extreme filth of the person, depravity and wretchedness of mind, and all the other evils of a squalid poverty which can scarcely exist in anything like the same extent or degree in any part of the United States, not only combine to depress the vital powers of those immediately affected, but, as Dr. Alison has recently well shown, exert a most unfavorable influence on the health of the whole community. In regard to the use of bleeding in fever, too, we scarcely think that Dr. Paine has given sufficient weight to the character of the epidemic. In places where fever is constantly prevalent, such as Edinburgh, the change of type manifests itself fully as much by the kind of treatment found to be most successful, as by the alteration of the symptoms; and a few months will often reverse the therapeutic system as completely as if a new disease had appeared.

The next essay, which seems to us still more vague in its object and tedious in its character, is on the "Humoral Pathology;" the resurrection of which from its state of oblivion seems to excite so much alarm in Dr. Paine's mind, that nothing but a manifesto of 300 closely-printed pages can relieve him of the weight of responsibility, which, as the champion of neglected truth, he believes to rest upon himself. The best attention which we have been able to give to this essay has failed to impress our minds with a clear idea of the author's opinions upon its subject. The expressions in which they are couched are often so contradictory (to our minds at least), that we know not which to adopt. Take for example the following consecutive sentences: (1.) "These considerations will show us that the blood is neither a primary cause of disease in the solids, in virtue of its own morbid condition, nor can it be an aggravating cause of disease when altered in its character by the morbid action of the solids. (2.) None will deny what is affirmed by M. Andral, that every morbid change in the action of the solids is probably followed by a change in the blood. (3.) Whilst we fully agree with him, that any *primary* alteration of the blood of a morbid nature must, with greater certainty, produce disease of the solids. The latter proposition is the basis of humoralism." (p. 646.) Now the first of these sentences appears to us so far contradictory of the third, that we can only get out of the difficulty by supposing it to be our author's meaning that no primary morbid alteration *can* take place in the blood—an opinion so completely opposed to all that sound physiology and pathology teach us, that we scarcely venture to attribute it to him. As far as we can understand him, he believes disease to consist in a deranged action of his independent vital powers or forces, to which all material changes are but secondary. As we have already attempted to show that these forces have no existence but in the actions of the material organism, depending upon the properties with which matter was at first endowed by its Creator, we

* The following is one of Dr. Paine's general conclusions: "Bloodletting is equally safe at all periods of life, is most indispensable in old age, though not less important in many diseases of infancy."

not here stop to discuss the unphilosophical character of this hypo-
In regard to the main question, we may briefly state our opinion :—
he *vital* properties of the blood (for with such we have formerly
it to be endowed) are, like its physical properties, capable of alte-
by various causes; that some of these causes may originate in
stem itself—as, for example, a disorder of the apparatus of san-
ition, of assimilation, of respiration, of secretion, whilst others are
al and act directly on the blood, especially through the medium of
spiratory organs and the nervous system; and that, when the
y of the blood has been changed, either primarily or secondarily,
teration may give rise to other morbific changes, and will especially
what is commonly termed the "constitutional state."

Second Volume opens with a long essay upon Animal Heat; in
the author, in pursuance of his favorite doctrines, endeavours to
this function from the grasp of the chemists, and to prove it to be
doubted offspring of pure vitality. What is his success, we shall
t to his readers to determine; for to go through his whole train of
ing on the subject would occupy too much of our space. This
less regret, as it appears to us extremely inconclusive. But of
neral results and of the mode in which he attains them, we shall
e a few specimens. "The only connexion which has been
ably shown," he observes in commencing, "of the forces of che-
with the process of respiration relates to the decomposition of
. From this, and from the necessity of respiration to animal tem-
re, in common with the other products of vital actions, has followed
duction that the generation of heat is a chemical process." (p. 4.)
ll not impute the mistake of calling the separation of the oxygen
atmosphere from the nitrogen *mixed* with it an act of *decomposition*
. Paine's ignorance of the meaning of chemical terms; but we
that a much closer connexion has been shown between the act of
ition and the laws of chemistry than he states. For not only is
n removed from the atmosphere, but carbonic acid is added by this
s; and to mention one only of the proofs that this exchange fol-
rdinary chemical laws, it takes place out of the body as well as in
ler the influence of purely physical conditions. Again, Dr. Paine
es that the production of animal (or rather organic) heat is not more
tely connected with respiration than are the other functions; slur-
ver the important experiments of Newport on the production of
n insects, in which this relation is shown to exist in a remarkable
, and neglecting altogether the results adduced by vegetable phy-
ists as to the constant relation between the development of heat, in
flowers which most prominently exhibit the phenomenon, and the
it of oxygen converted into carbonic acid. The case stands
thus: oxygen is introduced into the system from the atmosphere.
ne part of the system (*where*, for the general argument is of no
uence), it combines with carbon, set free in the ordinary processes
trition; and in this state it is thrown off in the gaseous form from
dy. Now will Dr. Paine tell us why, since we know that the
nation of carbon and oxygen elsewhere produces heat, it should
so here? And if it does—and if, with a few occasional *apparent*
ions to which the operation of all physiological laws is subject, the

general relation between the amount of heat generated and the amount of carbon united with oxygen is constant—we ask, why should we seek for any other efficient cause? But, when we apply the question a little more closely, it is seen that chemists have changed the position which they occupied some years ago; and, instead of maintaining that the union of the two elements takes place in the lungs, they consider it as diffused over the general system. So, says Dr. Paine, like a victorious general, "the chemists having abandoned this old entrenchment, we shall take possession and fortify it with the forces of life." But Dr. Paine seems to forget that it was not by *his* army, to use his own metaphor, that the chemists were compelled to yield; but by a division of the more enlightened among themselves, who saw that this new position would be a much less assailable one than the other.

Now what will our readers think is the doctrine for the defence of which Dr. Paine so cunningly seizes upon the lungs as his citadel? Neither more nor less than this—that animal heat is a secretion; and, like other secretions, a production of pure vitality, to the parentage of which chemistry has no claim whatever. Here again we must revert his nomenclature. Secretion, as physiologists have always been accustomed to use the term, implies a *separation* from the circulating fluid of certain ingredients which preexisted in it; these ingredients being often previously united into the peculiar substance which is characteristic of the secretion (in which case their *mere* separation is most evident); or being so united in the very process of separation. However this may be, *the elements* must have preexisted in the fluid; and it implies the idea of an absolute creation of new matter in this act, to entertain any other opinion. Now, does Dr. Paine mean that *heat* was preexisting in the blood in a state not manifest to the senses, and that it is only *separated* by the act of secretion to which he attributes it? If so, we see no essential difference between his hypothesis and that of Dr. Crawford; since this preexisting heat exactly corresponds with the *latent caloric* of the chemist, which only requires certain chemical changes to render it sensible. On this view, then, Dr. Paine's hypothesis is only a modification of a form of the chemical explanation which has long been familiar. For any other, a new definition of the term secretion must be given; and we see no reason why the vital laws should not as readily *produce* gold or silver, as *secrete* heat from that which did not previously contain it.

In speaking of the relation between respiration and animal heat, we do not wish to be understood as asserting that this is direct or immediate. We believe it only to operate through the nutritive processes, and the molecular changes which they involve. The *aggregate* of these processes throughout the body may be most readily ascertained by comparing arterial and venous blood; and that the *principal* difference between these consists in the presence of oxygen in the former and of carbonic acid in the latter, cannot, we think, now be denied by any one who impartially views the evidence, and gives due weight to the causes that have produced those contradictory results which induce Dr. Paine to look with an eye of scepticism upon the whole. It is not, then, the change of arterial into venous blood, but the processes which cause that change, which are the real source of the production of heat. In these processes there are probably many chemical changes involved, besides the combi-

ı of carbon and oxygen; and it is to the aggregate of all these ıst, however, being the chief) that we attribute the liberation of c which was previously latent. These processes may, in some ani- continue for some time without a fresh admission of oxygen into ngs; and in them, the dependence of animal heat upon respiration refore more remote than in others. But in insects, the feeble mo- of whose blood is counterbalanced by the admission of air into part of the system, the relation is extremely close. A stimulus ı excites the individual to activity also increases the number of its ations and its consumption of oxygen; and the temperature is raised, '. Newport has shown, exactly in the same proportion. Now, in ng to Mr. Newport's experiments, Dr. Paine entirely overlooks this and hazards the bold assumption, that "upon this simple fact (the rapid increase of temperature when the insect is excited), so e are the laws of nature—it will be safe to found a general induction the dependence of animal heat upon the forces of life," (p. 66,) s, we understand Dr. Paine from his other remarks to imply its : independence of all chemical laws. How this *general induction* a *single fact* is to be attained, it surpasses our comprehension to ver; it savours a little of the "go-a-head" principle, which phy- ;ists, of all men, should carefully eschew.

ı one point, the connexion of the nervous system with the production ımal heat, we have pleasure in stating that we think Dr. Paine's in the main correct and well expressed. We shall therefore pre- them in his own words.

'e have spoken of the probable controlling influence of the brain over the ation of animal heat, under particular circumstances. It may be more ılt to arrive at the extent of its influence in the natural state of the animal n. *Like other secreted products,* animal heat is, doubtless, primarily de- :nt on the organic powers. These are variously influenced by varying tions of the cerebral and ganglionic systems; and, of course, their actions he products of the secerning system, over which they preside, are affected orresponding manner, whilst they are also modified by the direct action reign agents. In the perfectly natural state, there is reason to suppose he brain may have but little connexion with the phenomena, but may be- powerfully instrumental in modifying the powers and actions and products e, when unusual conditions exist, or when unusual impressions are trans- d to the brain. We shall see that analogy, as supplied by the vegetable lom, affords presumptive evidence that the brain may have no active par- tion in the elaboration of heat, in the natural condition of the body, whilst nduction is strengthened by what is known of other secreted products in of the animated kingdoms. Still, in respect to the animal kingdom, the existence of the cerebral system, its remarkable properties and suscep- ies, and its intimate connexion with all parts of the organization, is, *primâ* conclusive that it determines important influences upon the *vires vitæ* hat its presence is indispensable to the integrity of every function. This een experimentally ascertained in reference to many; and that unusual or :n impressions that are not unnatural, as the operation of the passions, for ıce, may be extensively and profoundly propagated from the brain to organs. It has been fully demonstrated (?) that the natural condition of :cretions depends upon the integrity of the nervous connexion betwixt the ning organs and the brain; whilst it has been equally shown that the or- functions and all vascular action may be immediately and powerfully in- ced by impressions made upon the brain." (Vol. ii. p. 23.)

In Dr. Paine's opinion, then, as in ours, the acknowledged influence of the nervous system upon the production of animal heat is exercised through the medium of the organic functions; and the difference between us consists in this, that *he* regards it as one of those functions incapable of being explained by any other than vital laws—which, in the present state of our knowledge of those laws, is equivalent to saying that we know nothing at all about it; whilst *we* consider it as a *result* of those functions produced by the molecular changes which they involve—these changes being themselves governed by the ordinary laws of chemistry, as we have elsewhere endeavoured to show.

In the last section of this essay, we find some original experiments on the heat of the stems of trees, made for the purpose of confirming John Hunter's results, and thereby proving that the evolution of heat is an organic function, and not dependent on the nervous system. Of Dr. Paine's qualifications as an experimenter, or rather, we should say, as a recorder of experiments, our readers may judge, when we tell them that, in his first series made at the beginning of April, he gives us a table of the degrees at which the mercury stood in thermometers inserted in the stems of twenty trees, varying from 48° to 68°, whilst, to enable us to compare these with the external temperature, he only tells us that the range of the thermometer in the shade, during the six hours that the experiments lasted, was from 38° to 52°, and that the temperature of a dead tree, selected as the standard of comparison, was 45° at the close of the experiments. Of a similar series, made ten days later, when vegetation was consequently more active, we are, in like manner, only informed that the range of the external thermometer, during five hours, was from 40° to 65°; the temperature of two dead trees at the end of the observation, 60°; and the temperature of the earth six inches below the surface 47° at the same period. Let our readers compare these, if they think it worth while, with the precisely-narrated experiments of Hunter, in which a distinct standard of comparison is given for every tree experimented on. In these it was often observed that the temperature of the stem was *lower* than that of the atmosphere, a result which Dr. Paine does not seem to have met with. This shows that there must be some cause interfering with the manifestation of the *proper heat* of the parts surrounding the bulb; and this cause we believe to be partly the slow conducting power of the wood in a transverse direction; and partly the important circumstance altogether overlooked by Dr. Paine, that the sap which ascends the stem is derived from a considerable depth of soil, (about four feet is stated as the average position of the spongioles of a large tree), and that its temperature is, therefore, no indication of the proper heat of the stem. Moreover, the vital process taking place in the interior of the stem are so very slight (the formation of new wood taking place just beneath the bark, and the old wood serving but as the conduit for the ascending sap) that this circumstance of itself indicates the fallacy of such experiments. We recommend to the attention of Dr. Paine, and of such of our readers as feel interested in the subject, the experiments of Dutrochet, made with the thermo-multiplier, in which the sources of fallacy we have indicated were carefully guarded against, and which are by far the most satisfactory to our minds of any of those which

profess to prove that an evolution of temperature takes place during the *ordinary process of nutrition* in plants.*

The next essay professes to give the "Philosophy of Digestion;" by which we should understand a scientific account of the process, explaining it by the operation of known general laws of some description or other. This, however, is far from being the case. The author's object is to explode altogether the chemical theory; and to show that, once conveyed into the stomach, food is only acted on by the vital forces. Finding that our present knowledge of chemistry is insufficient to account fully for the reduction of the food, and its conversion into the organizable product albumen, he jumps at once to the conclusion that the gastric juice acts as a solvent in virtue only of the "vital forces" which are mixed up with it; and, as the separation of the fluid from the body does not prevent its retaining these, for a certain time, the solution which may be artificially produced by the employment of it is to be explained in the same manner. The experiments of Müller, Schwann, Eberle, and others, are very summarily disposed of; Dr. Paine proceeding upon the principle that if our present very imperfect knowledge of the principles of organic chemistry (a science confessed by all to be in its infancy) will not explain everything, those principles have *no share* in the result. The following is the commencement of his creed. "The vitalist maintains that the gastric juice is a substance *sui generis*, endowed with vital powers; that it can only be generated by a living stomach, and that it is liable to modifications which may arise in the vital condition of that organ; that it *cannot*, therefore, *be remotely imitated by art*, any more than art can manufacture semen or the cerebral substance; that its properties, like those of every other vital part, are in *perfect opposition* to the forces and agencies of chemistry," &c. &c. (p. 122). These "transcendental vitalists," as one of our contemporaries denominates them, seem quite to forget that there is no proof whatever that the gastric juice has any concern in *vitalizing* the alimentary substances submitted to its action. Its office seems to be to reduce them to the form of albumen, which is the chief constituent of the chyle as first absorbed. Now it is to the *fibrine*, which is *subsequently formed*, that the coagulation and other vital properties which the chyle possesses are due; and this substance, which Dr. Paine speaks of as existing in the stomach, is, we are tolerably confident, never found there, except in the state of half digested food. In fact, the gastric juice has for its office to reduce the aliment, which is to be presented to the absorbing origins of the lacteals, to the same state in relation to the animal, as the watery fluid, holding various substances in solution, conveyed by the soil to the roots of a plant, has to the vegetable which it sustains. There is no more reason to regard the one as a *vitalized* product than the other. The only indication of vitality in albumen is the globular form of its particles; and it is yet to be shown that this is not as characteristic of albumen as any crystalline form is of a particular salt. The organization and *consequent* vitalization of this substance commences as soon as, being taken into the vessels, it is admitted into the living system, to which, so long as it remains in the stomach, it is really external.

As connected with this subject, though by what train of thought is

* See B. and F. M. R. vol. ix. p. 546, and Ann. des Sci. Nat. N. S. Botan. tom. xii.

not very evident to us, we have an essay upon spontaneous generation, in which the author strongly opposes this hypothesis in all its forms. We are as convinced as he is, that no combination of inorganic elements can ever *spontaneously* produce a living organism, the previous agency of another living organized structure being a necessary condition of *their* organization. But we may take this opportunity of stating that our belief in the general proposition, which we long ago put forth in an interrogative form (vol. IV. p. 20), that "plants or animals of a high degree of organization are capable of producing from various parts of their tissues beings corresponding to those of the inferior orders of their kingdoms," has recently been much strengthened by additional evidence.

We have next a learned review of the Theories of Inflammation; the object of which is evidently to prove that they are all wrong; but it is not so evident what the author upholds as right, save that this disorder consists in disorder of the "vital forces." As far as we can understand his views, he regards inflammation as consisting in the exaltation of these forces. "The capillary vessels, then, performing at all times in health the actions of contraction and dilatation through vital forces, there can be no reason assigned why, when those forces are exalted, the contractions and dilatations should not be increased in a corresponding manner." (p. 156.) Having so recently discussed this subject in full, we shall not now enter upon it again; but we may remark that, by his almost exclusive attention to the state of the vessels, Dr. Paine seems to us to wander from the main point of enquiry, which concerns the relation of the blood and the tissues supplied. But, according to our author, although blood may be altered from its healthy condition, it never can be, strictly speaking, diseased; that is, it must always bear a *constant* relation to the solid tissues. This he endeavours to establish in his essay on Humoral Pathology, by what appears to us a most loose and inconsistent train of argument; to which it seems to us quite sufficient to reply that, in all cases of local disease, the blood must bear a *different* relation to the healthy and diseased solids respectively.

The next essay, professing to give the "Philosophy of Venous Congestion" is, we think, altogether the most valuable in the work; though we should be able to give it higher commendation, if it were compressed from *four hundred* pages to forty. It is marked by all the faults of the author's style of reasoning and writing; excessive dogmatism and exclusiveness, hasty inferences, vague statements, looseness of expression, and unnecessary amplification. But, withal, it contains some interesting views, which we deem worth prosecuting, although scarcely so well established by the author's arguments as he seems to consider them. By the term "venous congestion," Dr. Paine implies "a morbid fullness of the veins, arising from disease of their parietes; having no reference to the state of the arteries, and being wholly different from that fullness of the veins which is consequent upon a preternatural quantity of blood thrown upon them by the arteries under various influences. Nor do we recognize, in the last, as appertaining to this affection, that turgescence of the veins which is produced by obstructing causes." To establish the existence of such a condition as a primary morbid alteration, is the object of the present essay; and we think that the author is quite right in asserting that, in many instances, the enlargement of the veins and the

ion of the blood in them cannot be accounted for on the ordinary s, and that they are due to some change in the veins themselves. e further disposed to admit the probability that this change is of immatory character, though we cannot perceive any distinct proof

But we must protest against such a system of dogmatising as hich induces Dr. Paine to declare that, because *all* cases of venous ence cannot be explained by obstruction of the trunks or stagna- the lungs, these causes never operate; for this in fact is his position. He refers to Goodwin, Bichat, Andral, Louis, Kellie, ais, and others, as having "clearly shown, either by experiment natomical facts, that no pulmonary or thoracic affection can con- an obstruction to the return of blood to the head." (p. 249.) et it be remarked that in this sentence he states the *impossibility* occurrence; and yet in the next, which he quotes from Broussais port of his assertion, the *rarity* of it is alone intimated. "He re- d that the difficulty of the disgorgement of the superior vena cava lf seldom produces cerebral symptoms." In the next page we are at "if in dyspnœa the face be sometimes injected, or the jugulars it arises frum compression of the superficial veins by the respira- uscles, or, in the former case, a determination of blood upon the l system." Will this explanation account for that reflux of blood very contraction of the heart, which causes the *venous pulse* so bserved in cases of obstruction in the pulmonary circulation, and is permitted by the "safety-valve function" of the valves on the ide of the heart, when its cavities are over-distended? One would too, that Dr. Paine had never seen a case of asphyxia, in which ngestion of the cerebral veins, as well as of those of the face, liver, nes, and general surface, is sufficiently apparent, and evidently ds upon the stagnation of the blood in the pulmonary capillaries. aine will not, we apprehend, feel highly flattered by our comparing de of philosophising to that of Magendie; and yet, opposite as eir results, they are attained by processes essentially the same; logmatically condemning the theories of others, professing to ab- ll theory, and yet building up hypotheses of his own still more un- than those which he endeavours to overthrow. We will take just ore example: "In obliterations of the jugular veins, or of the ava descendens, there may take place preternatural accumulations od in the cerebral veins. But we do not find that they have ever ttended by any of the phenomena of congestion, or of extravasa- of blood or serum, or by any interruption of the cerebral functions." 2.) Surely *hanging*, whether judicial or suicidal, is not so rare an ence in America, that Dr. Paine has never witnessed its post- m phenomena; but if he is sceptical as to the disturbance of the al functions which is stated on very credible authority to result he obstruction to the venous circulation in the neck, he has only ce a ligature on any one in such a position that it shall compress gulars without interrupting respiration, and we are satisfied that l be very soon desirous of loosening it.

ving established, in his own opinion, that "venous congestion is in pect mechanically determined by a *remora* of blood," he proceeds w, and we think with more success, that a mere passive relaxation

of their parietes will not produce it; and that the cause is to be sought in some morbid condition of the trunks themselves. This he regards as of an inflammatory nature; producing varicose enlargements when local and chronic, and giving rise to "congestive fevers," of which the cholera asphyxia is one form, when general and acute. A considerable body of evidence, from symptoms, post-mortem appearances, and the effects of treatment, is brought forward in support of this view; and we are disposed to recommend it strongly to the attention of our readers, although not in all the exclusiveness in which it is urged by its author. We may remark that that kind of permanent congestion, or varicose condition of the veins of the choroid coat of the eye, which frequently produces well-marked changes in its external aspect, is unquestionably of inflammatory origin. Dr. Paine speaks highly of the effect of free venesection in the *early* stage of such forms of typhus as would otherwise *soon* require large quantities of stimulants to prevent fatal depression. In an appendix to this essay, the author attributes death by cold to *his* form of venous congestion, of which he considers that agent to be one of the most common exciting causes. We cannot but think, however, that he does not give sufficient weight to the congestion of the internal veins, which is produced by the contraction of those nearer the surface, and which, although *remotely* dependent upon the vital contractility of the superficial veins, is *immediately* brought about by a physical cause. May it not be that this mechanical distension is itself an exciting cause of the morbid condition of the parietes of the veins, to which Dr. Paine restricts the term congestion?

The next essay, "On the comparative Merits of the Hippocratic and Anatomical Schools of Medicine," contains many sound remarks on the absurdity of the pretensions of the modern French school of morbid anatomy, and the necessity of the observation of the phenomena of disease in the living state for the success of medical practice. Having on several occasions expressed our opinion strongly on this point, both in its practical and abstract bearings,* we shall not now go over the ground again with Dr. Paine; but we may remark that he does not, in our opinion, sufficiently discriminate between the *science* and *art* of medicine—pathology and practice. Hence, if a certain fact, or series of facts, is useless as regards the latter, he neglects its application to the former; forgetting that the more perfect the science is made the more sure must the art ultimately become. He seems to forget, also, how much the observation of phenomena during life has been guided by the knowledge derived from post-mortem examination, and how frequently the value of a particular sign would be lost, if it were not connected in the mind of the practitioner with a coincident morbid state, ascertained by post-mortem examination of similar cases to be probably existing. How else, for example, should we have learned that the occurrence of delirium in acute rheumatism is an almost certain indication of incipient disease of the heart, and that remedies must be promptly applied to the chest instead of to the head? Our readers will, doubtless, think of many similar instances for themselves. It is an old observation, that we do not know the value of a blessing until we have lost it; and we would say, in like manner, that we could scarcely tell how much of our knowledge

* For the latter, see especially vol. VI., p. 106.

of the phenomena of disease in the living state is really due to post-mortem anatomical researches, unless we were entirely deprived of the assistance we have derived from that source.

The work concludes with a severe review of the writings of Louis, in which Dr. P. points out certain alleged fallacies of his method of generalizing, and exposes his hasty condemnation of previous observers. It may be thought a little strange that Dr. Paine should see these faults so glaringly in another, but should be so utterly unconscious of them in himself; but, alas! for human nature, such a self-delusion is by no means uncommon. We consider this review as strongly marked as any of Dr. Paine's writings by his peculiar exclusiveness; for, not being able to allow to M. Louis's system of observation all the merit which has been claimed for it, he seems to regard it as not only useless but strongly injurious; and he does not give him by any means sufficient credit for the mass of valuable materials which he has collected for the benefit of those who can use them aright, or for the numerous and admirable results, pathological and practical, which have flowed from the use of his method by himself and followers.

In taking our leave of Dr. Paine, we have to express our regret that it has been our misfortune to differ so materially from him in our estimate of the value of his opinions. We doubt not that none but the most laudable motives could have induced him to bestow so much labour and expense on the propagation of these opinions, and we feel grateful to him for the frequent and respectful appeals which he makes to our sentiments; but we cannot blind ourselves to the want of adaptation between the greater part of his volumes and the present state of medical science, and to the frequency of the rash assumptions which prevent us from attaching weight to his judgment. Learning, zeal, and industry they certainly evince, and those of our readers who find these qualities sufficiently attractive, and who can spare time to discuss fifteen hundred full octavo pages, had better form their own opinion of the other merits of the work, without being influenced by our award.

Art. VI.

Lectures on the Morbid Anatomy of the Serous and Mucous Membranes. By Thomas Hodgkin, m.d., &c. Vol. II. Part I. *On the Mucous Membranes.—London,* 1840. 8vo, pp. 541.

In noticing the first volume of this work in a former Number (vol. IV. p. 31,) we entered at some length into the pathology of the serous membranes, reserving the consideration of a portion of the volume in which some other subjects were treated of until the remaining lectures should come before us. The time which has since elapsed, however, although it has by no means lessened the value of the information contained in these lectures, has yet taken away from its novelty; and as most of our readers have probably become acquainted from various sources with the substance of Dr. Hodgkin's views upon the nature and general characters of malignant disease, we shall at once pass on to the mucous membranes, which form the subject of the volume before us.

The first lecture, the thirteenth of the series, is on the general

anatomy of mucous membranes, and professes to give a general view of their structure and analogies. The relation of this tissue to the cutaneous system is pointed out, and the opinions of various authors as to this point brought forward. Bichat's division of the mucous system into gastro-pulmonary and genito-urinary is then adverted to, and a third section, which may be termed the mammary, consisting of that portion which lines the lactiferous tubes and ducts, constituted. This leads to a consideration of the question of the origin of these membranes from the skin, which is shown from the phenomena of progressive development to be untenable. In his observations upon the structure of mucous membranes, we notice that Dr. Hodgkin is not more inclined to admit the globular theory of elementary texture in the case of these membranes than he is in the case of the serous membranes. He adds, however, that neither Mr. Lister nor himself have been able to detect in the mucous membranes that distinctly fibrous texture which belongs to some at least of the serous membranes, and considers their structure to be of a more amorphous character. (p. 14.) In alluding to the varying extent of these membranes, and to the alternate states of distension and contraction to which they are subjected, Dr. Hodgkin takes occasion to remark that many of their movements are effected "by a layer, in most cases double, of contractile fibres, generally considered as muscular, which may be regarded as an appendage to the mucous membrane, stituting a true panniculus carnosus." He infers, however, from the difference found to exist between these fibres and those of true muscle when examined under the microscope, and from a dissimilarity of their chemical composition, that they form a distinct animal tissue. These are not the only motions which the mucous membranes undergo, since by the aid of powerful lenses ciliary movements have been observed on the free surface of several of these membranes, which it is probable exercise an important part in some of the functions performed by them. (p. 15.)

The observations which follow on the pathology of the mucous membranes embrace their congenital or acquired deficiency and excess; partial acquired redundancy forming polypus; varieties in their secretion; the effects of inflammation; and other subjects to which we shall take occasion to refer in the course of our remarks upon the succeeding lectures.

The mucous membranes of the larynx, trachea, and bronchi, with the terminations of the bronchial tubes, form the subjects of lectures xiv. and xv. A good account of the morbid changes observed in the pulmonary mucous membrane generally is given, but we do not find that it contains anything with which the writings and descriptions of preceding authors have not already made us acquainted. Dr. Hodgkin examines first the mucous membrane of the larynx and upper part of the trachea; secondly, that of the lower portion of the trachea, the bronchi, and their ramification; thirdly, the bronchial cells or terminations of the tubes in the pulmonary tissue. The changes connected with a diseased state of the mucous membrane itself, of the larynx, trachea, and bronchial tubes respectively, differ but little if it all, and might have been classed together with advantage as far as regards the descriptive details. For the most part they may be resolved into the effects of inflammation variously modified, the plastic form giving rise to the formation of false mem-

nes, which when seated in the larynx and trachea constitutes croup. : ordinary form, that in which the matter effused on the surface of the nbrane does not partake of the characters of coagulable lymph, varies ch in degree as well as in extent, and may present simply a congested e of the capillaries or be attended with more or less thickening, ening, abrasion, or ulceration; the secretions at the same time underng every gradation of change from simple mucus to purulent or at st puriform matter with or without admixture with the red particles, um, or other constituents of the blood. The great difference in a ctical point of view and in the physiological effects produced arises m the situation of the parts affected. The larynx and trachea occupying upper portion and entrance of the air-passages when seriously involved st necessarily present symptoms of very threatening aspect. This arises only from the danger of immediate suffocation from partial or comte closure of the tube, but also from the secondary effects on the brain. this respect therefore diseases of very different character are found present similar symptoms, as for instance, inflammatory œdema of the ttis and croup. It is, however, of great importance, as Dr. Hodgkin tly observes, to form the diagnosis between these two affections during , as the measures which would be applicable in the former case, and en indeed absolutely essential to save the life of the patients would be the latter if not injurious certainly of little avail. It is the submucous lular tissue which is the seat of disease in the œdema laryngis, the cous membrane itself being usually little if at all altered. This n of disease is commonly found to occur in syphilitic patients who e taken cold while under the influence of ptyalism, and we recollect nessing a case in which a fatal result ensued from the operation of heotomy being declined by the surgeon called in consultation, on the sumption of the trachea being implicated in the disease.

n detailing the morbid alterations found in the bronchial tubes, the hor alludes to the dilatation of the tubes first pointed out by Laennec. ee forms of this affection are distinguished by Andral: the first, that which certain of the subdivisions of the bronchi, instead of becoming dually smaller, nearly equal in size the tubes from which they arise; second, where there is an absolute dilatation in one or more tubes, ning a kind of *cul-de-sac*, often of considerable size, which has been npared to the finger of a glove; the third, in which there are numerous rnate dilatations and contractions, somewhat resembling, as Laennec tes, the appearance of the podded fronds of the *fucus vesiculosus*. In first form the mucous membrane of the dilated tubes is often of a livid our, sometimes softer than natural, and occasionally ulcerated: somees, as Dr. Hodgkin observes, of extreme tenuity; in other cases, re is sensible thickening, with cartilaginous hardness of the affected es. Where the dilated tubes are numerous, the surrounding pulmoy tissue becomes consolidated; and M. Louis has observed that in h cases, when the consolidation is extensive, a dull sound is occaned on percussion over the affected part. In the second form, which ald seem to be more frequent in children, the dilated tubes are more nmonly found to contain fetid mucus and pus; whence the affected an, when cut into, presents the appearance of containing numerous ll abscesses. The third form is the most rare, and like the second

is also usually found in the lungs of children, the cavities being, in like manner, filled with muco-purulent matter, and presenting similar characters when the lung is cut open. Among the causes assigned are the occurrence of paroxysms of cough, as in hooping cough, and viscidity of the bronchial secretion, leading to its accumulation in certain tubes or portions of tubes. The former, it is thought, is most likely to be concerned in the production of the first form of dilatation; the latter in giving rise to the second and third forms. In reference to the distinguishing of these dilatations, Dr. Hodgkin observes:

"Although there is little danger of mistaking dilated bronchial tubes in the dead subject, when the examination is made in the mode which I have pointed out, by laying them open from the principal branches, yet, when we come upon them by sections into the substance of the lung, they may be mistaken for abscess in the lung, as I have already hinted, or for an excavation produced by the softening and expectoration of a tubercle, to which the resemblance is particularly close, when the dilated tube and the surrounding pulmonary texture have become indurated. If the section be made near to the spot at which several dilated bronchial tubes unite, the appearance which they present is almost precisely that which is seen on the section of a multilocular tuberculous cavity; yet, even in these cases, error may be avoided, by tracing the tubes to a short distance after the section has been made." (p. 62.)

Various forms of bronchitis and catarrh result from the pathological states of the mucous membrane and its secretion. A deficiency in the quantity of the secretion gives rise to that form of bronchial disease, described by Laennec under the term *caturrhe sec.* An increase of the secretion, with more or less change of properties, characterizes the acute mucous catarrh, the chronic mucous catarrh, the pituitous catarrh, and their varieties, of the same author. Dr. Hodgkin believes "that a very general increase of the secretion of the bronchial mucous membrane, sufficient to produce almost universal mucous rattle, and a corresponding difficulty of breathing, imminently threatening or even causing death, may be occasioned by an irritating cause, the direct application of which may be very partial." (p. 64.) The cases to which he refers, however, scarcely admit of explanation upon this principle; and we should be more inclined to consider them as examples of some general affection of the mucous membrane, such as we often witness in cold, damp situations, supervening upon the more local and limited disease, than as directly taking their rise from that source. The nature of the secretion effused into the bronchial tubes, as we have before remarked, embraces every shade, from the natural transparent mucus up to muco-purulent and puriform matter; it is also frequently complicated with the serum of the blood, or the vessels of the membrane may pour out blood itself in large quantities. Like the mucous lining of the larynx and trachea, the bronchial membrane is, in like manner, occasionally the seat of a plastic form of secretion. Dr. Hodgkin mentions a case; and several will be found in some of the older writers, to whom, however, the real nature of the affection was apparently unknown.

In inflammation the membrane is found variously altered, usually vascular and injected, and presenting different shades and degrees of redness; sometimes it is of a bright and deep scarlet, at other times purple brown, or livid. The livid tint is probably most usual in the chronic forms of bronchitis; but in these, especially when there is a free and passive

secretion, the membrane is not unfrequently pale throughout. Dr. Hastings states, that in the bronchitis supervening upon measles, he has found the mucous membrane red in patches which approach nearly to the shape of a crescent. He has also observed ulcers in those dying of variola, similar in size to the pustules on the skin. According to the experience of Dr. Hodgkin, ulceration in the bronchial tubes, unless in connexion with tubercles, is of rare occurrence; in which statement he is borne out by Laennec. According to M. Louis and Dr. Stokes, however, ulcers are more frequently met with in the bronchial membrane; and, in many of Dr. Hastings's cases of the chronic form of bronchitis, small ulcers and superficial ulcerations were observed, the membrane being at the same time variously congested and sometimes thickened.

The third portion of the pulmonary mucous membrane, or that lining the air-cells, will require a rather more extended notice. The following method, for which the author acknowledges his obligations to the late Dr. Babington, is recommended as well calculated to show the ultimate ramifications and terminations of the bronchial tubes.

"A collapsed portion of a healthy lung should be taken, having as small an incised surface as possible; and, on this account, one of the lobes of the lung of an inferior animal answers remarkably well. This portion of lung should then be injected, from the bronchial tube, with the white of egg, in sufficient quantity to distend it, and render its pleural surface smooth. The bronchial tube and the incised surface are then to be secured by ligature, and the whole boiled for a sufficient length of time firmly to coagulate the albumen. By the same process the cellular membrane is so much softened as greatly to facilitate the separation of the structure of the lung without injuring the albumen, which has taken the impression of the cavities into which it was injected. In this way we may discover that nearly all the bronchial ramifications lose their fine tubular form, when they have arrived at a particular degree of minute subdivision; and that, beyond this point, the injected albumen is infiltrated through a spongy texture, so minute that not only its precise form cannot be made out, but its white colour is lost and converted into a gray, from its intermixture with the structure forming the minute cavities in which it is situated." (pp. 79-80.)

Two opinions have been advanced, as our readers are aware, respecting the terminations of the bronchial tubes; one, that there is a free communication between the air-cells and the cellular tissue of the lungs; the other, that the ultimate ramifications of the bronchi are individually unconnected with each other, or with the surrounding pulmonary texture. It would seem from the above method of examination, as well as from others, as Dr. Hodgkin remarks, that there is a communication existing among the air-cells, but that this communication is not indefinitely extended throughout the whole pulmonary tissue.

In relation to the anatomical characters of this, the lining membrane of the air-cells, the question as to the serous or mucous character of the tissue is considered. Unquestionably there are some points of difference between the pulpy villous surface of the better-defined forms of mucous membrane and the attenuated texture of this portion of the bronchial membrane. The transition, however, is gradual, and its appearance analogous to what is observed in the conjunctiva of the eye and in the membrane lining the tympanum, while some of the characters of the serous membrane are wanting. The spongy tissue in which the minute rami-

fications of the bronchi terminate is regarded as a modification of the cellular membrane.

Passing over various irregularities of development and arrangement, we come to dilatation of the air-cells, or pulmonary emphysema. The causes of this affection, which has of late been so ably illustrated by MM. Louis and Lombard, would seem to be the same as those productive of dilatation of the bronchial tubes. One curious peculiarity attendant upon this disease, to which we are desirous of drawing attention, as having escaped the notice of many acute observers, is that, although there is a strong tendency, as Laennec remarks, in those suffering under this form of pulmonary disease, to organic changes in the structure of the heart, and especially to hypertrophy of the right ventricle, there would seem to be a corresponding diminution in the liability to inflammation of the pulmonary organs. Dr. Hodgkin follows Dr. Addison in regarding all forms of pneumonia, or inflammation of the substance of the lungs, as having their seat on the internal surface of the air-cells. The arguments in favour of this view are, the existence of the rattle, the changes taking place in the expectorated matter when the complaint terminates in resolution, and some phenomena occurring in connexion with pulmonary apoplexy, as it is termed, or the effusion of blood into the air-cells. These characters, it is inferred, necessarily derive their origin from the bronchial membrane and its secretions being implicated—a conclusion which we are by no means disposed to deny; but we cannot see that the fact of this tissue partaking in the morbid action by any means excludes the surrounding cellular texture from being itself the main or primary seat of the inflammatory process.

It is well known that the leading appearances recognized as deriving their origin from inflammation of the pulmonary structure are those designated as engorgement, red hepatization and gray hepatization, and that these are usually considered as indicating different stages of the same process; the engorgement making the first stage, and the red and gray hepatization the successive and more advanced steps of the morbid action, in inducing disorganization of the texture of the lung. Dr. Hodgkin is disposed to take a different view of these appearances, and regards some of them, at least, as being connected with and dependent upon a difference in the process of inflammation itself. We shall endeavour to give a condensed account of his opinions upon this subject, as they involve considerations of some importance as well on practical as on theoretical grounds. It will be recollected that, in treating of the serous membranes, as well as in examining the lesions of the lining membrane of the larynx, trachea, and bronchial tubes, two forms or modes of inflammation have been recognized, in the one of which there is secretion or effusion of fibrinous layers susceptible of organization, and assuming a membranous character, in the other the product chiefly consists of serous or watery fluids occasionally holding minute particles in suspension, which, however, show no disposition to cohere together or become organized. To the first of these the term plastic inflammation is applied, to the second that of non-plastic, or inflammation without plastic effusion. In the plastic form of inflammation of the lungs, the most unequivocal specimens of which are met with in children, the effused lymph is almost in-

riably tinged with the colouring matter of the blood, the substance of : lung loses its spongy texture and becomes perfectly solid, and, in- ad of its usual light red or grayish colour, generally assumes a dull t deepish red, very much resembling that of a tolerably healthy liver. in slices, carefully washed in water, although they may lose some of ir colouring matter, do not allow of the deposit being washed out, d, consequently, do not recover their cellular structure. (p. 94.) . Hodgkin thinks that he is borne out in considering this red hepa- ation as the product of the plastic form of inflammation by a case in ich it was combined with plastic effusion in the bronchial tubes lead- ; to the affected portions of lung, similar to that produced in the tra- a in well-marked croup. This alteration is most usually seen in the lobu- pneumonia of children, but is sometimes also met with in adults, both the lobular form, and more diffused through the lung. The author is opinion that the puckering or contracted appearance, not unfrequently served on the surface of the lung, is occasionally owing to the loss of stance, contraction, and induration of the cellular tissue, which should low the subsidence of the inflammation. "On cutting into the sub- nce of the lung," he observes, "to ascertain the state of the interior, which the superficial appearance depends, we do not always discover ner the partially obliterated remains of a tuberculous cavity, or any er indication of the preexistence of tubercular disease, but, on the trary, a portion of lung remarkable for its induration and solidity, l want of cellular character. It is densest at the centre, whilst its ill- ined circumference passes imperceptibly into the surrounding healthy icture." (p. 97.) We need not tell our readers that Laennec had ne to the conclusion that the puckered appearance of the surface of : lung in question depends upon the gradual obliteration of cavities m which tuberculous matter had been removed, and the contraction l drawing together of the pulmonary tissue; and cases are not want- to bear out his conclusion. The general fact that these peculiar ap- rances are usually found at some portion of the superior lobes of either g, that is, in the parts most liable to tubercular deposition, and less quently the seat of inflammation, is also opposed to the opinion put ward by Dr. Hodgkin; still, we confess, we are more disposed to pt his view of the case than that of his illustrious predecessor.

The gray or light-coloured consolidation of the lung (gray hepatization, it is termed) is considered to be the product of the non-plastic form nflammation, by no means a subsequent stage of the red hepatization, exhibiting the same pale colour from the very commencement: Dr. dgkin is induced to form this opinion, first, because the duration of es, which, upon inspection, have proved to belong to this form of umonia, has been too short to admit of the probability of a transition n the red to the gray having taken place; secondly, that while in most plastic form of inflammation the bulk of the lung is but little red, in this form there is often great distension of the pulmonary tex- :. "On making a section through the affected portion, the incised face is of a dead soiled white colour, mottled with black pulmonary ter, with a few scattered spots of a reddish colour, generally small : irregular. These red spots are produced by the section of blood- sels, or by small portions of lung still containing red blood. Slight

pressure causes a whitish opaque cream-like fluid to exude from every part of the incised surface." (p. 99.) A thin slice of lung affected with this form of inflammation, if carefully washed, without breaking down the texture of the part, may have the product of the inflammation so completely removed as to show the natural and healthy character of the lung. When this form of pneumonia occupies a considerable extent of the lung it generally proves fatal; when of less extent, the substance of the lung, it is thought, becomes broken down, giving rise to a cavity much resembling those produced by the softening of tuberculous matter, and by its subsequent contraction, sometimes occasioning a puckered appearance on the surface of the lung. It is subsequently stated that every intermediate gradation between these two forms may exist. The more exquisitely-marked states are the most fatal; the intermediate varieties more readily admit of a favorable termination, the deficiency in plasticity, and consequent inferior tendency of the product of inflammation to adhere to the parts producing it, favouring its expectoration, and protecting the air-cells from obliteration on the one hand, while on the other the distension is less and the vitality of the tissue not affected to the same degree as in extreme cases of the non-plastic effusion. (pp. 98-102.)

In addition, however, to engorgement and the two forms of hepatization, with their complications and intermediate states, there have been enumerated among the products of inflammation of the pulmonary texture abscess of the lung, gangrene, pulmonary apoplexy, and œdema of the lung. The first of these is of very rare occurrence, and probably takes place only in the midst of portions of lung in which the air-cells have become obliterated and the texture consolidated by previous inflammation. The pus so formed is, according to Dr. Hodgkin, neither very copious nor very pure; he has never had occasion to see more than a drachm collected in a cavity of this description. The expectoration of purulent matter in considerable quantity, which has sometimes given rise to the opinion of the bursting of an abscess into the bronchial tubes, is now pretty generally admitted to be the result of a communication established between the pleura and the air-tubes in cases of empyema. Other sources of error are the muco-purulent collections found in dilated bronchial tubes; partial and limited empyema of an interlobular fissure, the matter being confined by adhesions towards the edge of the fissure; tuberculous cavities containing puriform secretion, and cavities formed by the breaking down of the lung under the non-plastic form of inflammation. (p. 110.) The remarks on gangrene of the lung present nothing requiring our attention here, unless it be an allusion to its frequent occurrence among the insane, as observed in the lunatic asylum at Ghent. We know not that this observation has been confirmed elsewhere, but we may remark that one of the most marked examples of this state which has come under our notice was in a man affected with delirium tremens.

Pulmonary apoplexy, as it is called, or the effusion of blood into the lungs, though usually resulting from a congested state of the capillary vessels, can scarcely be considered as one of the products of inflammation. A good account is given of the subject, as far as we are acquainted with it; but we may observe that it is one which requires closer investigation than it has hitherto met with to determine the source of the effused blood, and other points of interest connected with it. That the blood

arely proceeds from the rupture of a vessel of any size, is now generally admitted; in which respect a striking analogy is established with hemorrhage from the stomach and intestinal canal generally. To this circumstance we shall have occasion to advert when considering this part of the subject, at present we must be content with quoting the following remark of the author: "I must not omit to state," he observes, "that since the peculiar appearance of the lung, which I have just described as connected with hæmoptysis (small ecchymosed spots scattered throughout the substance of the lungs) first arrested my attention, the examination of one or more recent cases, and the observations of my friend, A. Tweedie, have induced me to feel disposed to unite with him in doubting whether the spotted appearance of the lung has not been the consequence rather than the cause of the hæmoptysis; seeing that it may be brought about by the passage of the blood down the air-tube into the lungs, rather than in the opposite direction." (p. 124.)

In section xvi., Dr. Hodgkin treats of tuberculous deposits in the lungs, prefacing his account by some general remarks on tuberculous matter, its chemical analysis, the variations in form and character which it assumes, its nature and origin, connexion with inflammation, &c. A pretty full account of tubercle, as it appears in the pulmonary organs, is given under the several heads of miliary tubercles; crude tubercles; cavities produced by the expectoration of softened tubercles, a remarkable but frequent form of tuberculous deposit in the lungs which does not appear to have been hitherto distinctly pointed out; tubercles combined with a peculiar form of emphysema of the lung; tubercular infiltration; the stages of crudity, and softening of infiltrated tuberculous matter; tuberculous deposits, succeeding to pulmonary inflammation; and tubercles which do not undergo the process of softening. Some general remarks upon phthisis, and a tabular view of morbid appearances observed in phthisical subjects, drawn up by Mr. T. W. King, from the work of M. Louis, are appended. Several questions of much interest are alluded to in the course of the lecture; but, with the exception of the sections referring to the undescribed form of tuberculous deposit, and to the combination of tubercle with a peculiar form of emphysema, we have not discovered anything with which our readers are not already sufficiently familiar. Under the former of these heads is described the infiltration of portions of the pulmonary texture, varying in size from that of a pea to that of a walnut, or even a small orange, with a grayish translucent effusion, which Dr. Hodgkin regards as an early stage of tuberculous matter. Almost at the circumference of the mass a multitude of small opaque whitish points, similar to those observed in the centre of miliary tubercles passing into a state of crudity, are generally found, and these are so thickly placed together, that, in whatever direction a section is made through the indurated portion, it exhibits a dotted whitish margin, so as almost to suggest the idea of chalcedony set in small pearls. In the translucent gray centre a few small whitish points, less than those which constitute the margin, may frequently be observed. When these become larger and more numerous, and unite, we have a rounded mass of opaque tuberculous matter; and ultimately, when they soften, a tuberculous cavity of the same size. Dr. Hodgkin infers from these appearances an argument in favour of the views of Laennec, and opposed

to those of M. Andral, as to the primary character of the gray translucent matter, and the subsequent conversion of the same into the opaque whitish matter. (pp. 164-6.)

In the combination of tubercle with a peculiar form of emphysema, the tuberculous matter is described as occurring in smallish irregular fragments, many of which are accompanied by a cavity containing air in the form of a vesicle, and bearing no resemblance to the ordinary tubercular excavation. Instead of somewhat thickened parietes lined by an imperfect false membrane covered with the *debris* of the tubercle mixed with muco-purulent secretion, the cavities here spoken of have very thin parietes. This vesicular character of the cavities constitutes a peculiar form of emphysema, to be distinguished both from the ordinary emphysema depending upon more or less general dilatation of the air-cells, and from interlobular emphysema of the cellular tissue.

"In the cases now under consideration, although the cavities evidently depend on dilatation of some of the air-cells, yet there does not seem to be necessarily any tendency to dilatation of the neighbouring air-cells. The dilatation appears to be essentially connected with the deposition of tuberculous matter, to which I believe it to be a sequel; since, in the same lung in which these tubercles with vesicles occur, we may find numerous other tubercles without any accompanying vesicle. The tuberculous matter in this cavity appears to be lodged there without any envelope; yet, upon careful examination, in some instances I discovered a very thin and almost imperceptible membrane passing off from the sides of the cavity, and retaining the tuberculous matter in its situation. This membrane appears to throw a little light on the mode in which this complication of emphysema and tubercle is brought about. It would seem that tuberculous matter has been so deposited as to prevent the exit of air, although it allows its ingress: the cells placed under this influence consequently become dilated, and form a somewhat irregular cavity. This distension must of course, in degree, compress the neighbouring pulmonary structure, and bring the tubercular matter to appear as if placed within the cavity; which, indeed, eventually becomes the case, when the delicate membrane before mentioned gives way." (pp. 167-8.)

Before leaving the subject of tubercle of the lung, which, whether in the form of infiltration, or in that of miliary tubercle, is regarded by Dr. Hodgkin as being situated, like the product of inflammation in pneumonia and the effused blood in pulmonary apoplexy, within the air-cells, we must allude to an extraordinary omission of all reference to the views of Dr. Carswell. This is a serious oversight in a work on the mucous membranes, as the effusion of tubercular matter upon the surface of the membrane lining the bronchial tubes is at least as important, and deserves to the full as close an investigation as the varieties of muco-purulent and plastic effusion, or many other subjects treated of in the lectures before us. Perhaps the omission is accounted for by the lecture having been delivered previously to the publication of Dr. Carswell's work, but surely the subject might have been referred to in a note.

The morbid states which form the subject of the seventeenth lecture are for the most part, strictly speaking, foreign to the mucous membranes, though connected with the pulmonary organs. They include the products of disease seated in the cellular structure uniting the lobules, alterations in the vessels and nerves of the lungs, certain adventitious productions, accidental injuries, &c. Some of them would have found

a more appropriate place among the lesions of the serous and sub-serous tissues; others might have been considered under the heads of parasitic animals, adventitious structures, and malignant disease, described in the lectures forming a part of the first volume; others again should have been reserved as belonging to the nervous, vascular, and glandular systems. Contenting ourselves therefore with a reference to this lecture, which contains many interesting notices and valuable observations relating to pulmonary diseases, we pass on to the remaining portion of the volume, of which the mucous membrane of the alimentary canal, with its morbid states, forms the subject.

In describing the alterations of structure which are found to occur in this portion of the gastro-pulmonary mucous membrane, Dr. Hodgkin adopts the following divisions: 1, the mucous lining of the mouth; 2, that of the fauces and pharynx; 3, the œsophagus; 4, the stomach; 5, the duodenum; 6, the remainder of the small intestines; 7, the cæcum and colon; 8, the appendix vermiformis; 9, the rectum. The first six of these divisions are considered in lectures xviii. to xxiii.; the last three are reserved for a succeeding portion of the work.

The affections of the mucous membrane of the mouth, fauces, and pharynx, present certain peculiarities, arising partly from exposure, and partly from a difference in anatomical structure. Under this head ptyalism, aphthæ, ulceration and sphacelus, certain diseased states of the tonsil, pharyngeal abscess, &c. are treated of. The account given, however, of some of these states is defective, and exhibits neither the precision nor the comprehensive details which, for the most part, characterize other parts of these lectures. The blue discoloration of the gums in persons affected with lead-poisoning we find no allusion to, and cancrum oris, with its allied forms of disease implicating the mucous membrane and neighbouring tissues, are dismissed with the brief remark, that "in children who have suffered from acute and severe disease, inflammation of the mouth sometimes proceeds to the production of sphacelus of all the textures, not excepting the common integuments. This disorganization proceeds rapidly, produces a dark and almost black colour, and is quickly fatal." (p. 228.) Both these are points deserving of further illustration, the former as likely to prove of material assistance in establishing a correct diagnosis in doubtful cases, the latter as constituting one of the evils occasionally resulting from the confinement and impoverished diet of some of our parochial and criminal establishments. We were desirous of ascertaining whether Dr. Hodgkin, in the course of his extensive experience as a pathologist, had met with any cases of abscess at the back of the pharynx similar to those recently narrated by Dr. Churchill. No example of this kind, however, seems to have come under his notice, though the following case, as far as we can judge from the concise manner in which it is detailed, presents considerable analogy to Dr. Churchill's cases:

"A soldier was admitted into this hospital (Guy's) who had passed a considerable piece of lint up behind the velum palati, where it was completely concealed, being firmly lodged quite at the upper part of the pharynx: this circumstance was not discovered till the body was inspected: his symptoms had been, inflammation about one ear, and *very severe cephalic and thoracic derangement.*

Extensive phlebitis had been set up, affecting not only the veins of the neck, but the sinuses within the cranium." (p. 245.)

There is little requiring notice in the observations on the diseased states of the œsophagus. A remarkable case of polypus is quoted from Dr. Monro's work on Morbid Anatomy, and in considering the subject of stricture of the œsophagus some judicious reflections upon the use of the bougie are introduced. The contractile fibrous coat beneath the mucous membrane is regarded by Dr. Hodgkin, notwithstanding its continuity with the muscular expansions of the pharynx, as being of a different character from muscular tissue, and identical in structure with the fibrous coat of the stomach and intestines generally. He observes, "that notwithstanding its fleshy appearance, its fibres, when seen through the microscope, present no trace of that appearance of transverse striæ which is exclusively met with in confessedly muscular fibres; and that it does not, when very thinly extended, moistened, and compressed, exhibit the perfect nacreous lustre which, under these circumstances, he has invariably observed to be distinctly visible in genuine muscle." (p. 260.)

In entering upon the consideration of the morbid changes occurring in the stomach, Dr. Hodgkin very properly commences with an endeavour to ascertain what are the appearances presented by its internal surface in the state of health. Much difference probably exists according to the varying circumstances under which the organ is habitually placed: thus, it has been shown by John Hunter, that an herbivorous animal, the sheep for instance, might be made to eat flesh; and, on the other hand, that the eagle might by education become an exclusively vegetable feeder, such alterations as might have been expected causing a remarkable change in the stomachs of these animals. (p. 265.) Nor can it be doubted that the habitual use of various kinds of diet, as the exclusively or chiefly farinaceous, the vegetable, animal, &c., liquid or solid, mild or stimulant, must exercise an influence on the general state of the mucous membrane and its secretions, without inducing what can properly be termed disease. Very various are the accounts given by different authors who have written upon this subject, of the natural and healthy colour of the membrane. It has been described as being white, grayish white, grayish, reddish, grayish approaching to yellow and red, straw coloured, &c. Billard, in whose opinion Dr. Hodgkin is inclined to place most confidence, states it to be a dead milky white. According to Buisson and Bichat, the colour is of a deep red, and Sabatier and Habicot describe it as of a reddish purple and dull purple. Gavard, Boyer, Sœmmering, Chaussier, and Adelon, make it of various shades between red and grey. Rousseau, who derived his opinion from the examination of the bodies of criminals dying by the hands of the executioner, (by the guillotine we presume,) states that the colour of the gastro-intestinal canal is white, or white faintly tinged with red. Dr. Yelloly states, that in various opportunities which he had of examining the human stomach soon after death, "in such parts of it as were free from vascularity, it had usually a light straw coloured tinge," but gives it as his opinion that "from the analogy of the mucous covering of the mouth and fauces, and of the urethra, it is probable that when circulation is going on in the stomach, its inner surface is of a pale red hue, arising from vessels so minute as to give an

uniform colour, without any appearance of distinct vascularity." (Med. Chir. Trans. iv. pp. 393-4.) We are ourselves rather disposed to agree with M. Hippolyte Cloquet, who describes the usual appearance of the membrane as being of a reddish white and mottled (comme marbrée); but we must observe, that this diversity of opinion as to a fact so evident to the senses, could only have arisen from the varying appearances of the membrane presented to the several observers under different circumstances of disease, or from the effects of certain physical agents acting during the last moments of life. The manner of death would appear to exert considerable influence; the presence of aliment recently taken into the stomach causes a decided red tinge throughout the membrane; extremes of cold and heat, according to Beaupré, are also productive of a like effect in the mucous membranes generally, and the stomach has been observed to take a decided tinge from various medicines administered shortly before death. Other variations also, in addition to those of colour, are observed in the mucous membrane of the stomach to the full as important in the due appreciation of its diseased conditions: thus, as Dr. Hodgkin remarks, differences in the thickness and firmness of its substance, and we may add in the varying nature of its surface, may exist between the stomachs of different individuals in perfect health, and causes in themselves trivial, and totally independent of disease, may occasion differences in these respects, which by an inexperienced or incautious observer, might be mistaken for the products of some morbid action. To these variations, however, in equality of surface, in texture and colour, as well as in the appearance of vascular injection not unfrequently presented by the mucous membrane, we shall recur in the course of our observations on various morbid changes taking place in the different portions of the intestinal canal.

The secretions of the stomach are liable to considerable alteration, giving rise to some of the more prominent symptoms of certain forms of gastric disease. With these, however, we need not here delay, but pass on at once to the consideration of the organic changes noticed by Dr. Hodgkin. Preternatural contraction of the stomach is sometimes met with as a consequence of inanition, and especially in connexion with stricture of the œsophagus; but the more usual morbid state in which there is deficiency of the gastric mucous membrane, is when it has been removed by softening and solution, or by ulceration. Dr. Hodgkin alludes to a case related by Dr. Graham of Edinburgh, "in which a very considerable portion of the stomach was supplied by the viscera about that organ; which were so united by adhesion, as to form the parietes of the cavity into which the food was received." (p. 277.) There are many on record of a similar character, though perhaps not equal in extent to the one especially referred to. Of redundancy arising from dilatation of the stomach, there is an interesting collection of instances recorded by Dr. Peebles in the Edinburgh Medical and Surgical Journal for July last, some of which are especially worthy of note as being unconnected with stricture of the pylorus, with which, according to Dr. Hodgkin, this peculiar state of the stomach would seem to be necessarily complicated. There is, however, another form of redundancy, consisting in the preternatural thickness of the membrane to which Dr. Hodgkin assigns the term hypertrophy. This form may be either "the result of an extinct

inflammation, or of a preternatural growth from other causes," and is not to be confounded with the thickened pulpy condition often found during states of irritation of the membrane, or in chronic inflammation.

"I should consider," says Dr. Hodgkin, "a portion of the mucous membrane of the stomach of the thickness of about one sixteenth of an inch or upwards as preternaturally thickened. If not actually in a state of inflammation, such thickened mucous membrane, although possibly reddened by the development of its capillaries, might be distinguished from the thickening accompanying present inflammation, by its superior tenacity and firmness, and by the healthy character of the mucus upon its surface. It is not so easy to distinguish those cases of preternatural thickening of the mucous membrane which are the result of inflammation, from those in which it is more probable that inflammation had no part. Yet, on the one hand, we may infer that inflammation has been concerned in producing the thickened state, when we can find traces of ulceration in or near the thickened part, where it is either exceedingly thickened, or inelastic, or excessively lacerable; and, also, when the subjacent cellular membrane is likewise altered in character, having lost its natural laxity, so as to fix the mucous membrane, and interfere with its movements on the subjacent coats. On the other hand, I should feel considerable difficulty in admitting the connexion between inflammation and thickening of the mucous membrane of the stomach, though this were considerable, if no trace of ulceration were discoverable, if the submucous cellular membrane retained its perfectly healthy condition, and if the mucus upon the membrane possessed its healthy and natural character, or, at most, was only rather redundant in quantity." (pp. 278-9.)

Partial hypertrophy of the mucous membrane of the stomach may be observed in that form of disease, designated by Billard and Louis as *mamelonné*, or mamillated. It consists in numerous elevations of a rounded figure, and from two to three lines in diameter, thickly scattered over the surface of the stomach to a greater or less extent, and separated from each other by furrows, in which the membrane is perhaps thinner than natural. Dr. Hodgkin had long been in the habit of noticing this state of the mucous membrane, which he had designated by the term "granular." He regards it as arising from the greater or less development of the natural inequalities existing in the surface of the membrane which are sometimes seen in the healthy stomach when examined early after death. This state is frequently accompanied with ulcerations, from which M. Louis infers that it is a product of inflammation. From this opinion Dr. Hodgkin dissents, although he recognizes a somewhat similar alteration, to which he applies the term fungous as the result of that process.

"In these cases," he observes, "which are comparatively very rare, the elevations bear no resemblance in figure to the natural inequalities before spoken of. They have not the same oblong figure, are rounder, and perhaps more elevated; the mucous membrane is thick, and, containing more blood, is much redder than is natural; the stomach is contracted, and the cellular membrane, as well as the mucous, is much altered." (p. 283.)

This state is mentioned by Billard as occurring in conjunction with disease of the heart. After describing what he terms the inflammatory form of the affection, he proceeds to show that in diseases of the heart, and in those of the great vessels—in a word, in all cases in which the blood is permanently impeded in its course, the cellular tissue becomes infiltrated, œdematous swelling supervenes in almost every part of the body, and the

cellular coat of the intestinal canal partakes in the distension, giving to the mucous membrane the œdematous appearance which he had previously described. On the other hand the blood, driven back upon the vena porta and the mesenteric veins, or arrested in the arterial branches, finds its way into the intestinal vessels, and becomes diffused through the mucous membrane, giving rise to redness of that membrane, and increasing yet further the swelling : thence arises a fungous appearance of the inner coat, not of inflammatory origin, and which it is essential not to confound with the results of inflammation. It should be observed that Professor Carswell is inclined to view these elevations as being the follicles of the stomach enlarged, the red spots which are sometimes observed in the centre of the elevations being considered to be the orifices.

In the investigation of those morbid conditions of the mucous membrane which are presumed to indicate the existence of a previous inflammatory state, it is important to bear in mind that some of these are so entirely analogous in their appearance to changes induced in the membrane by other circumstances not connected with disease, as to be frequently with difficulty distinguished from them. Billard, whose work is a valuable contribution to the pathology of the gastro-intestinal mucous membrane, has stated, as we have seen, the usual colour of the internal coat in the state of health to be whitish; but he remarks also, that while the process of digestion is going on, it presents a diffused light rose-red colour. To the accuracy of this observation we can bear decided testimony, as many years ago, and before we had seen the work of this lamented author, we had, from experiments instituted for the purpose, arrived at the same conclusion. Similar appearances, as Dr. Hodgkin states, are produced by the use of alcohol and other stimuli, and the transition from the healthy activity characteristic of the digestive function to that state of irritation bordering upon inflammation, produced by the excessive abuse of some of these stimulants, is so gradual as not to allow of any decided limits to be drawn between healthy action and irritation on the one hand, or between irritation and inflammation on the other. Dr. Hodgkin mentions an appearance which he believes to be essentially characteristic of a recently acute inflammation of the mucous membrane of the stomach, which we neither remember to have met with, nor to have seen described elsewhere. The appearance to which he alludes "is that of an almost infinite number of scattered, small, nearly opaque, whitish spots, which are rather lodged in the mucous membrane itself than situated upon its surface. He has seen it in a case where death arose from prussic acid, and also where arsenic had been taken in a state of solution, but is not prepared to decide whether these spots are depositions of coagulable lymph or not.

Active inflammation of the lining membrane of the stomach is characterized by diffused florid redness of greater or less intensity, with points of still greater intensity, showing a distended state of the capillaries, which may assume either an arborescent appearance, or exhibit numerous small points, striæ, and stars, not unlike in form those of some minute lichens growing on the bark of various trees. From this tension of the capillary system, a degree of softening of the membranes takes place; it becomes easily lacerable, and, during life, in cases of great intensity, blood is sometimes effused, as in other mucous membranes, and in the serous membranes

under like circumstances. Sometimes also, as is observed in cases of poisoning with irritant and corrosive poisons, there is exudation of patches of coagulable lymph. This however is a rare occurrence in the stomach, and the acute form of gastritis, except in cases of poisoning, is scarcely ever observed. In speaking of this subject, Dr. Hodgkin takes occasion to allude to the opinions of Broussais, in reference to the connexion of fever with acute inflammation of the stomach, or other portion of the intestinal canal as its cause. We cannot but agree with him in thinking that, however Broussais may have been forestalled in the expression of his leading views by Baglivi, Rega, Prost, Frank, and others, and however erroneous some of his theories may have been, the lasting thanks of the medical profession are yet due to him for calling their attention to this subject, and for much valuable information upon it.

We cannot here follow the author through his description of the ordinary effects of chronic inflammation upon the condition of the membrane. It is sufficient to state that they present little difference from those of the more acute form, except in extent and intensity. It is of more consequence to make our readers acquainted with the marks which he lays down, by which cadaveric changes—the effects of a disordered state of the pulmonary organs, or of diseases of the heart, &c.—may be distinguished from those of inflammation. To assist in the discrimination of a congested state of the mucous membranes, the result of the cases referred to, from the effects of inflammation, attention is drawn to the following points:

"1. The texture of the mucous membrane in a state of injection differs only from the healthy condition in containing a larger quantity of blood, and that for the most part, of a venous hue. It is neither softened nor indurated; except, that the former state may sometimes have been partially induced by the operation of the solvent juices of the stomach, or as a part of the general effect of softening of different textures of the body; amongst which, the mucous membranes of the stomach is the most frequent as well as the most striking seat.

"2. The secretion on the surface of the membrane is not altered in quantity or consistence, except when discolored by transuded blood, when it may receive various degrees of intensity of red, and perhaps some increase of consistence.

"3. The submucous cellular membrane retains its natural texture, and therefore allows the natural mobility of the mucous membrane upon it: it likewise allows of the mucous membrane being torn off in shreds, as in the case of a mucous membrane presenting its most natural appearance.

"4. The vessels communicating with the mucous membrane, but more especially its principal venous branches, are distended and turgid with dark blood: this last appearance is perhaps the most important criterion by which we may be led to distinguish the effects of *congestion* from those of *inflammation*. (p. 295-6.)

In subsequently alluding to the brown and gray discolorations, Dr. Hodgkin advocates the opinion that they are to be regarded either as the effect of a wholly subsided irritation, or of a cadaveric change produced by the action of gases upon the blood in the capillaries. We think there is some reason to doubt the correctness of this view, especially with respect to the brown discoloration, which, although by no means disposed to consider it as being always the product of chronic inflammation, we are yet inclined to look upon as very commonly resulting from some modification of that process.

Ulceration and softening of the mucous membrane of the stomach next

engage attention; and under the latter of these subjects we find some important remarks. Dr. Hodgkin is disposed to adhere to the opinion of John Hunter, "that the softening of the stomach is really a cadaveric change, dependent on the structure of the stomach being acted on after death by its own solvent secretion." (p. 306.) We doubt not that, in many reported instances of softening, such may be the case; and we should in all cases be inclined to view the appearance with suspicion when it is conjoined with the presence of crude or partially digested aliment in the stomach. Still, that there are other instances in which the process of softening takes place during life, and is accompanied by certain symptoms which may be taken as characteristic of the affection, we cannot for a moment hesitate to admit; and, indeed, from the subsequent observations of the author, he would himself seem to incline to this opinion—that is, if we understand him aright. He observes, "that the assemblage of symptoms of gastric disturbance, which has been enumerated in connexion with cases of softening of the stomach, is not to be considered as dependent on that derangement of structure, but that the symptoms mark the morbid condition of the stomach in which its vitiated secretion possesses a morbid intensity of solvent power." (p. 309.) The existence, therefore, of the morbid condition is recognized in everything but the production of the softened state of the tissue, it being assumed—for there is no evidence of the fact—that this disorganization does not actually occur until the vitality has ceased.

In connexion with the pathological states of the stomach, there are also examined some forms of malignant disease; the question as to the existence of a follicular apparatus appertaining to the mucous membrane of this part; morbid changes in the sub-mucous tissue, and those seated in the contractile or fibrous coat. Some valuable observations, with cases and experiments upon animals are added, illustrative of the effects of corrosive and irritant poisons upon the inner coat of the stomach, and other parts of the alimentary canal. An important remark, which, from its bearing upon medico-legal enquiries, we must not pass over, is, that where an intense active agent (boiling water and sulphuric acid are especially referred to) has been swallowed or forced into the stomach, it is, as it were, discharged against that part of the internal surface of the stomach which is immediately opposite the opening; and that upon this spot an almost instantaneous change is produced, which is deeper and more intense than that which is afterwards produced in other parts of the mucous membrane, when the agent is diffused over them, lowered in its activity by the mucus, which is rapidly secreted, and which does not merely dilute the noxious agent, but, in some degree, protects the membrane. This spot is that portion of the great curvature situated immediately opposite the œsophagus, whereas, in other cases, the most intense degree of injection is usually met with at the cardiac extremity; when, therefore, this part of the stomach is affected rather than that which is the more common seat of the appearances of inflammation, it may, as the author observes, lead us to the suspicion that some fluid, capable of producing an immediate effect, has been swallowed. (pp. 343-4.)

In the account of the mechanical injuries to which the stomach is liable, we find a case alluded to in which mischief appears to have been done by the incautious use of the stomach-pump. This instrument, it

seems, had been employed to remove some deleterious fluid from the stomach of a man in a state of stupor. No relief was experienced, and after the patient's death a few small ecchymosed spots were discovered on the internal surface of the stomach, which corresponded so completely with the end of the instrument as to leave no doubt of their having originated from that source. There is no reason to suppose, it is added, that the fatal result was in any way connected with this injury; but it suggests a useful caution, which those who have occasion to introduce instruments of any description into the stomach will do well to profit by.

The distinction of the three portions of the duodenum, as recognized by Dr. Hodgkin, is well marked by the difference of structure and function by which they are severally characterized, and affords a useful guide in the examination of the lesions of this part of the intestinal tube. Before entering upon the more particular details, certain general deviations from the normal condition, such as the deficiency and excess, dilatation, variations in colour, &c., to which we need not here refer, are pointed out. It should, however, be borne in mind, that, in respect of colour, the mucous membrane of the duodenum, as observed by Billard, certainly differs from that of the stomach, the former being more commonly of an ash colour, not whitish, nor tinged with red, like the latter, excepting during the process of digestion, when, like the stomach, it has more or less of a roseate hue. The first division of the duodenum, the pylori-valvular space of Billard, is that portion which extends from the pylorus to the commencement of the valvulæ conniventes, and is usually about an inch and a half in length. It is in this situation that the solitary glands of Brunner are found in greatest abundance, the whole surface of the membrane being studded with them. The most important morbid alteration discovered here is ulceration, the ulcers being well defined, and of a roundish figure. The ulceration is sometimes connected with tubercular deposition in the sub-mucous tissue, and more frequently still with malignant disease of the neighbouring parts. In the next or middle portion of the duodenum, the valvulæ conniventes are thickly placed, and the orifices of the biliary and pancreatic ducts are here situated: the villi are also very evident in this part. Injection of the mucous membrane is often remarkably apparent, in cases where irritation has existed on the edges of the valvulæ. Many of the French pathologists, belonging to the school of Broussais, give great importance to inflammation of this portion of the intestinal canal, considering many hepatic disorders to take their rise primarily from this source. Dr. Hodgkin, at the same time that he denies the general inference, admits the occasional dependence of jaundice upon an inflamed state of the duodenum. "I have examined," he says, "the bodies of patients who have died in a state of jaundice, in whom no derangement could be detected in the course of the ducts which contained and allowed the passage of bile. In these cases, it seemed highly probable that the impediment had existed at the mouth of the ducts towards the intestine, which is also naturally the narrowest part of the passage. The manifest relief at times afforded by means which act gently on the alimentary canal seems also to favour the idea that the cause of obstruction and the application of the remedy are both to be referred to the duodenal extremity of the ducts." (p. 376.) We may observe that when the difficulty which is not unfrequently found in the discovery of the

orifice of the ducts in the healthy state is taken into consideration, it will readily be perceived that a very slight degree of thickening of the membrane, such as must occur in a congested state of the capillaries, may easily be a cause of obstruction, and consequently of jaundice. But however this may be, the practice founded upon the idea that jaundice is dependent upon inflammation of the duodenum will be often found remarkably efficacious. We have often had occasion to see jaundice, in which examination of the region of the liver has afforded no evidence of disease of that organ, give way rapidly to the application of a few leeches to the right of the epigastrium after the use of other remedies had produced little effect; and we cannot but consider that in these cases the relief was probably owing to the removal of an obstruction existing near the opening of the ducts, and dependent upon either an inflamed or congested state of the duodenum. With the exception of such a condition of the lining membrane, this portion of the intestine does not seem to be subject to morbid alterations; we may, however, remark that, although Dr. Hodgkin has himself never heard of strictures existing here, a case is referred to by Dr. Abercrombie (whose work on Diseases of the Abdominal Viscera, by the way, does not seem to have received so much attention from our author as it deserves), in which this lesion was observed, and another is mentioned by Dr. Peebles among the cases of dilatation of the stomach before noticed. The remarks on the biliary ducts, gall-bladder, and pancreatic duct, we are compelled to pass over. The last portion of the duodenum much resembles the jejunum, differing chiefly in its movements, being more restrained, and its valvulæ conniventes and glands being more numerous. The alterations of structure which it presents are either of little importance or resemble those which are found in the jejunum. This portion of the duodenum, therefore, the jejunum, and the upper part of the ileum, have much in common in the general characters of the lesions to which they are respectively subject; these consist in variations in colour, altered secretions, preternatural distension and contraction, inflammatory and congestive injection of the capillaries, thickening, and ulceration: which last, however, is rare in the jejunum, and mostly found in that situation in connexion with tubercular deposition in the submucous tissue.

Intussusception, or the invagination of one portion of the bowel within another, is also a frequent occurrence met with in the small intestines. Of this state, Dr. Hodgkin distinguishes two forms; the first constituting a very serious affection, which is attended with obstruction, vomiting, and all the symptoms of strangulated hernia, is most commonly fatal in its termination, the mucous membrane being found highly vascular and of a livid colour; the other is a cadaveric change, probably, as the author suggests, taking place in *articulo mortis*, and the result of the powerful peristaltic action not unfrequently observed just before death. It is to be distinguished by the absence of all marks of inflammation, the contained portion being a little contracted, and perhaps somewhat paler than the adjoining part. (p. 400.)

In connexion with this part of the intestines, the author enters upon an account of the muciparous glands, and the morbid processes to which they are liable. His descriptions are confined to the solitary glands of Brunner and the aggregate glands of Peyer, the glands of Lieberkuhn

and the distinction of those of Brunner into two species, being unknown to him at the time the lecture in which they are noticed was delivered. An account of the admirable researches of Dr. Boehm, to whom we owe a more definite and accurate knowlege of the glandular structures appertaining to the gastro-intestinal mucous membrane, than had previously been acquired, will be found in our Second Number. (Br and For. Med. Rev., vol. I. p. 521.) In the healthy state the solitary glands are small and inconspicuous; sometimes, however, more distinct, especially among children: when diseased they present deviations from the normal structure of much importance; these are described under the heads of increased development or hypertrophy, acute inflammation, and chronic inflammation. The acute inflammation often runs into ulceration, the ulcers being generally small, well-defined, and of a more or less circular figure: many of the instances of perforation of the intestines have arisen from this source. The chronic inflammation of these glands is most commonly observed in connexion with tuberculous deposit, and is also very liable to run into ulceration, being a frequent occurrence in tubercular phthisis. The changes induced by disease in the aggregate glands of Peyer are similar to those which occur in the solitary glands, but differ somewhat in the appearances to which they give rise in consequence of their greater extent and peculiarity of structure. In the acute inflammation of these glands, as in that of the solitary glands, Dr. Hodgkin recognizes two forms: in the first of which he describes the thickness of the patches as being comparatively little increased, while they are reddened by minute and more or less intense injection, in which the surrounding mucous membrane frequently participates. (p. 432.) In the second form there is considerable increase of thickness, the surface becomes uneven, and, as it were, quilted, ulcerated, and of a yellowish or dirty olive colour. In the first form ulceration may also occur, though perhaps more partially than when there is much thickening of the glandular structure. The chronic inflammation and ulceration of the agminated glands is, like the corresponding affections of the glands of Brunner, usually the result of tuberculous deposit.

Dr. Hodgkin has reserved the last portion of the small intestines for separate consideration, on account of the great importance which attaches to the pathological appearances connected with inflammation of this part. The character of these appearances, whether in the primary steps of the inflammatory process or in the ulcerative action which succeeds, presents little if any difference from what has been before noticed as occurring in other parts of the small intestines; but, in consequence of the greater development of the glandular apparatus in the inferior portion of the ileum, and perhaps also from other causes connected with the situation of this part of the bowel, there would seem to be a greater disposition both to inflammation and ulceration here than elsewhere. It is here, therefore, as the author remarks, that the alteration of structure is most frequently met with; it is here, also, that it is met with in its greatest intensity, and when inflammation has affected the glands higher up in the canal, it appears to have been by extension from this part rather than from their having been the primary seat of the derangement. (p. 459.) Our readers are well aware of the views of M. Louis respecting the connexion of fever with a diseased state of the glandular apparatus of this

part of the intestinal canal. From his numerous and minute investigations, he has arrived at the conclusion that the acute forms of inflammation and ulceration of these glands are confined to one other disease besides fever, the epidemic cholera, and that the chronic affection is never found, except as connected with the existence of tubercle in the lungs. The application of the numerical test has enabled this faithful and accurate observer to ascertain these facts, the correctness of which, as far as the circumstances of time and situation under which the observations have been carried on are concerned, cannot be questioned. But before we are entitled to extend the generalization to other localities and to other times, it is necessary that researches, conducted with the like patient perseverance and upon an extensive scale, should be entered into, and the results carefully noted and compared. The connexion of the red lenticular patches of the skin with diseases of the glands of Peyer, and consequently with fever, which has been pointed out by the same author, although, in fact, open to observation, has not been so generally recognized as we should have been inclined to anticipate. This eruptive appearance has been noticed by several persons in the fevers of this country, but it has been sought for by others without success; and we find Dr. Hodgkin himself, whose diligence and competence as an observer no one will question, saying, "I have seen—what I imagine all my medical brethren have seen—somewhat of a livid flush on the bodies of fever patients; but I must confess that I had no idea that cutaneous discoloration was either so frequent or so uniform in its character as for it to merit being placed amongst the peculiar symptoms of continued fever; nor am I yet practically acquainted with the appearance alluded to, which I must attribute, in part, to my not having sought for it with sufficient care and perseverance." (p. 461.) With regard to the diseased state of the intestinal glands we find him subsequently making the following remarks: "Although, in my own experience, the derangement of the aggregate glands has certainly been detected in most of the fatal cases of fever which I have examined, I am persuaded that I have examined cases which, during life, had presented the characteristic symptoms of fever, in which the aggregate glands, so far from being severely deranged, have been barely discernible, whilst the cerebral or thoracic derangements have been very considerable. In this assertion I am supported by Dr. Southwood Smith, who has long enjoyed most favorable opportunities for investigating the fevers of the inhabitants of London and its vicinity. Our observations have been made upon the same class of patients, yet our conclusions have been perfectly independent of each other, as I had not until lately consulted his observations." (p. 481.) We feel convinced that such, also, is the general experience of most of those who enjoy the fullest opportunities of becoming acquainted with fever, as it occurs in this country. On the other hand, we believe that a decided predisposition to a diseased state of the mucous surfaces, and especially of the gastro-intestinal mucous membrane, exists in the French capital; and we are inclined to attribute much of the inefficiency of French practice, as well as some of the phenomena presented by disease in that country, to this predisposition.

Dr. Hodgkin, although he admits the frequency of a diseased condition of the glands of Peyer in fever, rejects the idea that it has anything

to do with the causation of that disease. In his twenty-third lecture he propounds some opinions of his own regarding the nature of fever, whether it be produced by the influence of some local inflammation or lesion, or exist by itself, independently of such exciting cause. He imagines fever "to depend on the suspension, or, at least, very considerable interruption of that process, by which, during health, the various parts of the system are continually undergoing a change: the old materials being removed, whilst others are substituted in their place;" or, as it is otherwise expressed in the margin, "on the suspension of the universal molecular changes." (p. 490.) We cannot now enter into the examination of these opinions, nor of the arguments by which they are supported; and we are the less disposed to do so, since they are foreign to the general objects of the treatise. We should ourselves have preferred to see the subject of the intestinal mucous membrane completed in this volume, which we conceive might have been readily accomplished without detriment to the arrangement pursued. Dr. Hodgkin's views are, however, always distinguished by good sense, and the practical information which he gives is always sound, and there is no part of the work we have been analyzing which will not well repay an attentive perusal. It is needless to say that we cordially recommend both this and the preceding volume on the serous membranes: they must always rank among the standard records of medical science, and neither the student nor the experienced practitioner ought to be without them.

Art. VII.

Second Annual Report of the Registrar-General of Births, Deaths, and Marriages in England. Presented to both Houses of Parliament, by command of her Majesty.—London, 1840. Folio, pp. 165. 8vo, pp. 247.

We hail with satisfaction this Second Report from the Registrar-general. It comprises returns of births, deaths, and marriages, registered for the year commencing July 1, 1838, and terminating June 30, 1839, with copious illustrative tables. It is accompanied, as was the First Report, with an Appendix from Mr. Farr, which in the present instance contains remarks on the mortality and diseases of 1838, various tables on their relative prevalence in town and country, remarks on the influence of localities, pursuits, and the various circumstances comprised in the word hygiène on such prevalence, and valuable information on the progress of epidemics. We shall endeavour to present a condensed view of the contents of the Report and its Appendix.

The numbers registered in the year ending June 30, 1839, were births 410,540, deaths 331,007, and marriages 121,083, being an increase in the first and third classes, and a decrease in the second class, compared with the preceding year. The increase in the return of births is ascribed by the registrar-general, to a diffusion of the knowledge of the beneficial tendency of registration, whilst he considers that there was a deficiency in the marriages of the preceding year, in consequence of many, from a misapprehension of the object and effect of the recent act, having been solemnized immediately before the act came into operation.

ase in the reports of deaths is not supposed to be dependent ect registration, or any adventitious circumstance, but really e having been fewer to report, Mr. Lister, considering that lity during the preceding year was above the average, owing lemency of the weather in the beginning of 1838, and to some which have subsequently declined in prevalence and severity. : informed that at present it is impossible to do more than ite to a solution of the important question, what is the propor- e mortality to the population of England and Wales? The s his approximation: The population of the country, calcu- n the census of 1821 and 1831, according to principles ut in the first report and explained in our review of that , was, January 1, 1838, males 7,612,967, females 7,828,768, 41,734; and January 1, 1839, males 7,723,924, females , total 15,666,800. The deaths registered during the same ere, for the year ending June 30, 1838, males 170,965, 54,991, total 335,956; for the year ending June 30, 1839, ,112, females 161,895, total 331,007. This shows the mor- have been in 1837-8, one in 46; and in 1838-9, one in 47·3; ean of the two years to be one in 46·6; and, supposing 2 be a sufficient correction for omissions in the registration, the tality for these two years will have been one in 46.

hanges are made in the arrangement of the abstracts in the port as compared with the first. It will be remembered that report the deaths at each successive year of life were exhi- in the present one the deaths during the first year are divided eriods; during the four following years, they are shown for ate year, and after that for quinquennial periods.

sons for the minute subdivision of the first year are the num- ths which then took place, as was shown in the former report, y were found to form more than a fifth of the whole mortality gdom, and the rapid change in the expectation of life that rs. After the first year the ratio of mortality rapidly declines s shown by the enumeration of deaths for the four following he registers do not admit of a subdivision of these years, and lo so, it would appear to us needlessly minute to follow them. option of the quinquennial period for the succeeding stages of siderable source of inaccuracy is avoided, or at least its effect hed. The source to which allusion is made is the disposition r in round numbers." This is strikingly exemplified in an of ages, extracted from the burial registers of England and nd published in the population abstract for 1831, in which e is manifested, evidently from this habit, a striking increase ty at each decennial period from thirty to seventy inclusive, with the deaths in the intermediate years. Mr. Lister is of hat no enumeration, either of the living or the dead, more ian for quinquennial periods can ever be made with success, st ever be remembered, that correct tables of mortality, require construction these two series of facts—the numbers living at ges, and the numbers dying at the same ages, and the observed etween those facts. He has adduced high authorities, British

and foreign, for abandoning the attempt to enumerate accurate two classes of facts year by year.

Mr. Farr, in his appendix to the report, bestows his attention the diseases of males and females. He estimates that the mor males was 7 per cent. higher than that of females, and remark is well established that the mean duration of life is longer in than in males. This discrepancy, it is obvious, may arise from larity of habits and occupations, difference of organization, or bo jointly. Mr. Farr considers justly that to refer the difference first set of causes exclusively, would be to take too narrow for the differential mortality is greater in early childhood and birth than in more advanced ages. This throws us for explan a difference in original structure. In what, however, this differer sists we are ignorant, and equally ignorant are we of the cause varying prevalence of certain diseases in the two sexes; as the expresses it, we are not "aware that any anatomical or physi reason has been advanced to explain the singular fact, that 62(and 1828 females died of cancer; 4026 males and 5071 fe hooping-cough; 27,935 males and 31,090 females of consumptio males and 3430 females of hydrocephalus; or, 152 males and 55 of diabetes." It is no explanation certainly, but it may not be thy of remark, that in such of the diseases here mentioned a certain seat, the excess in the case of males is in affections of the organs and of the nervous system, and in that of females in thos organs of respiration: "34,321 males and 33,556 females die epidemic class of diseases; smallpox, measles, croup, thrush, di dysentery; cholera, and influenza proved most fatal to males; h cough to females; typhus, scarlatina, and erysipelas, were scarce fatal to males than to females." There was a small decrease in t epidemic) class, as compared with 1837, viz. from 4·7 per 100(per 1000 of the living. The 67,877 deaths from this class com per cent. of the total specified deaths, whilst in 1837, the proport 23 per cent. Of the 4·5 deaths to the 1000 of the living, 1· from smallpox and 1·3 from typhus. We are convinced that the item will surprise and grieve many; but this is a subject to w shall have subsequent occasion to revert.

Diseases of the nervous system destroyed 49,704 persons, or p about 14 per cent. of the total deaths. Of these deaths 26, classed under the vague head, convulsions; 7612, under that of cephalus; 2.78 are attributed to cephalitis. Regarding hydroce Mr. Farr remarks that, like consumption or mesenteric disease, modification of scrofula, in which view we concur with him, n standing the recently promulgated contrary opinions of Dr. Davi Farr justly remarks that this disease is very fatal in ill-ventilat neighbourhoods, where the inhabitants are poor and the mothers Are these the localities in which acute phlogoses arise? Are t rather the haunts of scrofula? When we have seen hydrocep the abodes of the wealthy, we have ever had reasons (independen disease) for considering the families scrofulous.

Diseases of the nervous system are 23 per cent. more fatal t than females, the difference being chiefly in infantile diseases. A

destroyed more males than females—paralysis more females than males. To chorea (St. Vitus's dance) the deaths of four males and twenty females were ascribed—a smaller number than we should have expected, as we have repeatedly seen this disease fatal, and more frequently so to females than males. To delirium tremens were ascribed the deaths of 167 males and 15 females. We should think from our own observation this statement also, especially as it regards males, considerably below the truth; and there is an obvious motive for inaccuracy, for the term delirium tremens is now familiar among all classes, and by no means in good repute. Deaths took place from tetanus in 100 males and 29 females; but it is justly remarked that men are more exposed in a similar proportion to the injuries from which it generally originates. The conclusion of the whole is that, however certain diseases of this class may have proved more fatal to females than males, the latter suffer more from diseases of the nervous system than the former, in the proportion of 23 per 100.

Diseases of the respiratory organs produced 90,823 deaths, that is, a mortality of 6·0 per 1000; while the annual mortality of the group in 1837 was 5·5 in 1000, or 11 per cent. less. The following is Mr. Farr's account of the variations in this class of diseases, occasioning the difference in favour of the last year. The mortality of consumption fell from 3·96 to 3·93 in 1000; that of pneumonia, bronchitis, and pleurisy, rose from 0·93 to 1·38 (69 per cent.); asthma from 2·5 to 3·8 (52 per cent.); 3·8 in 1000 males and 4·1 in 1000 females died of *consumption;* 11,691 males and 9488 females died of inflammatory affections of the throat, larynx, air-tubes, lungs, and pleura. He adds that consumption is 8 per cent. more fatal to females than to males. He specifies some errors in the registers for which allowance must be made, such as many cases (1218) referred to hemorrhage and registered ruptured blood-vessel, which he considers to belong to consumption. In this view we fully concur with him, having in every instance which we have had an opportunity of examining found the disease thus designated to consist of hemorrhage into a tubercular cavity. With these qualifications, 27½ per cent. of the total deaths were due to diseases of the respiratory system, and 18 per cent. to consumptions; namely, 16·0 per cent. of the deaths of males and 19·2 of the deaths of females.

These statements correspond very exactly with those of Mr. Farr for the former year, and which we transferred to our review of his Letter. In that document as in this there was shown an excess in the inflammatory class of diseases of the respiratory organs in males, but an excess of phthisis in the other sex brought the deaths from the whole class to an equality in the two. In the present Report we find the general equality not perfect, there being a slight excess in the deaths of females on the total mortality of the class, the proportion being 271 males to 278 females, owing to the preponderance of phthisis in the latter sex. On this subject Mr. Farr makes the following very important remarks:

"The higher mortality of English women by consumption may be ascribed partly to the in-door life which they lead, and partly to the compression preventing the expansion of the chest by costume. In both ways they are deprived of free draughts of vital air, and the altered blood deposits tubercular matter with a fatal, unnatural facility. 31,090 English women died in one year of the incurable malady! Will not this impressive fact induce persons of rank and

influence to set their countrywomen right in the article of dress, and lead the to abandon a practice which disfigures the body, strangles the chest, produ nervous or other disorders, and has an unquestionable tendency to implant a incurable hectic malady in the frame? Girls have no more need of artifici bones and bandages than boys." (Letter to the Registrar-General, p. 73.)

We entirely concur in the reprobation of this unhealthy and disfiguri practice; but the other faulty arrangement pointed out by Mr. Farr i equally deserving of censure. The daughters of the poorer class a "deprived of free draughts of vital air," and, what we think equally im portant, of that free exercise, without which the circulation is langui and the blood vitiated, when as dress-makers they are confined all da and a portion of the night in crowded and noisome appartments, minis tering to the vanity and, by the tight garments they fashion, to the de terioration of the health of the upper class. Of the daughters of thi latter class the health is sacrificed to those eternal *accomplishments* which, with small benefit to the intellect and none to the feelings, inevi tably dwarf and dwindle the body, and too often lay the beauteou fabric in the dust. Many hours to music, many to drawing, many t fancy-work, some to languages, and few—but very few—to exercis in the open air "wear through the longest day." When exercise i taken it is often in some public walk, under the superintendence of som silly and ignorant governess, by whom every ebullition natural and health ful to youth—the jocund laugh, the run and the leap—are repressed a ungenteel—that stupid and vulgar word to which so much of the healt and happiness of youth is sacrificed. In an education calculated t draw forth the powers of the mind and body all should be vigorou done. But this is overlooked in female education. During the lo hours of lessons in drawing, music, &c., the attention becomes langui the mind weary, objects impress it feebly, and much of the time that i thus literally wasted would be infinitely much more usefully spent i play. A shorter period of vigorous study and a longer one of bodil exertion of a different kind from what we have witnessed and endea voured to describe, would send young ladies forth to the world from the homes or seminaries of education at once better instructed and mor healthful than we now see them.

1·205 per cent. of the total deaths in males, and ·945 per cent. of thos in females, or 2032 of the former, and 1530 of the latter, are registere as having died of diseases of the heart and blood-vessels. This Mr. Fa remarks, and with justice, is below the true number; and he ascrib the deficiency to the art of auscultation not being sufficiently diffuse Much may be justly ascribed to this; but something, too, may be attr buted, in the numerous cases in which death occurs suddenly, to th mode in which inquests are conducted. The coroner and the jury lea enough to show that the death has not been unnatural or violent: wit this they are satisfied, and the customary but unmeaning verdict, "Die by the visitation of God," is returned: having no evidence of th actual pathological cause of death, they do not direct the only means revealing it—an anatomical examination. Aneurism destroyed thr times as many males (88) as females (31), the same proportion as w observed in 1837. Diseases of the digestive organs were less fatal th in the latter end of 1837, the mortality having declined from 1·4 to 1

per 1000, or, including thrush, diarrhœa, dysentery, and cholera, rom 2·07 to 1·59 per 1000. The proportions in the two sexes were i·966 per cent. males, and 5·709 per cent. females of the total deaths. There was an increase in the deaths from teething. Stricture of the intestines was, as might be supposed from its being very generally of a ancerous nature, more frequent in females than males. Of hernia there ied 318 males and 189 females; of peritonitis, 51 males and 117 females, proportion of the latter cases probably puerperal. Inclusive of teething, 10,992 persons died of diseases of the stomach and bowels; 3 of iseases of the pancreas; 3880 of diseases of the liver (including jaundice, 41); and 27 of diseases of the spleen. The 1385 classed under "disease of the intestinal canal," comprised cases of chronic enteritis, gastritis, dyspepsia, as well as some malignant diseases.

"1338 males and 313 females died of diseases of the urinary organs. The ortality of the former from stone and gravel was 4 in 100,000; of the latter, ·5. The difference in the seven heads is exaggerated by, but cannot be exclusively ttributed to, mechanical causes." (Letter to the Registrar-General, p. 73.)

The heads mentioned in this Report are, nephritis, ischuria, diabetes, ystitis, stone, stricture, and disease. Now under every one of these eads we find more cases in the male than the female subject, and some f the diseases appear to owe no aggravation to mechanical causes. We nd, for instance, under that of nephritis 113 cases occurred in males, nd only 44 in fèmales; of diabetes 152 in males, and 55 in females; f cystitis 103 in males, and 25 in females; and under the general term isease 578 in males, and 132 in females; so that there can be little oubt of the greater tendency to diseases of the urinary organs in males uite independent of any mechanical cause. So far indeed as these very aluable reports have proceeded they have manifested a greater tendency) diseases of the nervous system and the urinary organs in the male, nd of the organs of respiration in the female sex; whilst among diseases f uncertain seat, they have shown a very decided predominance of caner in females.

The diseases of *uncertain seat* proved fatal to 21,871 males and 22,361 emales. Of this class dropsy was observed in 5170 males and 7172 emales; hemorrhage in 730 males, 488 females; mortification in 802 nales and 541 females; malformation in 93 males and 73 females; crofula in 599 males, and 520 females; and carcinoma in 620 males nd 1828 females. Purpura, formerly referred to diseases of the skin, vhere it was manifestly misplaced, is now very appropriately transferred o this class. Of this singular disease there died 31 males and 27 females. Mr. Farr remarks respecting it that it appears to consist in an alteration f blood, in which view he is probably correct. But we question whether e alteration be of one kind in all cases, having certainly seen the disse in two different if not opposite conditions, the one having relation active hemorrhage and requiring depletion for its cure, the other being the nature of passive hemorrhage and requiring an opposite management. The deaths from hemorrhage were 730 males, 488 females, being the proportion of 1·5 of the former to one of the latter; but we have ready coincided in a suspicion expressed by Mr. Farr, that many of ese cases belong to consumption. The united deaths from all forms of

dropsy (hydrocephalus included, which belongs to this disease only in name) amount to, males 10,734, females 11,694—total 22,428, showing the same predominance of dropsy in the female sex which was shown in last year's report. Of sudden deaths 1840 occurred in males, and 1172 in females, being in the proportion (including violent deaths, which are generally sudden) of 10 females to 18 males, the former having so much less chance of sudden death: 12 per cent. of the deaths of females, and 10 per cent. of the deaths of males were ascribed to old age and to natural decay.

The deaths from manifest external causes are arranged under three heads, viz. intemperance, starvation, and violent deaths. To the first head were ascribed the deaths of 195 males and 36 females. From starvation by cold or want there arose the deaths of 126 males and 41 females; and the violent deaths were 8359 males and 3368 females. These may again be divided into voluntary and involuntary. The numbers ascertained of the former class, suicides, were, males 751, females 307—total 1058, and in many cases of individuals found dead the agent was not ascertained. Relative to suicides we are furnished with two tables, which are of interest. According to the one the tendency to this crime increases till the age of 60, the rate of increase from 30 to 60 being 49·6 per cent. every 10 years. From the other table we learn that the tendency to suicide is the highest in the metropolis, and the least in Wales, the deaths in the former locality from this cause being in the ratio of 12·6 per 100,000 inhabitants, and but 2·5 in the latter, whilst the average ratio for England and Wales is 6·8 per 100,000. From a third table we learn that the smallest number of suicides occur in the cold season of the year.

The general summary of the causes of death given by Mr. Farr is, that 36,799 died from inflammations; 85,506 from specific inflammations; 19,122 from the terminations of inflammation; 15,125 from hemorrhages; 2821 from carcinomatous diseases; 60,868 from tuberculous diseases; 2256 from disordered secretions; 2512 from depraved nutrition; 44,773 from disorders of the nervous system; 35,564 from old age; and 11,727 from violent deaths.

The augmented fatality in an urban compared with a rural population received, it may be remembered, considerable attention in the report of last year, and the subject is continued in the present one. The following is Mr. Farr's estimate on a large scale of the relative mortality of city and country—an estimate formed on the same principle as that of table E in the report of last year. In table C then of the present report we are presented with an abstract of the deaths from twelve classes of diseases in city and country districts, the former with a population of 3,726,221, the latter with one of 3,539,908. In the former there perished from epidemic, endemic, and contagious diseases 23,655, in the latter 13,685; from diseases of the nervous system the numbers stood respectively 15,651, and 8177; from those of the respiratory organs 28,973 and 18,508; from those of the organs of circulation 1301 and 712; of the digestive organs 6505 and 3361; of the urinary organs 417 and 373; of the organs of generation 984 and 547; of the organs of locomotion 653 and 354; of the integumentary system 144 and 66; of uncertain seat 10,447 and 10,529; from age 7374 and 8874; violent deaths 3104 and 2516; and from causes not specified 1811 and 2708: forming a total of deaths in the cities of 101,019, and in the country of 70,410.

he annual rate of mortality in the cities was 2·7, in the counties 2·0 per ent.; and the mortality in the cities 1·36 to 1·00 in the counties. The ean duration of life in the two sets of circumstances would differ early in the ratio of 37 years and 50 years.

"In examining the special causes of death," says Mr. Farr, "three classes may e distinguished: one class which was exaggerated in cities to the highest itch, a third class in which the mortality was nearly the same, or in excess in e counties, and an intermediate class. To 1 00 death in the counties, the eaths out of the same amount in the cities, were by asthma, 3·80; erysipelas, ·71; convulsions and teething, 2·57; cephalitis and hydrocephalus, 2·41: ydrophobia, 2·37; pneumonia, bronchitis, and pleurisy, 1·99; delirium treens, 1·98; typhus, 1·88; smallpox, 1·73; heart-disease, 1·73; child-birth, ·63; syphilis, 1·59; rheumatism, 1·58; gout, 1·55; hernia, 1·48; purpura, ·46; sudden deaths, 1·45; liver disease, 1·45; hepatitis, 1·35; tetanus, 1·32. he excess of mortality in cities, was less in the following cases: by consumpon, 1·24; croup, 1·23; violent deaths, 1·17; stone, 1·11; mortification, 1·10; alformations, 1·07; apoplexy, 1·07; hemorrhage, 1·02. The mortality by e third class of causes, was greater in the counties than in the cities; for the ortality to 1·00 in the counties was in the cities, by paralysis, ·99; dropsy, 9; jaundice, ·99; diabetes, ·97; cancer, ·92; hydrothorax, ·88; hematemes, ·79; debility (frequently premature birth), ·75; atrophy, ·75; scrofula, 6." (Letter to the Registrar-General, p. 81.)

In reference to these comparative numbers, whether in general or in articular, it should be borne in mind that the argument is stated in a rm by no means favorable to a rural population, for in the districts omprising the population of 3,539,908, and furnishing the mortality of 0,410, several cities are included. The excess of mortality then in ties as now constructed, compared with districts strictly rural, is greater an is shown by these returns. Mr. Farr asks the very reasonable uestion; "is the excessive mortality of cities inevitable?" In our review f the first report, we ventured in a similar spirit to remark that the cts relative to the mortality of rural districts and towns "were deduced om a comparison between rural districts and English towns, the growth of progressive civilization, and that the older parts of them, now chiefly habited by the labouring poor, were formed in the early stages of ivilization. We should like much to see a comparison between the ortality of country districts, and large towns brought rapidly into existnce, under a civilization fully developed—those of the United States r instance." Mr. Farr answers his own question by remarking that e aggregation of mankind in towns is not inevitably disastrous: and at health and life may be preserved in a dense population, provided e density be not carried beyond certain limits. He produces abundant idence from metropolitan districts of the truth of the first of these ropositions. With regard to the second we would remark, besides at "*certain* limits" like other certain things, seem to us the most *certain* possible, that nothing has impressed us more on examining e very valuable tables descriptive of the mortality in the metropolitan stricts, than the want of connexion they display between density of pulation and shortness of life. On referring to table G, one of sinlar value, we find, for instance, that the metropolitan district of ermondsey, with a space of 88 square yards to one person, has an annual ortality of 3·070 per cent.: Shoreditch, with 35 yards to one person, one

is density appears to manifest its effects chiefly in the production of ee classes of diseases, the epidemic class, affections of the respiratory ;ans, and of the nervous system. The metropolis has a population of ,903 persons to one square mile, whilst the average of England and ales, is 269 per square mile. The metropolis loses ·742 per cent. by demic, endemic, and contagious diseases, and 219 per cent. by typhus, ilst the average deaths throughout the country are by the entire class i2 per cent. and by typhus, ·125. Cheshire and Lancashire, with a pulation of·701 per square mile, mnch below the metropolis indeed, : next to that the most densely-peopled district in England, loses by : whole epidemic class, ·490 per cent., and by typhus, ·150. Thus it :ms that the most densely-populated districts have suffered most by : epidemic class; London having no rival in this respect, and Cheshire i Lancashire exceeding all the more thinly-peopled districts, with : exception of Monmouth and Wales, which district with a population 126 per square mile loses ·630 per cent. by the whole epidemic class, d by smallpox, ·202 per cent., scarlatina, ·123 per cent., and by ty-us, ·152 per cent. It is evidently true, as remarked by Mr. Farr, that : ordinary laws of mortality are at present disturbed in Wales, by the lux of workmen within the mining districts. The population sud-nly collected is exposed to all the evils of dense districts, without the eviations which spring up in towns of slow growth. The overt Chartist :esses, and the more lurking, but still close political organization which know, pervades the district, indicate its unhealthy social condition, d, at the same time, aggravate the source whene they spring. Ex-isive, however, of the anomaly presented by Wales, the increase of mor-ity in a dense population from the epidemic class of diseases, is mani-ted in this table.

This pernicious influence of a dense population on diseases of the re-iratory organs appears to be unequivocally evinced by the same table. e average deaths throughout the country, from all diseases of the re-iratory organs, are ·605 per cent. and from phthisis ·393 per cent., but London, the deaths from diseases of the respiratory organs generally, : ·770 per cent., and from phthisis, ·414 per cent., and in Cheshire and ncashire, they are from the former ·783 per cent., and from the latter,)9 per cent., without there being an excess above the average in any ner district worthy of mention. Again, diseases of the nervous system oduce an annual average mortality of ·332 per cent., and the only excess yond this is in the districts already so often referred to, the metropolis senting a mortality from this source, of ·437 per cent., Cheshire and ncashire, one of ·461 per cent.

Considering the mortality from these three sources in these the most nsely-peopled districts in this country, we are disposed to attribute a od deal to density of population, especially in the epidemic class of sease; but the excess in the manufacturing districts is so great in dis-ses of the nervous and respiratory systems, especially in the latter, that ien we consider, moreover, the relative population of the two districts, : are forced to acknowledge that some other instrument of destruction sides density of population is in action, and we naturally look to the se and foul air of cotton mills.

In some remarks on the influence of the seasons, supported, as his re-

marks always are, by reference to tables, the reporter shows that the well-known statement of Celsus on the same subject, "Saluberrimum ver est, proxime deinde ab hoc hiems, periculosior æstas, autumnus longé periculosissimus," is inapplicable to *our* seasons at present. The following return of deaths in the metropolis expresses, we believe, very correctly, the order of insalubrity throughout England. There were registered in the winter quarter of 1838, 15,611 deaths; in spring, 13,109; in summer, 11,397; and in autumn, 12,581. This order Mr. Farr considers to have prevailed in England ever since the beginning of the last century. Prior to this period, however, the order of insalubrity was the same in this country as that indicated by Celsus; the autumn, at least, was "longé periculosissimus." We believe the explanation of the extreme insalubrity of autumn in this country formerly, and in many parts of the world at present, to be, that it is the season when intermittent fever, and especially those forms of it to which we give the name of remittent, but which elsewhere are called pernicious, pestilential, and malignant intermittents, are most prevalent. The febres algidæ of Torti, and some of the worst forms of those described by Morton—diseases bearing, in many respects, no slight resemblance to Indian cholera—committed their ravages principally in autumn. Under the influence of civilization and cultivation, countries become much more healthy; but the first and greatest amelioration is manifested in the extinction of diseases originating in malaria, which was most widely diffused and malignant in autumn, and still is so where it exists, as many parts of Europe and some of our own colonies can testify. England itself, however, may be considered as civilized and cultivated beyond the malarious point, and hence our autumns have ceased to be the insalubrious seasons of the year, the preeminence in this respect being conceded to our rigorous winters.

Many of the causes of disease, remarks Mr. Farr, act with equal force from year to year; others regulated by the seasons, increase or decrease with the temperature. Epidemics, however, follow laws of their own, and, in this class, the greatest variety may be expected in these annual reports: they have been a fertile source of discussion. How far registration may throw light on their origin is doubtful, but it seems well suited to illustrate, at least, the laws of their propagation.

The smallpox epidemic is investigated at considerable length in the report throughout its prevalence, from July 1st, 1837, to December 31st, 1839; a period comprising ten quarters, and two winters, two springs, three summers, and three autumns. It was composed of a succession of smaller epidemics, and whether the commencement or the acmé be considered, it is evident that it was not influenced by such circumstances as temperature or change of seasons; for, at the time it was beginning in one district, it was at its height or was declining in another place apparently in the same circumstances. This is manifested by Mr. Farr's tables, and especially by tables (*m*) and (*n*), which will be found at page 92 of his letter.

The total deaths from the epidemic were 30,879, which occurred in the following numbers in the successive quarters: in the first quarter, comprising the months of July, August, and September, 1837, they were, 2543; in the second, 3289; in the third, 4242; in the fourth, 4489; in the fifth, 3685; in the sixth, 3851; in the seventh, 2982; in the eighth, 2505;

in the ninth, 1533; and in the tenth, 1730: total, 30,789. The annual rate of mortality from the disease was 0·8 in 1000; in the metropolis it was 1·1, and in Monmouthshire and Wales 1·2 per 1000. From a careful survey of the very ample table (Table P, pp. 174-195), the epidemic appears to have visited the whole of England and Wales, but by no means occupying successive points: there are no indications of its migrating from district to district on a tour through the country. We find it throughout the whole of the ten quarters in the metropolis on the south-east, and in Sunderland, Tynemouth, Newcastle, and Berwick-on-Tweed in the north-east. It is observed, too, during the same period in Bath and and in Wales on the south-west, and in Carlisle and various other places in Cumberland on the north-west, whilst almost every supposable intermediate point is similarly affected. Mr. Farr appears to express the view which we certainly take, that these tables furnish no reasonable grounds for supposing that this epidemic was communicated from place to place. Adopting the illustration furnished by Liverpool and Manchester, between which two towns the intercourse is perhaps more intimate than between any towns in Europe, he remarks that isolated cases of smallpox existed all the while in Manchester; the seeds of the epidemic were there, and would not the causes which generated the epidemic in Liverpool, the place first attacked, have led to the same result in Manchester? "At any rate, the evolution of the epidemic in Liverpool could not be traced to external contagion, and the problem remains for solution—Why did the deaths from smallpox rise so rapidly, that at last 418 individuals perished in three months, while the ordinary mortality in Liverpool and West Derby from smallpox is 27 in three months?"

This vexed question the reporter very judiciously leaves undisturbed by further vexation, and confines himself to tracing the law of the diffusion of the epidemic; it increased up to the fourth of the ten periods into which the epidemic is divided, and the rate of increase was 30 per cent., or 1·30. This is shown in the first three successive numbers, 2513, 3829, and 4242. From the third to the fourth number (4489), the increase is only six per cent., or 1·06; it then remains stationary, as Mr. Farr describes it, like a projectile at the summit of the curve which it is destined to describe.

To calculate the rate of decrease, Mr. Farr takes the mean of the third and fourth numbers (namely 4365), and, calculating from this, finds the decrease to have proceeded at an accelerated rate, the rate of acceleration being 1·406, and the rates of decrease standing thus, 1·052, 1·101, 1·152, 1·205, 1·268, and 1·138; or, in other terms, 5, 10, 15, 20, 26, and 32 per cent.: the last seven numbers divided each by the corresponding number marking the rate of decrease, presents in the number next below, within an insignificant fraction, the quotient which should result arithmetically from such a division.

Finding it exceedingly interesting to be presented with definite proportions, where all has hitherto been vague, we shall proceed to show Mr. Farr's calculations of the same epidemic in the metropolis, where the mortality was greater than in all other parts of England. The deaths in London in the ten successive periods were, 257, 506, 753, 1145, 1061, 858, 364, 117, 65, 60. The rate of increase in the first and second pe-

riods was 1·97, and in the second, third, and fourth periods, it was 1·50. —The rate of decrease is thus stated by Mr. Farr. The mean registered quarterly deaths from the fourth period were 1103, 959, 611, 240, and 91. The calculated series was 1103, 967, 611, 278, 91 ; certainly a remarkable approximation. The number 1103 was the mean of the deaths registered in the fourth and fifth, 959 was the mean of those registered in the fifth and sixth periods, and the other numbers were obtained in the same manner. The first rate (the rate of decrease) of the calculated series was 1·14, and the other rates were obtained by multiplying 1·14 four times in succession by 1·39, the constant, or the rate of acceleration.

"The rates," it is added, "vary with the density of the population, the numbers susceptible of attack, the mortality. and accidental circumstances; so that to obtain the mean rates applicable to the whole population, or to any portion of the population, several epidemics should be investigated. It appears probable, however, that the smallpox increases at an accelerated and then a retarded rate; that it declines first at a slightly accelerated, then at a rapidly accelerated, and lastly at a retarded rate, until the disease attains the *minimum* intensity and remains stationary." (Letter to the Registrar-general, p. 97.)

Tables are furnished, presenting a view of the progress of four other epidemics, measles, typhus, hooping-cough, and scarlatina in the metropolis, and an abstract of their contents will be found in table (*q*) in the modest form of a foot-note at the close of Mr. Farr's letter. He remarks that they exhibit the same regularity as smallpox, but considers that the laws which govern their course will be more conveniently discussed when the abstract of the observations has been extended over another year. There perished during the latter half of 1837 and the year 1838 in the metropolis, by measles 1942, by typhus 6011, by hooping-cough 3149, and by scarlatina 1942.

Mr. Farr has laboured this branch of his subject with great effect: it is one to which the present report owes much of its interest, and to which future reports will probably be equally indebted; for whilst sporadic diseases hold nearly the same course annually, and must consequently elicit, if any, similar remarks, it is in the epidemic class that the greatest variety may be expected; and the evolution of the laws of their diffusion will, in all probability, furnish forth the labours of many years. The interest which the reporter feels in this subject has led him to comment favorably on the hypothesis, which, though not originating with either of these writers, has received from Drs. Holland and Henle* much illustration and support—that the material of contagion is composed of organic matter. Supported as this doctrine is by much plausible analogy, we consider, notwithstanding, that we have designated it properly when we have called it an hypothesis, and regarded merely as such we do not object to it, but, on the contrary, highly approve of it, thinking well-devised hypotheses the best stepping-stones to substantial theories—believing the observation required for their comparison with actual phenomena to be the best means of bringing out the whole truth.

We have concluded our analysis of the medical part of this valuable volume, and should have concluded altogether, had not one point of extra-professional information incidentally, as it were, gathered from the

* See British and Foreign Medical Review, No. XVIII., p. 398.

of marriages, struck us as being one of so much interest, that we osed to communicate it to our readers; it is, in fact, the sta- f education among all ranks of the adult population throughout ntry, deduced in a mode which we shall leave Mr. Lister to ex- his own language.

ost every marriage is duly registered, and every register of marriage is y the parties married, those who are able writing their names, and ho are unable, or who write very imperfectly, making their marks; e an enumeration of the instances in which the mark has been made w the proportion among those married who either cannot write at all, very imperfectly." (Report, p. 7.)

Registrar-general remarks that 30 in 1000, or three per cent. of riageable portion of the community are married annually, the therefore whose signatures appear on the marriage registers of a year is sufficiently small to be affected by accidental circum- ; and it cannot safely be asserted that the 33 in 1000, from signatures we would draw an inference respecting the other 970, t happen to consist of more than the proportionate number of un- ed persons. With this caution, then, against drawing a conclu- garding the education of any particular county or district from urns of a single year, we are furnished with the educational re- these returns for the year ending June 30, 1839. In the metro- he proportion per cent. of those who, on marrying, signed with a vas, men 12, women 24, mean 18; in the south-eastern counties, , women 40, mean 36; in the south midland counties, men 43, 53, mean 48; in the eastern counties, men 45, women 52, 18; in the south-western counties, men 31, women 47, mean 38; western counties, men 40, women 54, mean 47; in the north mid- ounties, men 32, women 50, mean 36; in the north-western coun- en 39, women 63, mean 51; in Yorkshire, men 34, women 49, mean the northern counties, men 21, women 42, mean 31; in Mon- shire and Wales, men 48, women 70, mean 59.

main points of the whole case are, that the state of education, at mong the adult population, is most deplorable for a country pro- y the most civilized in the world, for that, in the whole of England ales, out of 121,083 couples married, there were 40,587 men ,959 women who could not write; that the metropolis stands de- y superior to the rest of England in respect of education; that, the metropolis, the north of England (Durham, Northumber- Cumberland, and Westmorland) is superior; that the principal ncy is in Lancashire, Bedfordshire, Monmouthshire, and Wales; at the men are superior to the women throughout the country in oportion of 33 to 49.

high estimate of the merits of this volume is sufficiently manifest he length to which our extracts have extended, and the remarks which they have been interspersed. Perhaps we cannot express inion more decidedly than by saying that the second report has d to the utmost the promise of the first.

ART. VIII.

Organic Chemistry in its application to Agriculture and Physiology. By JUSTUS LIEBIG, Ph. D., Professor of Chemistry in the University of Giessen. Edited from the MSS. of the author by LYON PLAYFAIR, Ph. D.—*London*, 1840. 8vo, pp. 387.

Traité de Chémie organique. Par M. JUSTUS LIEBIG. *Introduction.* —*Paris*, 1840. 8vo, pp. 195.

THE two titles which we have given belong to one and the same book. An edition has also appeared simultaneously in the German language. The idea of writing the work, however, was suggested to the author in England. Professor Liebig visited this country in 1837, and was present at the meeting of the British Association at Liverpool, when he communicated the intelligence of the remarkable discovery made by himself and Wöhler, viz., the formation of allantoin, a constituent of the liquor amnii of the cow, in the laboratory, which constituted the second example of the production by purely chemical means of an animal product. Wöhler had previously formed urea by passing cyanic acid through ammonia, and had thus opened a field of almost unbounded discovery and of vast importance to the physiologist. At the meeting to which we have referred the author, who, although in the prime of life, has already been for some years in possession of a European reputation in connexion with the chemistry of organic matter, was requested, on the motion of Dr. Thomson, of Glasgow, to draw up a report, for the Association, on the state of organic chemistry. Liebig cordially assented to this serious call upon his time. The result is now before us. The object of the author in this elaborate report has not been to present to his readers a catalogue of isolated facts; but he has endeavoured as far as possible to apply the doctrine of organic chemistry so as to explain the apparently mysterious processes of vegetable and animal growth. To plant the seed in the barren soil, to see it sprout into a living thing, to observe it gradually increase in weight and size until it has obtained an altitude perhaps immense, and all without apparent motion or sound, is at once most wonderful and most mysterious. But to analyze the soil in which the seed is sown, to investigate the properties of its varied constituents, the water which falls from the clouds to moisten it, the atmosphere in which it is immersed, and all the elements which act upon its existence is, partly at least, to elevate the veil of mystery and reveal to us the works of the simple and noiseless machinery of nature.

Liebig divides his work into two parts. In the first he discusses the chemical processes in the nutrition of vegetables, and dwells especially upon the secondary causes of nourishment. The second division is devoted to the chemical processes of fermentation, decay, and putrefaction, by means of which he considers nutrition to be supplied from its primary sources, contagions and infections generated both in and out of the human body, and a perpetual succession of changes in the form of matter to be kept up. Matter thus assumes alternately inferior and beautiful forms, in such infinite variety and so worthy of admiration that "even custom cannot stale" them.

Many of the views of the author are so novel and of such extensive application, that it is somewhat difficult to afford a correct analysis of them in a short space. We shall endeavour, however, to take notice of the principal applications of them to theory. We follow in general the French edition, in which the various subjects are more readily found, from more convenient arrangement.

Food of plants. It is obvious from even a superficial survey that plants require food just as animals do, and that the sources of their nourishment must be the soil in which they are planted and the air in which their stems are immersed. Van Helmont had deduced from an experiment on a willow that rain-water alone was sufficient for the support of the plant, because when supplied with this kind of food, to all appearances alone, it increased from 5 to 164 lbs. Bergmann, however, showed that 1 lb. of rain-water contained 1 grain of earthy matter, which would amply suffice for contributing inorganic matter to the plant. Van Helmont also omitted in his calculation the substances absorbed from the atmosphere by the soil, and conveyed into the vessels of the plant; and, finally, though this is not the least important consideration, he forgot the nutritious power of the atmosphere.

When plants are examined by destructive analysis they afford carbon, nitrogen, and oxygen and hydrogen, in the proportions to form water. It will be convenient to consider the sources of each of these separately.

Assimilation of carbon. Every one is familiar with the fact that when we burn a piece of straw at a candle a portion is consumed, and what remains is charred or blackened at the edges. Few consider that this is a process of destructive analysis. The oxygen and hydrogen are driven off, and the carbon or charcoal—the main constituent of the organic substance—remains behind. From whence does the carbon derive its origin? Liebig endeavours to answer this according to the strictest rules of analytic reasoning. Physiologists have been in the habit of viewing the decomposing vegetable matter of soils, or *humus,* as one of the principal sources of the food of plants. This substance they conceive to be absorbed by the roots and thus conveyed into the proper assimilating vessels, where its carbon is abstracted. Humus is described by chemists as a brown matter, dissolving partially in alkalies. The portion which dissolves in alkalies has been termed *humic* acid; the insoluble part *humin.* A careful chemical examination of the substance, however, has demonstrated that it possesses no constant chemical constitution, and that as it is formed in the soil it is merely a vegetable matter in various stages of decay. Liebig, in showing the fallacy of the usual opinion respecting the influence of humus in the nutrition of plants, proves his point by a *reductio ad absurdum.* He admits, for the sake of argument, that humic acid is absorbed by plants in the form of that salt which contains the largest proportion of humic acid, viz., in the form of humate of lime, and then from the known quantity of the alkaline bases contained in the ashes of plants he calculates the amount of humic acid which might be assimilated in this manner.

According to Berthier 1000 lbs. of dry fir wood yielded 4 lbs. of ashes. In every 100 lbs. of these ashes, after subtracting the chloride of potassium and sulphate of potass, there remained 53 lbs. of basic oxide, consisting of salts of potash, soda, lime, magnesia, iron, and manganese.

Now 40,000 square feet, Hessian measure, of woodland yielded annually, according to Heyer, upon an average, 2650 lbs., Hessian, of dry fir-wood, which contain 56 lbs., Hessian, of metallic oxides. But, according to the experience of Malaguti and Sprengel, 1 lb., Hessian, of lime combines chemically with 10·9 lbs., Hessian, of humic acid; 5·6 lbs. of the metallic oxides would accordingly introduce into the trees 61 lbs., Hessian, of humic acid, which, admitting humic acid to contain 58 per cent. of carbon, would correspond to 91 lbs., Hessian, of dry wood. Hence there is only 91 lbs. which can be accounted for as possibly deriving its origin from the humus, and 2559 lbs. which must have proceeded from a different source.

There is another possible source, however, of humus, viz., through the agency of rain-water, which Liebig carefully examines. The quantity of rain which falls at Erfurt is 17½ lbs., Hessian, over every square foot of surface. If the whole of the water were absorbed by the roots of plants saturated with humate of lime, the plants growing on 40,000 square feet and receiving 700,000 lbs. of rain-water, would not take up more than 300 lbs. of humic acid, since one part of humate of lime is soluble in 2500 parts of water. But the amount of land mentioned produces 2580 lbs. of corn, in grain and straw alone. Hence the 300 lbs. of humic acid will not account for the quantity of carbon contained in the roots and leaves alone. Again, 100 parts of straw dried in air contain 38 per cent. of carbon: hence 1780 lbs. of straw contain 676 lbs. of carbon. In 100 parts of corn there are 43 parts of carbon; 800 lbs. must hence contain 344 lbs., that is, altogether, 1020 lbs. of carbon. By comparing these with other facts Liebig concludes that equal surfaces of land adapted for agriculture produce equal quantities of carbon.

The soil being thus repudiated as an element in accounting for the fruitful origin of carbon, we are obliged to recur to the only other possible source from which it can be derived, viz., the atmosphere. The mean of the experiments of Saussure, made at various seasons, leads to the inference that the quantity of carbonic acid in the atmosphere amounts to one thousandth of its weight. Now, as far as can be discovered by the analysis of airs from ancient jars and pyramids, the quantity of oxygen has been constantly the same in the atmosphere for thousands of years. We have no reason to believe that the relative proportion of carbonic acid in the atmosphere has increased during the same period. Hence it is possible that the oxygen stands in some fixed relation to it, that is to say, that two causes exist, one of which prevents the accumulation of carbonic acid by removing it as it is generated, and another which supplies oxygen in place of that which is lost by respiration, putrefaction, and combustion. These two causes are found united, Liebig considers, in the *vital action* of plants. If we take the sprig of a plant and introduce it into water, saturated with carbonic acid, we shall find that the whole of the carbonic acid disappears, and is replaced by oxygen, which may be collected and examined, the latter occupying exactly the same bulk as the former. This is a beautiful provision, as it tends directly to the preservation of a pure atmosphere for animal life. From the circumstance of the oxygen given out by plants being equal in bulk to the carbonic acid taken up, Liebig concludes that the carbonic acid is absorbed by the assimilating organs of the plant, where it is decomposed—the carbon being retained and the oxygen displaced. But it may be asked, is it possible that a body existing

to the amount of only one thousandth part in the atmosphere, can supply the whole of such an essential ingredient of all the living masses of plants and animals which cover the surface of the earth? The simplest use of arithmetic will soon demonstrate the possibility of this apparent paradox. Upon every Hessian square foot of the surface of the earth there rests a column of air, weighing 2216·66lbs., Hessian measure. Now the diameter, and consequently the surface, of the earth being known, the weight of the whole atmosphere may be calculated with the greatest accuracy. The thousandth part of this is carbonic acid, which contains twenty-seven per cent. of carbon. From which it results that the atmosphere contains 3000 billions of Hessian lbs. of carbon—a quantity which is certainly greater than the weight of all the plants, coals, and lignites upon the face of the earth.

It has been observed that during the night plants exhale carbonic acid, and absorb oxygen. The bulk of the latter however is never equal to the former, and hence Liebig concludes that, on the departure of light, the carbonic acid ceases to be decomposed, and being absorbed along with the juices, it is given out by the leaves, in quantity corresponding to that of the water which evaporates. From the absence of a proper explanation of the latter phenomena, some physiologists have considered the theory of the assimilation of carbon from the air as refuted, although the original experimenters had come to the conclusion which has just been developed. The reason of their erroneous inference has been powerfully portrayed by Liebig:

"All the talent and labour of botanists have been expended in reseaches on the structure and external form of plants, without calling into their assistance the resources of chemistry and physics, although calculated to explain the most simple phenomena, and powerfully adapted to discover the truth. The experiments and laws of these two sciences have always been neglected—they have been neglected because they have not been studied. All discoveries in physics and in chemistry, all explanations of chemists, must remain without fruit and be useless, because even to the great leaders in physiology, carbonic acid, ammonia, acids and bases, are sounds without meaning, words without sense, terms of an unknown language which awaken no thoughts and no associations. They treat these sciences like the vulgar, who despise a foreign language in exact proportion to their ignorance of it; since even when they have had some acquaintance with them, they have not understood their spirit and application. Physiologists reject the aid of chemistry, in their enquiries into the secrets of vitality, although it alone could guide them in the true path: they reject chemistry, because in the pursuit of knowledge it destroys the subjects of its investigation, but they forget that the knife of the anatomist must dismember the body and destroy its organs, if an account is to be given of their form, structure, and functions."

Since humus then is not the source of the carbon of plants, for what purpose, it may be asked, are its decomposing elements destined? Liebig answers this question in a manner if not demonstrative, at least highly plausible. Humus, he observes, nourishes plants not by being absorbed and assimilated *per se*, but by presenting to their roots in a decomposing state a slow and continual source of nourishment, in the form of carbonic acid. This source is only beneficial to plants until their leaves are formed, which enable them to assimilate carbon from the atmosphere.

Assimilation of hydrogen. The solid portion of plants or lignin contains carbon and the constituents of water, or the elements of carbonic acid and a certain quantity of hydrogen: 100 parts of carbonic

acid require the addition of 8·04 parts of hydrogen to form lignin, and 72·35 parts of oxygen are evolved in the state of gas, if we suppose that the hydrogen is derived from the decomposition of water—so that, as in the case of the assimilation of carbon, by the decomposition of carbonic acid, we may consider that the assimilation of hydrogen is preceded by the decomposition of water.

Assimilation of nitrogen or azote. We have seen that carbon and hydrogen are not presented in their simple state to the absorbing vessels of plants, but are taken up in union with oxygen; *pari passu*, we might infer that azote would not gain admission in the uncombined form in which it exists in the atmosphere. That the azote is derived from the atmosphere and not from the soil is obvious from the following considerations. Let us conceive a farm properly cultivated, and of sufficient size to be supported on its own resources: a certain quantity of azote will then exist upon it, in the form of men, animals, cattle, corn, fruits, and excrementitious matter; the farm is managed without the addition, from extraneous sources, of azote in any form. The products are annually exchanged for money and other necessaries of life, which contain no azote, but by exporting a certain portion of the corn and cattle, a certain quantity of azote is removed without being replaced. Yet after the lapse of some years the amount of azote has increased. Hence it is evident that plants, and consequently animals, must derive their azote from the atmosphere. Liebig discusses the condition of the azote as it is presented to plants in a very able manner, and has adduced several novel experimental results. Ammonia, he concludes, is the form in which *nutritive azote*, if we may use the expression, exists in the atmosphere. From whence proceeds the immense depôt of ammonia requisite for this purpose?

The ultimate products of the decomposition of animal substance are exhibited in two different forms: in cold or temperate climates, under the form of ammonia; in the torrid zone, under that of nitric acid. However, it has been demonstrated that the formation of nitric acid is constantly preceded by the production of ammonia: the latter is the final product of the putrefaction of animal matter; the nitric acid results from the slow combustion of this product.

A generation of a thousand millions of men is renewed every thirty years; millions of animals perish and are reproduced in a shorter period: what becomes of the azote with which these bodies were so largely endowed? It has been dissipated in the air by the law of gaseous diffusion through the regions of the atmosphere. That ammonia is present in the air is proved by the fact of its presence in rain water: this is easily rendered obvious to the senses by adding a little sulphuric or muriatic acids to a quantity of rain water and evaporating to dryness: these acids combine with the ammonia, and deprive it of its volatility—the residue then contains sal ammoniac or sulphate of ammonia, whose presence may be detected by the aid of bichloride of platinum, and still more readily by the odour developed on the addition of pulverized hydrate of lime: ammonia also exists in snow-water, and in this case, when the odour is evolved, it possesses a strong taint of perspiration and excrementitious matter, which sufficiently points to its animal origin. This investigation has elicited an explanation of a fact, which must have struck those engaged in the minute details of pharmacy; namely, the precipitation produced in distilled water by subace-

lead. According to Liebig, this proceeds from the presence of ate of ammonia: it may be obviated by the addition of a small of alum or sulphuric acid before commencing the distillation: tness of rain-water is also due to the same cause. A portion of monia dissipated in the atmosphere gains admission to the soil by of rain-water, some of this evaporates again with the water, while r portion is absorbed by the roots, and, being assimilated, gives o albumen, gluten, &c.

ig shows that the action of dung is to supply ammonia to the f plants for the assimilation of azote. In Flanders, putrid urine essfully employed as manure: every one knows that the charac- feature in this state of the fluid is the evolution of ammonia: *ino* of Peru, or dung of birds, is valuable from the same cause. luence of lime in the soil is now manifest from the facts detailed. rbonate of ammonia dissolved in the rain-water decomposes the e in the same manner as in the manufactories of sal ammoniac. inous soils owe their value to the property of absorbing ammonia: , which absorbs ammonia most powerfully, is highly useful for the eason.

ganic constituents of Plants. No one can suppose that the nstituents of plants are fortuitous, or that plants contain more an is required for their existence; hence we should expect that would be necessary for vegetation, because all acids which plants are found in the state of neutral or acid salts. Saussure and r analyzed pines from Breven and La Salle, and concluded that t soils exercise an action upon the proportion of the metallic present; but what is curious, and these chemists did not ob- , the saturating power of the oxides is almost identical in the two This explains some important considerations in pharmacy; for f the species of opium contain meconic acid in combination with ely variable quantities of narcotine, codein, morphia, &c.; but ntity of one of these bases always increases as the other dimi- the smallest quantities of morphine are found constantly accom- by a maximum of narcotine; the presence of alkalies, therefore, s necessary for the development of some, and the action of parti- lts is indispensable for the existence of other plants. Some have ed these saline matters to be generated by the vital action of ; but when we are acquainted with the facts, that common salt is ted into the air along with sea-water, and that saltpetre is dissi- by the action of heat without being decomposed, we can at once the fallacy of such deductions. From the previous considerations, therefore be concluded, according to Liebig, that carbonic acid, ia, and water, are the indispensable sources of the food of plants; t for their perfect development, and in order to afford facilities for ration of these elements, the presence of inorganic substances is doubtedly necessary.

ly plausible though the view be which ascribes the source of vege- zote to the decomposition of ammonia, we may ask in the lan- f the Hottentot preacher, and after the example of Liebig him- eference to humus—whence came the primitive ammonia? Liebig inion that the ammonia, if evolved along with boracic acid in vol-

canic situations, is an original constituent of the earth; but the quantity and sources are too limited to afford sufficient supply for such purposes as we have discussed. This point may therefore be considered as still *sub judice.*

Poisons, miasms, and contagions. Inorganic substances when introduced into the animal system act in the first instance chemically, and by producing abnormal action upon the nervous system give origin to the symptoms of poisoning, such as irritability of the stomach and paralysis. The iodide of potassium, sulphocyanuret of potassium, ferrocyanuret of potassium, nitrate, chlorate, silicate of potass, and in general salts with an alkaline base, pass without alteration into the blood, the sweat, the chyle, and the bile, and are thrown out by the kidneys. The citrates, the tartrates, and acetates of alkaline bases, in their circulation through the system, on the other hand, are converted into carbonates; this fact proves that they have accumulated in their passage a quantity of oxygen. In order to convert one atom of acetate of potash into carbonate, 8 atoms of oxygen are required, 2 or 4 remaining in combination with the alkali, and the remainder being evolved as free carbonic acid. During the circulation of these salts through the lungs, Liebig conceives that these acids take part in the peculiar process for which these organs are set apart; a certain quantity of the oxygen gas inspired unites with their elements, transforming their hydrogen into water, and their carbon into carbonic acid. A similar effect is produced when impure solutions of these salts are left in contact with the atmosphere; so that we have a mode of similating respiration artificially; on the other hand, mineral acids, and fixed vegetable acids, and mineral salts with an alkaline base, prevent *decay.*

Liebig has shown that the decay of organic matter, or *eremacausis* (slow combustion), as he terms it, is similar to the action which takes place in the lungs. This action would be arrested if an excess of acids or metallic salts could reach the lungs; but the accumulation of the latter is prevented by the circumstance that membranes are not permeated by concentrated saline solutions. A dry bladder remains more or less dry in a saturated solution of common salt; fresh meat which has been strewed with salt will be found, after twenty-four hours, to be swimming in brine, although no water has been added; the water in immediate contact with the salt has dissolved the latter, and consequently lost the power of permeating animal substances; hence the mode by which salt preserves meat, viz. by extracting the water from it. The same observation pertains to alcohol. Liebig applies these facts to explain the action of salts when introduced into the alimentary canal; a concentrated saline solution when conveyed into the stomach, by extracting water from the coats of that organ and the intestines, produces thirst and a purgative action; a diluted solution, on the contrary, is absorbed. That this is the primary action of purgatives is abundantly obvious from the fact that all mineral salts without alkaline bases, have the same action; it matters not what the base or acid is, whether potash, soda, or magnesia, or phosphoric, sulphuric, nitric, or hydrochloric acids.

The salts of metallic oxides possess a tonic action. Instead of abstracting matter from the tissues they unite with them, (C. G. Mitscherlich and Dr. R. D. Thomson,) they constitute the true inorganic poisons; they are never in the excretions. When arsenious acid is given in solution, it

may enter into the blood and poison it, that is, combine with the fibrine in solution, and with every blood-globule with which it comes in contact. But how, it may be asked, can we suppose a grain of arsenic, which has been recorded as having produced death, to occasion such a chemical change in the blood, a fluid containing such a comparatively prodigious quantity of matter capable of combining with the arsenic? The contrast however will not appear so remarkable, when we consider that 1240 grains of arsenious acid require for combination 6361 grains of fibrine; now we could form an artificial fluid similar to the blood, if we were to dissolve 100 parts of dry fibrine in 30,000 parts of water; so simple is the dissipation of mystery, when we remove all error and wrestle with the pure truth. With respect to the action of strychnine, other alkalies, and prussic acid, it is true that no primary action has hitherto been elicited, but ignorance is no argument against the existence of anything; the reason for supposing that no physical change takes place, because the action is so exceedingly rapid, is fallacious, especially since the experiments of Tiedemann were published, (Brit. and For. Med. Rev. vol. I. p. 243.) He injected an ounce of spirit of wine into a vein in the thigh of a middle-sized dog: the injection was scarcely over, before the odour of the alcohol was perceptible in the expired air; again no poison acts like a blow on the brain. Hydrocyanic acid is perhaps the most rapid of all poisons in its action, and yet after numerous observations we have never seen it exhibit any symptom upon the animal economy earlier than fifteen seconds, a period sufficiently protracted to enable it, according to the results of the German physiologists, to pass through the whole sanguineous circulation.

The substances to which we have hitherto referred exercise their influence upon the animal economy, by forming chemical combinations, or by containing a poisonous principle; but we are acquainted with another class of bodies, not less mortal, which act by virtue of the condition in which they are found. To comprehend the mode of operation, it is necessary to understand the processes of putrefaction, fermentation, and *eremacausis.* The term fermentation is, in common language, applied to that change which occurs in bodies, where gaseous bodies destitute of odour are disengaged: putrefaction denotes that spontaneous decomposition of organic substances, in which gases with a disagreeable smell are emitted. Chemistry demonstrates, however, that both of these processes are of a similar nature, the one implying the decomposition of substances destitute of nitrogen, the other of substances which contain it. Eremacausis differs from the two processes mentioned, in the circumstance that it cannot take place without the access of air: we have an example of it afforded in the decay of woody fibre: when this substance is exposed to the action of air or oxygen, the latter is converted into an equal volume of carbonic acid, and when the oxygen disappears the decay ceases. If the carbonic acid formed be replaced by oxygen, the decay recommences: hence this decay is precisely similar in its results to the combustion of pure carbon at very elevated temperatures; according to Liebig this is the process which takes place in the lungs during respiration.

But to return to fermentation: according to a law proposed by La Place and Berthollet, "a molecule set in motion by any power can impart its own motion to another molecule with which it may be in contact." It is

a mechanical law which is brought into force whenever the resistance which is opposed to motion is not sufficient to neutralize it. Now yeast is a decomposing body, the molecules of which are in a condition of motion or in the act of losing their equilibrium. If agitated in a vessel filled with a solution of sugar, the molecules of yeast communicate their condition to the particles of the sugar; the result is the formation of carbonic acid and alcohol, compounds in which the constituents are retained in combination with a greater force than in sugar: when the substance made to act on sugar is in a different state of decomposition, different products are the result: thus when brought in contact with rennet or putrefying vegetable juices, it is converted into lactic acid, mannite, and gum; under the condition we have mentioned, that is in contact with sugar, yeast disappears and none is reproduced; but when added to gluten contained in vegetable juices, new yeast is formed; yeast therefore is produced from gluten. Hence it follows that a decomposing body added to a mixture in which its component parts exist, is capable of reproducing itself in that fluid, in the same manner as new yeast is formed when yeast is added to liquors containing gluten.

These principles, it is obvious, may be applied to organic substances which are products of the animal economy. We know that all elements of these substances are derived from the blood, the most complex of all existing substances. Other bodies can act upon the blood, can attract and change it: but it possesses within itself a power of transformation. When putrid matter is laid upon fresh wounds, as in dissecting, the blood is poisoned, and direful effects result. The poison of decayed sausages belongs to this class of poisons. Several hundred cases of death have occurred from the use of this food. These sausages consist of liver, blood, &c. mixed with salt and spices, which are placed in bladders, or intestines, and after being boiled are hung up to be smoked. When these are properly prepared they may be preserved for months; but when the quantity of condiments is deficient they undergo a kind of putrefaction which commences in the centre of the sausage. No gas appears to escape; but they become paler in colour, and more soft and greasy in the putrefied parts, and they are found to contain lactic acid and lactate of ammonia. The cause of this action has been variously attributed to different sources. Prussic acid, sebacic acid, and benzoic acid have all been blamed for the mischief; but these causes are the dreams of theorists, not the deductions of practical men. The action elicited is situated in the function of assimilation, if we may judge from the aspect of the patients affected. The muscular development disappears, with all similarly constituted parts of the body; the patient becomes dried up like a mummy; the saliva is viscid and stinking; the dead body presents the appearance of coagulation, and does not putrefy. The result is analogous to the action of alcohol and boiling water. It is scarcely possible to mistake the action of the poison now described; for the experiments of Colin have distinctly proved that muscular fibre, urine, cheese in a state of decay can communicate a decomposing influence to much simpler bodies: when mixed with sugar they produce fermentation. Liebig contends that when fat and putrefying muscle produce death by being placed on living wounds, they act in a similar manner. They communicate their own state of decomposition to the blood *from which they*

were themselves formed. Poisons, such as those of smallpox, syphilis, plague, which are formed from the blood, are capable of reproducing themselves when introduced into that fluid.

The similarity of the phenomena induced by contagions and by yeast is so striking that it attracted the attention of Hippocrates; but to that distinguished physician the idea was merely figurative. He could have no tangible notion of the nature of the action, because he was ignorant of the chemical influence of yeast itself. Between the mode of action of yeast and contagion there appears to be almost as strong an analogy established as between lightning and electricity. Contagious matters are neutralized, Liebig states, by heat and alcohol, while acids, mercurial salts, sulphurous acid, chlorine, iodine, bromine, aromatic principles, volatile oils, empyrematic oils, smoke, coffee, deprive them of their contagious powers. The same influences, without exception, retard in general fermentation, eremacausis, and putrefaction. Contagious matter, we thus discover, must be a solid organized substance, capable of reproducing itself in the fluid from which it has been formed. By this consideration we shall be enabled to appreciate, at least in part, some of the poetical philosophy of Jahn. (System der Physiatrik, 8vo, 1835; Br. and For. Med. Rev., vol. IV., p. 120.)

The inferences which we deduce from the views of Liebig are opposed *in toto* to the idea of inorganic gaseous substances being capable of producing those diseases which possess regular phases. The average of the phenomena of smallpox, measles, &c. are not less regular than the phases of the heavenly bodies. Interruptions or irregularities, it is true, we often find recorded. But how often do these result from errors of observation, or from influences whose value is capable of calculation? These views are also opposed to the animalcular or infusorial theory of diseases, which is certainly possessed at least of great ingenuity. It assumes the theory of fermentation broached by Cagnard Latour, that the process is occasioned by the operation of infusoria—a gratuitous assumption, according to Liebig. It would admit of less objection if the animalcules are considered to be in a state of decomposition when they reach the circulation. In this case it is obvious that the theory tends to explain nothing, save that the occurrence of swarms of putrefying animalcules in the air might account for epidemics. If we admit their decomposition we might as readily grant the presence of volatilized putrefying matter derived from various sources.

It has been well observed that "the condition of medical science requires the separation of what is ascertained from what is only imagined." (Dr. Marshall Hall.) Now, much mystery has been thrown over many diseases by the substitution of *words* for *ideas*. What definite notion can we affix to the terms dynamic power of the organism in the present state of our knowledge? We have only the means of studying the effects of vital influence: it is beyond our power to ascend by pure induction from the effects to their causes. Many escape from their difficulties by adopting, with Dr. Prout, the old hypothesis of the existence of *independent* vital principles or agents superior to and capable of controlling and directing the agents operating in inorganic matters, on the presence and influence of which the phenomena of organization and life are supposed to depend. The chemical forces acting in the system,

according to them, are subject to this invisible cause. Of the existence of this cause we are only made aware by the phenomena which it produces. Its laws must be investigated just in the same manner as we examine those of the other powers which effect motion and changes in matter. When a chemical compound is introduced into the stomach it must exercise a chemical action upon every body with which it comes in contact. It is opposed to the vital power. If the latter predominate the substance is digested. If the chemical compound resists the vital power, its operation is *medicinal* or *poisonous*, according to the ascendancy which it attains. All the phenomena with which we are acquainted in relation to contagions are opposed to the idea of their possessing life. Their effects, it is true, have some resemblance to some actions of the animal economy. But the cause of these effects is simply a chemical action, capable of neutralizing other chemical actions exercised in a contrary direction. Some of the poisons generated in the body by disease lose their power when introduced into the stomach, while others retain their influence. The poison of the contagious fever in cattle, and small-pox matter, which are alkaline or neutral, are destroyed by the action of the acid in the stomach; while the poison of sausages, which is acid, has its power increased, or, at least, its energy is not retarded by the same influence. Diseases, however, are not contagious unless the matter which produces the disease meets with another substance by means of which it can be reproduced. Saliva is capable of converting sugar into lactic acid, without the addition or loss of any element. But in this process the exciting body is not reproduced, for the conditions necessary to its reproduction do not exist in the elements of the sugar. But when yeast excites fermentation in a mixture of sugar and gluten, the former is reproduced, because gluten is present from which the yeast had been primarily formed. Hence the formation of the existing body depends: 1, on the presence of the substance from which it was originally formed; 2, on the presence of a second substance, which is susceptible of decomposition in contact with the exciting body. Now in applying this reasoning to the action of contagion, we shall find that we must admit the presence of a second body in the blood capable of being decomposed. So long as no contagious matter gains access to this substance the human body is healthy, although in a state of *predisposition* to be affected. By this view we can account for the fact of many individuals escaping from the action of contagious diseases during the prevalence of epidemics. The second substance may be produced by imperfect assimilation, by a vitiating diet, and by a variety of causes which have hitherto been entirely overlooked. We shall now be enabled to estimate the value of attention to diet in warding off disease, and shall discover that this department of the physician's knowledge is even of superior importance to his acquaintance with therapeutics. We shall also feel the necessity in our practice of substituting wholesome plain food for the unnatural and often putrefying aliment contrived by the vitiated and pampered taste of dyspeptic man.

Liebig likens the action of cowpox virus to that of low yeast. It communicates its own state of decomposition to a matter in the blood, and from a second substance is itself reformed by a different mode of production. The product possesses the mild form and all the qualities of

cowpox lymph. The predisposition to infection by smallpox must cease after vaccination, because the cause of the predisposition has been removed by the action of the vaccine lymph. But the predisposing substance may be again generated in the same individual, so that he may again become liable to the contagion, and a second or a third vaccination may be required to remove the regenerated susceptibility. From the preceding considerations it would appear that when matter undergoing decomposition is the product of a disease it is called *contagion.*

We now turn to another cause of disease, viz. *miasm,* which is the product of the decay or putrefaction of animal and vegetable substances. Contagion or gaseous contagious matter occasions disease by reproducing itself in the blood. *Miasm* produces disease, but is not regenerated in the blood. All observations upon gaseous contagions show that they are substances in a state of decomposition. Air charged with them deposits upon vessels filled with ice a fluid, which soon becomes turbid and putrefies. All gases exhaled from putrefying matter have a bad smell, proceeding from substances in a state of decomposition, which is volatilized along with the gases. A number of metals, &c., exhale an odour when rubbed, as arsenic, phosphorus, &c., but only when in a state of eremacausis, that is to say, when they are oxidized at the ordinary temperatures. These gases are generally accompanied with ammonia, which is frequently the cause of their assuming the gaseous form. It may be detected in the air of a contagious room by condensing the moisture of the air by ice, in the manner described, and adding corrosive sublimate. Liebig accounts for the action of acids in destroying contagion, by their neutralizing power upon the ammonia, upon the presence of which the volatile nature of the contagion depends. Muriatic and acetic acids are best adapted for this purpose. Chlorine possesses similar powers; but, in consequence of its direful action upon the human economy, its influence where human beings are breathing is to be carefully shunned.

Such is a brief outline of the new views of Professor Liebig. We trust he will continue to pursue his researches, and render doubly sure many of his statements, which may be hesitatingly received, more from their novelty than from any apparent want of consonance to nature. We cannot, however, conclude without expressing our approbation of the admirable manner in which the English version has been executed, and our reprobation of the extravagant price of the volume. The French edition may be had gratuitously by the purchasers of the first volume of the *Chimie Organique.*

Art. IX.

Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society of London. Vol. XXIII.—*London,* 1840. 8vo, pp. 436.

There is always a pleasure in the anticipation of a volume of Transactions of the Medico-Chirurgical Society, and generally the volume itself realizes the anticipated pleasure. But we cannot say quite so much of that now before us, the merit of the communications being, with two or three exceptions, but little beyond mediocrity. It is often, we may say

always, too great a tax on reading-time, and patience, to be compelled to wade through pages of letter-press for some one important fact or supposed important speculation; and the impression is too often forced upon the mind by some writers, that they feel they must "make a paper," caring but little how, if so be that the "paper" be made. We have endeavoured from the volume before us to glean its few facts, after having cut away its harvest of words; and we think that a benefit would be conferred on the members if the Society subjected its volume before publication to a similar treatment.

I. A case of strangulated hernia in which the bowel was ruptured by the patient in his efforts to reduce it, is related by Mr. Travers. Beyond this fact, however, we do not see that it possesses much interest. The hernia appears to have been of the kind which the French term "hernie par engouement;" Mr. Travers terminates his communication by the relation of two cases, from which he was early disposed to decide negatively the question as to whether in the operation of hernia the ruptured parts should be returned into the abdomen without opening the sac. On this point we believe that he will find very very few surgeons at all at variance with his opinion.

II. The next paper is by Mr. George Gulliver, "*On the blood-corpuscles and pus-globules in certain animals.*" Mr. Gulliver remarks on the opinion that the pus-globules are but globules of blood deprived of their colouring matter, that his own observations have not led to any satisfactory results. It is well to remember in all these enquiries that this question is "one of more difficult solution by mere microscopic observation, than would be supposed by any one who had not specially examined the subject; for the blood-corpuscles are so singularly susceptible of modifications in form, size, and general characters, from very slight agency, that examples might readily be shown of their approximation in appearance to the globules of pus." (p. 27.)

III. Mr. James Paget has communicated some additional reasons for supposing that the *white spots upon the surface of the heart* are a result of inflammation. The fact of chief importance on which this opinion is founded, and which would constitute pericarditis a more frequent disease than it is supposed to be, is that "with these spots there almost always coincides some adhesion, by organized lymph, between adjacent parts of the pericardial membrane." This lymph sometimes consists of slender threads and granulations, and these occasionally require careful examination before they can be detected. In four cases, Mr. Paget found a band of adhesion passing from the surface of a spot to the pericardium opposite to it. He states "from the records of cases which I have examined, I find that in forty cases in which there were white spots on the heart, thirty-five have presented anormal adhesions or their remains. In five cases only were the adhesions absent, and in four cases only an adhesion was found where there were no spots." (p. 31.) The adhesions which are found in these cases are stated by Mr. Paget to be seated near the great vessels, where, in consequence of the comparatively small motion, there is every facility for their formation.

IV. The next paper consists of "*Remarks on Emphysema of the Lungs,*" by Dr. G. Budd.

"It is one of the chief objects of this paper to show that want of elasticity

ı the lung, in other words, absence of its natural tendency to collapse is he cause of many of the other anatomical characters of emphysema, and of most f the symptoms by which this affection is recognized. In order to show this ıore clearly, I shall first give a general idea of the act of breathing in the naıral state. In natural breathing, when the expansion of the chest has attained s limit and inspiration is complete, a quantity of air, equal to that which has een inhaled, has been again expelled, chiefly if not wholly by means of the elascity of the lung, which restores that organ to the volume it had before inspiraon. The *muscles* and parietes of the chest do no more than follow the lung in s collapse, in every other respect they are passive; the elasticity of the lung the main agent of inspiration. That this property of the lung is more than lequate to accomplish that action is proved by the fact, that when the chest is pened after death, and the atmospheric pressure tending to compress the lung consequently equal to that which tends to dilate it, there ensues a still furıer expulsion of air from the lung effected by the residual elasticity of that organ, that the only cause which during life prevents more complete expiration than oes actually take place, is the inability of the parietes of the chest to follow the ırther retrocession of the lung. When therefore the lungs have their natural ndency to collapse, the amplitudeof the act of inspiration varies with the degree which the parietes of the chest can follow them in their retrocession ; and this egree evidently depends in great measure on the obliquity of the ribs. . . . . hese points being established," &c. (pp. 39-40.)

But we do not agree with Dr. Budd in supposing that the above points re established; rather, we consider them quite doubtful, if not quite rong. Dr. Budd's proposition is, that the elasticity of the lung is the ıain agent in the vital act, expiration. We first of all object to any very rong argument being deduced in favour of this theory, from what is obırved on the dead subject, living and dead lungs being very different rgans. We next object that no allowance is made for the resilient acon of the cartilaginous portions of the ribs, none for the direct action of ıuscles evidently calculated to diminish the size of the cavity of the chest, one for the tonic action of other muscles, which by the descent of the iaphragm in inspiration have been extended beyond their normal point f repose. The lungs are supposed to do all that is done in expiration, nd for none of these various powers is any allowance made. We are much ather disposed to think that the elasticity of the lungs is a comparatively eeble power in the act of expiration; but however this may be, it is quite lear that Dr. Budd's proposition cannot be regarded as "established."

There are some other remarks in Dr. Budd's paper, to which we must all the reader's attention. Dr. Budd notices the raised position of the ribs n emphysema of the lungs, and the little space through which they can nove in inspiration, and points out the consequent modification which s effected both in the act of respiration and of coughing. It is well known hat a diminished function must exist in portions of emphysematous lungs; ınd the pale appearance and lessened moisture of their texture, are also vell known. The diminished vascularity of these parts is remarked ; and lluding to this in one case, Dr. Budd says "the coincidence of the pale, nemic condition of the pulmonary tissue with the congested state of the nucous membrane of the bronchial tubes in the same lung is worthy of bservation, as showing an essential difference between bronchitis and neumonia, a difference which has its origin in the different purpose and istribution of the pulmonary arteries." Another case is related in which

this lessened vascularity of an emphysematous lung is considered as protecting a large portion of the lung from pneumonia. From some observations made by Mr. Jackson, and mentioned by Dr. Budd, it would appear highly probable that emphysema of the lungs is an hereditary complaint.

"Of twenty-eight persons affected with emphysema of the lungs, Mr. Jackson found that eighteen were the offspring of parents (father or mother) affected with the same disease, and that several of these had died in its course. In some instances the brothers and sisters of these were also emphysematous. On the other hand, of fifty persons not affected with emphysema of the lungs, three only were the offspring of emphysematous parents. I need not dwell on the interest of this fact as it regards man, but I would point it out to the breeders of horses as one of great importance, for there can be little doubt that the disease is transmitted in the same way in the horse. It is undoubtedly owing to ignorance or disregard of this fact, that broken-wind is so common, even in young horses." (pp. 49-50.)

The dilatation of the air-cells in emphysema is explained by Dr. Budd a necessary consequence of want of elasticity of the lung.

"The powerful muscles of inspiration are continually acting to dilate the chest, and thence, by virtue of atmospheric pressure, the air-cells. This agency is not counteracted as it should be by the natural elasticity of the lung, and the air-cells as well as the cavity of the chest are, in consequence, permanently dilated." (pp. 52.)

The paper terminates by some considerations on the subject of asthma, in which Dr. Budd gives his reasons for adopting the opinion that the transverse fibres of the bronchial tubes are not muscular, but that their contractions are simply the result of elasticity.

V. Dr. Burton has described "*a remarkable effect upon the human gums produced by the absorption of lead.*" We referred in our last Volume (p. 427) to this appearance as noticed by continental authors, with whose observations Dr. Burton was not acquainted when he wrote his paper. Dr. Burton regards it as a "sign which will enable physicians to establish with increased facility a precise diagnosis, in derangements of health depending on the unsuspected presence of lead; and also to obviate in many cases, the infliction of lead colic, during the treatment of other diseases by saturnine preparations." The following is Dr. Burton's description of the state of gums above referred to:

"The edges of the gums attached to the necks of two or more teeth of either jaw, were distinctly bordered by a narrow leaden-blue line, about the twentieth part of an inch in width, whilst the substance of the gum apparently retained its ordinary colour and condition, so far as could be determined by comparing the gums of these patients with those of other patients of the same class in the hospital; there was no invariable tumefaction, softening, or tenderness, about them, neither was there any peculiar fetor in the breath, nor increased salivary discharge to be observed on any of the fifty patients." (p. 66.)

We do not believe that Dr. Burton's diagnostic sign will be found so general as he imagines: we wish our readers, however, to partake of all the chances it supplies to improve their practice. It is certainly true that many diseases which fall under the physician's care seem to resemble, more than is generally supposed, the action of a poison upon the whole system, although the attempt is constantly, how often vainly, made to localize them in some particular organ.

"Cases often occur in hospital practice in which the functions of the brain and cerebral nerves are paralyzed by lead, and in which coma, vertigo, headach, amaurosis, and sometimes deafness are the most evident effects; in other instances the patients complain of articular pains resembling those of chronic rheumatism, periostitis, and secondary syphilis. In many of these cases, an inspection of the gums will assist in making a correct diagnosis. . . . . I think articular pains proceeding from the action of lead have been treated sometimes as those of chronic rheumatism, at others as those of secondary syphilis, often empirically. . . . . To obviate the opprobrium consequent on *mala praxis* originating in an erroneous diagnosis, I repeat that a careful inspection of the gums will be sufficient in most cases of illness depending on the presence of lead, to reveal immediately the origin of the ill." (pp. 74-5.)

VI. Mr. Stanley has related "*a case of disease in the posterior columns of the spinal cord.*" It is one of those puzzling cases in which, in accordance with what are supposed to be sound physiological laws, the symptoms during life have appeared to justify the conclusion that after death disease would be found in the anterior columns of the spinal cord, when after death the said disease is limited to the posterior columns. Such facts give abundant scope for speculations, which do not accord at all with our present purpose.

VII. The next communication is from Mr. Liston, "*on the arrangement of the intermediate vessels on surfaces secreting pus; with a note regarding the vascularity of articular cartilages.*" Delineations are given in this volume of the facts which Mr. Liston has described. Beneath a coating of lymph which lines the interior of abscesses, which varies in thickness, and in immediate contact with which is the purulent deposit, is a highly vascular membrane, the capillary vessels of which are most intimately interlaced and looped in their general arrangement. These vessels are regarded, with much probability, as the source of the purulent secretion. On examining a portion of an injected ulcer, the secreting vessels are found to be similarly arranged to those just described. But here they are very much and irregularly dilated, "without doubt attributable to want of support from the natural elastic covering, and in a great measure also to the affected part being often kept in a position unfavorable to the ready return of blood." Mr. Liston offers some sensible practical observations on the appearances which he has so carefully described; and speaks first of all, of "the mischievous effects of squeezing together the sides of suppurating cavities."

"By this proceeding," he says "the lymphatic coating is separated from its base; the circulation of the part is unnecessarily excited; bloody and often putrid secretion is poured out, and the general health in consequence disturbed. If a sufficient opening is made in a dependent position, the accumulated secretion is rapidly enough discharged, and the walls of the cavity come together and coalesce, through the natural elasticity and action of the parts. As regards ulcers, the paramount advantage of an elevated position of the affected part must be sufficiently obvious. The rapid disappearance of congestive swelling and of inflammation by an observance of this practice alone in many cases, must make apparent the good effects of favouring the return of blood. The larger veins, previously varicose and over-distended, become collapsed, and almost disappear. The same effect upon the varicose capillaries in the solution of continuity necessarily follows; the colour of the sore is speedily altered for the better, the painful feelings abate, and the nature of the discharge is ameliorated. Until this is the case, and so long as over-action to any degree exists, sooth-

ing and relaxing applications are advantageous; exudation of lymph and plentiful secretion of pus are thus encouraged. These are followed by mild astringents and stimulants, by which the dilated and weakened condition of the coats of the vessels may be supposed to be amended. The discharge is thus moderated, and the granulations prevented in a manner from becoming exuberant. The beneficial effects of uniform support can also be well understood." (pp. 91-2.)

We have given the preceding remarks not so much for any novelty they contain,—for, indeed, we should hope that Mr. Liston's observations respecting the rough mode of handling abscesses alluded to, were scarcely necessary in these times,—but because the description before given of the anatomical condition of abscesses and ulcers so well explains the efficacy of the best recognized modes of treatment. Mr. Liston has succeeded in demonstrating the existence of vessels in articular cartilages, and has given in a plate a sketch of the appearances presented by them.

VIII. The next paper, by Mr. Cæsar Hawkins, is "*on the diagnosis of foreign bodies in the larynx.*" Mr. H. relates an instance of this kind, and comments on the various difficulties of diagnosis presented by similar cases. The whole paper is well worthy of attentive perusal. The fact which we shall here notice is valuable as excluding certain signs usually considered as important in diagnosis, but which are here shown to be sometimes wanting when their presence might be most expected.

"In three cases in which a foreign body was fixed in the direction of the cricoid cartilage below the glottis, the severe paroxysms of coughing which are invariably looked for as evidence of the presence of a foreign body (but which really belong especially to its presence in other parts of the tube) were entirely absent in two, and were mild in the third, so as to lead the surgeon to suppose they could not arise from the entrance of the pebble, as the child asserted, and were afterwards entirely absent in the last month of her life; that even the voice was unaffected in two of the cases, although hoarse in the third case; but that in all three cases there were soreness and uneasiness in the part where the foreign body was fixed, a *noise* in inspiration or expiration, or in both, from the mechanical effect of the intruding substance, and in all the *patient asserted* that something had been *swallowed*. Where such circumstances as these are present to guide the surgeon, I consider that he is imperatively called upon to operate without delay." (p. 106.)

This opinion, we apprehend, few surgeons will be found to depart from.

IX. Connected with the same subject is a "*Case of tracheotomy,*" related by Mr. Benjamin Travers, jun. From this case, which still further confirms the opinion announced in the last paper, Mr. B. Travers infers the expediency of performing tracheotomy in doubtful cases; and the inexpediency of employing any tube, whether metallic or elastic, instead of making a sufficiently capacious opening by the knife.

X. To these shorter papers succeed two essays by Dr. Marshall Hall, of a more important character. They are his second and third memoirs "*On some principles of the pathology of the nervous system;*" and as they involve questions of the greatest practical moment, we shall devote as large a space as we can afford to their consideration.

It is impossible that any great improvement in the science of physiology should be without its influence on that of pathology; and this must be especially the case with regard to the nervous system, the natural functions of which are so complex, and the morbid conditions of

which are productive of so great a variety of irregular actions. Hitherto, as every practitioner knows, to say that a disease is *nervous* has almost been tantamount to a confession of ignorance as to its real nature. The discoveries of Sir C. Bell, which separated the two leading divisions of the nervous system, the *afferent* and the *efferent*, from each other, afforded the first glimpse of a more satisfactory method of viewing the subject; but, however important these were in opening a new field of investigation, we cannot but regard their immediate value in the explanation of the phenomena of disease, and in the indication of measures for its cure, as less than that of the discoveries of Dr. M. Hall. For whilst the former almost solely concerned the phenomena of sensation and the influence of mental conditions which were subjects of common observation, the latter reduce to general laws a series of phenomena previously altogether unexplained, and unveiled to us others which had been entirely unnoticed. The phenomena to which we allude are those of the *involuntary* actions of the nervous system, arising from the operation of stimuli of various kinds upon its incident trunks, and the reflexion of these stimuli in the form of motor impulses along its efferent branches; these actions being independent not only of the will but of the consciousness of the individual, and manifesting themselves as well in regard to the functions of organic as in those of animal life.

The application of Dr. M. Hall's physiological discoveries to the explanation of morbid phenomena is as yet scarcely commenced; and of this no one is more sensible than their author himself. He rightly says that scarcely any cases of nervous disease formerly observed are now of any service; and that an entirely new system of observation must henceforth be adopted, in accordance with the present state of our knowledge of the normal functions of the respective divisions of the system. The papers now before us are merely designed to indicate, from observations already made, the general course which must be pursued; the author promises ere long a more specific "Plan of Observation of Diseases of the Nervous System," which we hope will be zealously followed up by those whose opportunities afford them the means of assisting in the full developement of his discoveries.

The principal part of the first memoir is occupied with a recapitulation of the principles which may now be regarded as established, as to the relative share of the brain and the true spinal cord in the production of motion. In order that the *reflex* actions in the human being should be apparent, three conditions, Dr. M. Hall remarks, are necessary: 1. That the interference of *volition* should be removed. 2. That the *vis nervosa* and the *vis musculuris* should be unimpaired, not to say augmented. 3. That the *reflex nervous arcs* should be uninterrupted.

1. That volition interferes with some of the manifestations of the reflex function is obvious from various phenomena of sleep, and of comatose and paralytic affections. The closure of the fingers of the hand of a child in profound sleep, when the palm is touched; the closure of the eyelids on touching the eyelashes during the coma of hydrocephalus; and the excitement of spasmodic actions in limbs completely paralysed as to voluntary motion, by stimuli which have no such effect on limbs still under cerebral influence, are adduced by Dr. Hall in proof of this position. He might have strengthened it by a reference to the compa-

rative physiology of the lower animals, in which it is pretty evident that the less the influence of the will, the more important and easily excited are the reflex actions.

2. In noticing the second principle we shall employ the term *vis nervosa* as indicating the means (whatever it be) by which a stimulus is propagated through the nervous system, so as to produce a motion. Our readers will remember that Dr. M. Hall and ourselves are at issue on this point: he taking credit for identifying the influence by which reflex actions are produced with the *vis nervosa* of Haller; whilst we do not regard this supposed discovery as of any real importance, since nothing is actually known of the changes which take place in the nerves of voluntary motion or of those concerned in reflex action, which can enable us to affirm that they are the same or different. Now, *immediately* after a violent shock to the nervous system, the muscular irritability and the conducting power of the nervous system appear suspended, so that no reflex actions can be produced; but at a more remote period they frequently return, and this even in an augmented degree.

3. The importance of the last principle appears self-evident; but the particular application of it needs to be borne in mind when individual cases of nervous disease are being considered. Thus in some cases of paraplegia the reflex actions are present, in others absent. In the former the disease must be in the dorsal or cervical portions of the spinal cord, leaving its lumbar portion free to carry on the reflex actions, though its connexion with the brain is interrupted. In the latter the disease is probably within the lumbar vertebræ, involving that portion of the nervous centres through which the reflex actions of the lower extremities are produced. It is of course necessary that the whole circle should be complete, and not merely the central organs concerned in reflex actions.

In Dr. Hall's former memoir he pointed out that the muscular irritability is augmented (for a time at least) in cases of *cerebral* paralysis; this augmentation probably resulting from its non-exhaustion by the operation of the nerves of voluntary motion. He now remarks that a corresponding facility of reaction to external stimuli is produced in tetanus, hydrophobia, and the artificial tetanus induced by strychnine, by the augmentation of the vis nervosa, so that the slightest external stimulus is sufficient to excite reflex actions in their most terrific forms. "What is remarkable is, that it is precisely the functions of the orifices and the sphincters, of the ingestors and egestors, which are most affected in these formidable diseases; and, most of all, the larynx, the pharynx, the organs of respiration, and the rectum." It is over these organs that the spinal system has a peculiar control.

The localization of the effects of remedies is another important branch of the enquiry touched on by Dr. Hall in this memoir. "Strychnine acts upon the glottis [we should rather say that it acts on the whole cord, but sometimes has a peculiar operation on the muscles of the larynx;] cantharides upon the neck of the bladder; aloes on the rectum; the secale cornutum on the uterus: all organs specially under the influence of the excito-motory power and reflex action of the spinal marrow." The action of cantharides upon the lower segments of the spinal cord is illustrated by the following curious case: "A young lady, aged twenty-

seven, had a fatty tumour within the tenth and eleventh dorsal vertebræ; it gradually, but completely, severed the spinal marrow, and induced paraplegia. The bladder lost its power of retention. On giving a dose of tincture of cantharides the power of retaining the urine was always restored *for the time*. This power would cease, and again be restored, on suspending or repeating the medicine. It is obvious that the cantharides acted through the segment of the excito-motory system left below the division of the spinal marrow." (p. 152.)

Dr. Hall subsequently directs attention to another important class of phenomena, in which the influence of the spinal system on the functions of organic life is remarkably displayed. "It is almost certain [the recent experiments of Valentin appear to have satisfactorily proved—see the first article in our present Number] that the gall-ducts, the ureters, and other excretory canals, are endowed with both incident and excitant, and with reflex and motor nerves. The passage of a biliary or urinary calculus excites vomiting; exposure to cold, a loaded intestine, certain passions, and in infants mere dentition, will, on the other hand, arrest the flow of bile, and induce icterus." (p. 157.)

The remainder of Dr. Hall's second memoir is occupied with an enquiry into the *retrograde* action, as he terms it, of stimuli acting on the spinal system of nerves. By this he wishes to generalize those phenomena, of no unfrequent occurrence, in which a stimulus conveyed to the lower part of the spinal cord, acts upon parts receiving their nerves from its upper segments. Now upon this we shall only remark that the fact of spasmodic actions being produced in almost every part of the body by excitants applied to parts below as well as above, has long been known to pathologists; and we do not see that, except in referring these actions, with others of a spasmodic nature, to his true spinal system, Dr. Hall has contributed anything to their explanation. To ourselves the matter appears quite simple. In the *true spinal cord* there is no such thing as *direct* and *retrograde*—terms which only apply to the course of that portion of it which consists of prolongations of the cerebral fibres. The true spinal cord may be regarded as a series of ganglionic centres belonging to the several segments of the body, and connected to each other by numerous sets of fibres passing in every direction. Nothing that we yet know of the course of these fibres explains why a stimulus transmitted to the upper part of the spinal cord should produce motion in the organs that receive nerves from the lower; and when this is understood we feel no doubt that similar connexions will be found to account for the converse phenomena, termed *retrograde* by Dr. Hall.

This memoir is concluded by a summary of inferences, the most important of which we have already interwoven in our account of it; and we shall therefore refer those who are desirous of following out the subject to the paper itself.

Dr. Hall commences his second memoir by pointing to what he considers the three causes or principles of muscular motion in the animal economy—*volition*, *emotion*, and the direct and reflex actions of the *vis nervosa*. In this enumeration we cannot but think there is some confusion: if the *vis nervosa* of Haller be intended (as Dr. M. Hall has constantly professed), this is alike the means by which volition, emotion, and simple reflex action produce muscular movement. No physi-

ologist ever speaks of a volition or an emotion being transmitted along a nerve; these being mental conditions which perform the part of a stimulus applied to some part of the incident system of nerves, in producing a motor impulse, or action of the *vis nervosa* (if Dr. Hall likes the phrase), which being propagated through the efferent trunks produces muscular action. The question between us is evidently one more of words than of things; but if it be an object to conform to the Hallerian phraseology, we must again affirm that Dr. M. Hall, in separating *vis nervosa* from the influence of volition, makes a wrong use of the former term.

"Volition," remarks Dr. Hall, "has a constant influence over some of the muscular actions of which we are almost unconscious, and which we only discover by carefully observing the effects of its subtraction." The acts of respiration, for example, originate in the reflex function of the spinal cord; but are regulated and rendered equable by this silent but constant influence, as is shown by their laborious and irregular character during great attention, profound sleep, coma, &c. The position of the body and every action of locomotion involve the constant and almost equally unconscious influence of volition; yet the movements of many animals, especially birds and insects, are performed after the removal of the cerebrum, with a degree of energy which shows that the will rather guides and controls than immediately stimulates them. Hence, observes Dr. Hall, we may account in some degree for the long flights of birds in migration; since the actions of their wings partake of the character of those of respiration, and others belonging to the true spinal system, which are performed without fatigue.

The influence of emotion is often still more important, and still less observed. It is manifested in the movements of *expression;* also in the spasm of some muscles, and the relaxation of others; in the contractions of the heart and arteries; the peristaltic movements of the alimentary canal; probably in the similar movements of the efferent ducts of glands, which greatly modify the secretion; and not unfrequently in movements of parts which are paralysed to volition. The organs influenced by emotion are so completely those which are most governed by the spinal system, that we cannot hesitate in agreeing with Dr. Hall in regarding this as its channel; which view is, in fact, no more than an extension of that of Sir C. Bell, who considered the movements of expression as belonging to his respiratory system. But Dr. Hall does not advert to the fact that *emotions* are generally *excited* through the *special* senses, which do not belong to the spinal but to the cerebral system; we cannot, therefore, avoid regarding them as having a distinct centre, though they may use the nerves of the spinal and ganglionic systems as the instruments of their operation. The researches of Valentin and others have made the source of the motory powers of the ganglionic system now quite evident; its motor and sensory fibres being entirely derived from the spinal nerves. There is, as Dr. Hall justly remarks, a near connexion between emotion and hysteria, which is doubtless very much a disease of emotion; the same organs, the same functions, are affected; and in both cases the will has a considerable power of controlling the movements which would otherwise involuntarily take place.

In quiet sleep we have the absence of all volition and emotion, and of their effects; these all return during dreaming and on awaking. The

first sleep especially, and the transition from sleeping to waking, are circumstances peculiarly connected with certain diseases of the nervous system: thus the singular frequency of the attacks of the croup-like convulsion of infants late in the evening, and of epilepsy in adults on awaking in the morning. These are, however, only hinted at by Dr. Hall, who justly observes that "to trace these principles of action in their healthy and morbid relations must be very important; but this task can only be accomplished by long-continued observation, and in some cases by numerical deduction."

With a view of illustrating these opinions, Dr. Hall again refers to the phenomena of those nervous diseases, in which one part of the nervous centres is severely affected almost or altogether to the exclusion of the other. Pure and uncomplicated hemiplegia dissects and severs, as it were, the cerebral from the true spinal system, volition from emotion and the reflex action. The power of voluntary motion of one side is suspended; but the action of the sphincters, the respiratory movements, and other operations of the true spinal system remain unimpaired; and the arm is often remarkably affected by mental emotion. Hence Dr. Hall concludes that the seat of emotion must be below the disease, probably in the medulla oblongata; and that the fibres which conduct its influence do not decussate. On the other hand, in tetanus the cerebrum is unaffected, but the whole spinal system is deranged. The intellect is serene; but the excito-motor power is augmented, and the actions of the muscular system (especially of those parts of it concerned in the ordinary associated movements of respiration, deglutition, defecation, &c.) are morbidly violent; and any sudden or startling noise, or any external impression, as a sudden jar, the contact of cold air, &c., are attended by the most painful and agonizing exasperation of the symptoms. "Such indeed," remarks Dr. Hall, "is the baneful influence of these various excitements, that I am persuaded that the very same treatment of tetanus may be successful or unsuccessful, according as we carefully avoid or admit the influence of emotion and external stimuli. Bearing this fact in our mind, the patient should be kept as free as possible from the intrusion of visitors, and should be carefully surrounded by an atmosphere of uniformly elevated temperature charged with moisture, every draught of wind, and all exposure of the cutaneous surface being cautiously avoided. We all remember the case in which the sudden plunge into a cold bath proved fatal. Other but less severe agencies of the same kind may prove injurious, though in a less terrible degree. Stillness and even darkness are essential to the safety of the patient." Although experience alone might have established the importance of these therapeutic rules, they derive additional weight from the knowledge of the manner of their influence. In traumatic tetanus, an excitant conveyed to some particular spot in the spinal cord, affects the motor nerves which pass off above as well as below it, constituting an example of what Dr. Hall terms the retrograde as well as the direct action of the spinal system.

In reference to the influence of emotion on the nervous system, Dr. Hall adverts to the fact that the irregular movements of chorea and incipient paralysis agitans subside during *quiet* sleep, but return during the agitation of dreaming; and also alludes to the well-known influence of emotion in aggravating stammering, the true idea of which irregularity

is, he considers, that of certain voluntary acts impeded and modified by emotion. We should rather say that there is an original deficiency of power to control and combine the several movements; and that this is aggravated by emotion.

Dr. Hall might have further illustrated his views by referring to cases (of which there is, we believe, more than one on record) in which the power of expression (an emotional act) has been lost along with the power of performing other sympathetic or excited movements, as those of respiration, whilst the muscles have remained completely under the control of volition. We feel no doubt that the practical applications of the discoveries which the last few years have witnessed in nervous physiology are as yet only in their infancy; and we would strongly urge our readers to do their best towards their improvement and extension, by applying themselves to the more careful discrimination of the phenomena of disease than has yet been customary; a discrimination which can only be exercised where the mind is under the guidance of sound physiological knowledge.

XI. Mr. H. B. Jones next gives a short paper "*On the presence of sulphur in cystic oxide calculus.*"

XII. Mr. Arnott, gives the case of a "*Large osseous tumour of the uterus,*" which weighed five pounds, yet occasioned no inconvenience during life.

XIII. In the next paper, by Mr. Dalrymple, "*On the rapid organization of lymph in cachexia,*" an attempt is made to show "that abnormal effusions from capillary vessels, without direct rupture of their coats, are more speedily and completely organized with vessels capable of being permeated by our minute injections, in those feeble and depressed conditions which we denominate cachectic than in the more vigorous and plethoric, where inflammations are more acute in the outset, and pass through more speedy and determined changes." In support of this view Mr. Dalrymple refers to the rapid organization of fibrine in cases of iritis occurring in habits very much debilitated from syphilis, or mercury, or other causes; to the progress of malignant disease of the eye in children, which, as in the case mentioned, appears to be influenced in its growth by the debilitated condition of the general system; to the vascularity of the lining membrane of some abscesses which were formed around a diseased knee-joint of very long standing, and where there was no vigour of constitution; and to an instance of scurvy, in which a quantity of blood effused beneath the periosteum of the tibia, was shown by injection to be most intimately permeated by vessels.

"The subject," says Mr. Dalrymple, "here briefly entered into is of vast extent, and appears to me interesting as a physiological enquiry; at the same time, it cannot be denied that in practice it would become important should future enquiry determine that the organizable materials of the blood become sooner and more completely organized in the debilitated and cachectic subject than in healthier and more robust constitutions."

The subject is certainly one of much importance, and Mr. Darymple has done a service by thus calling attention to it. But we do not think that the facts which he has brought forward go far enough to establish the great rapidity of the organization of lymph in cachectic subjects. Before

any precise inference can be drawn in such cases, we require to have the cachectic condition defined, or as nearly as possible defined, and this compared with the opposite state; also distinct evidence of the time which this or that product has occupied in becoming organized—such, for instance, as the lining membranes of abscesses mentioned, and the effused blood, and these compared with other abscesses formed in individuals not cachectic, and other quantities of blood effused (and not absorbed, another item in the question) in subjects not scorbutic. We make these observations only as hints and as difficulties, but still considering them important in the enquiry which has been entered into by Mr. Dalrymple, and which we hope he will pursue.

XIV. Mr. Stafford has related an instance of "*Recovery from cut throat, in which both the larynx and thorax were extensively opened.*"

XV. The next communication is from Mr. Bloxam, "*On the structure of the human placenta, and its connexion with the uterus.*" It is carefully executed, and illustrated by drawings, but scarcely a subject for notice in the present analysis.

XVI. Mr. Solly has given the "*Termination of the case of dry gangrene,*" mentioned in vol. xxii.

XVII. There then follow some "*Observations on injuries of joints, and their treatment.*" By Mr. R. Alcock.

Mr. Rutherford Alcock's paper is most carefully composed, and the facts and inferences from facts which it contains are apparently derived from extensive experience as a military surgeon. He is guided by safe principles in the views which it is his endeavour to establish, and we recommend the whole paper as worthy of attentive perusal, especially by those who are intended to act as military surgeons. Speaking of the danger of hasty generalization from a few facts, he observes, and the caution cannot be too often repeated:

"In the class of injuries under consideration this danger is especially evident. Many are the extraordinary and most unlooked-for successes attending the treatment of forlorn cases of injured joints. Were general rules or principles of treatment to be founded on these cases, which are but units among thousands giving contrary results, and were no reference made to those greater numbers which enlarged experience shows must perish in vain attempts to save limbs, an immense sacrifice of life and increase of human suffering would inevitably follow." (p. 245.)

The following remark respecting the saving of a limb it is well to remember:

"By a limb saved, I do not mean one with the wounds healed, having, nevertheless, the extremity contracted, bent, motionless, or otherwise useless; cases which by a loose kind of phraseology are often termed 'limbs saved.' The object of saving a limb is that it may be useful. If this is not the result, the member, by merely hanging to the body of the patient, is lost in my estimation as truly as if amputated, but with the additional circumstance of being converted into a source of misery to the sufferer, an impediment to the free motion of the rest of the body, and often a cause of irremediable ill health. Such cases I hold to be among the worst specimens of bad and injudicious surgery." (p. 246.)

In speaking of the excision of the ends of bones when injured by shot, it is considered that this operation is most applicable to the shoulder,

elbow, wrist, and ankle; that it is scarcely applicable to the hip and knee; and that it is most likely to be useful where the head of the humerus alone is implicated, and that by a musket-ball.

The results at which Mr. Alcock has arrived are the consequences of a careful analysis of about 100 cases of severe injury to joints, the notes of which he had taken care. Some of these results are presented in tables, these tables containing only the accounts of gun-shot wounds. And it is satisfactory that the author is fully alive to the mistakes into which statistics may, if not very carefully managed, lead us. "If," he says, "*all* the cases of a given period be included, they form sufficient grounds for just conclusions; but if one case be omitted the whole return is falsified: it may be a death, or a cure, or an amputation; but whatever the termination, its omission alters the legitimate conclusion." We shall endeavour to present to our readers such a brief analysis of Mr. Alcock's paper as may not be without practical interest.

All the injuries of joints are classed under three heads: 1. Where the limb may be saved, and where it should be a principle of practice to attempt this. 2. Doubtful cases, where diagnosis and treatment are difficult, where each case requires its separate consideration; but as they ultimately must require more or less protracted treatment, the same principles adopted for the first apply. 3. Where immediate amputation is required.

In lacerated or incised wounds penetrating the capsule of joints, Mr. Alcock does not think it of so much importance as it is mostly supposed to be, to exclude the air. Light dressings without any compression, cold applications, succeeded if agreeable to the patient's feelings by warm-water dressings:

"Or if the joint has assumed a puffy, swelled, and unhealthy appearance, a state often to be traced to the injudicious use of poultices, a more tonic and stimulating mode of dressing will generally cause improvement. Of this kind of dressing, the best seem to me either a decoction of aromatic herbs with the addition of a little wine, or warm camphorated or sweetened wine, which has not been freely adulterated with bad brandy, as are generally most of the wines consumed in England. Such dressing is frequently employed in the rest of Europe, and I have no hesitation in saying that I have seen the happiest effects from its use where more emollient applications, such as poultices, certainly did not arrest, but, on the contrary, appeared to promote the 'engorgement' of the limb." (p. 267.)

Pus must be freely evacuated when formed. Great quiet is long necessary. Some motion may be acquired if anchylosis be partial, and some force may be used to effect this; but if anchylosis be complete no attempts of this kind should be made. We would call attention to this last observation respecting the attempts which may be made to restore motion of anchylosed joints, in connexion not alone with gun-shot wounds but also in reference to the attempts which are now so frequently made to restore the proper position of deformed joints. There is sometimes great carelessness in the choice of cases for these operations, and we have seen partial or nearly complete anchylosis with contracted ham-strings following injury of the joint, in which the attempt to restore mobility disturbed the anchylosis and brought back the morbid action in the joint.

Mr. Alcock has given an analysis of the tables to which we have already referred. The following heads are those under which this analysis is given :

"1. Their proportionate numbers in relation to other classes of injuries, and of the articulations with each other. 2. Mortality, absolutely and relatively; number of amputations to which these injuries give rise, and proportionate number in different periods. 3. Causes of mortality, with regard to the whole number of deaths and to the number of deaths from each articulation, considered in relation to amputation at the three periods, primary, intermediary, and secondary, and to cases treated without amputation. 4. Influence of external and collateral circumstances." (p. 270.)

We have quoted the above arrangement that our readers may judge of the scope of Mr. Alcock's observations on the subject; from what follows we shall but extract such matter as is of practical or special interest, leaving a reference to the paper itself for the lovers of numerical calculations and detail.

The proportion of injured joints was found to be much less than Mr. Alcock supposed before he took the trouble to keep an accurate account of them; and of all joints none is so frequently injured as the knee. The mortality in all the cases of injured joints is very large indeed, taking into account deaths proceeding from the immediate consequence of injury to the joint itself, and injury proceeding from disease extending to the joint from the part primarily affected.

Mr. Alcock has taken pains to state general results in as clear a manner as possible, because the tendency of many of his observations is to lead to the saving rather than to the amputation of injured limbs. The following observations are important:

"Where the joints were directly involved, the number *treated*, that is to say, in which primary amputation was not performed, amounting to forty-four, present a mortality of twenty-five, considerably more than one half, whereas the primary amputations cause a loss of only one third, although naturally performed for the very worst injuries: and while twelve only were cured without loss of limb, eighteen died in the vain attempt to save, without for the most part offering any fair opportunity of remedying the evil by intermediary or secondary operation. Of the intermediary and secondary amputations, where treatment failing to save the limb amputation offered the only ground of hope for life, seven died out of fourteen, amounting to one half; but of the secondary amputations, properly so called, a fraction less than one half were lost, five in eleven. These cases form the forlorn hopes of surgery; all saved are snatched from nearly certain death." (p. 275.)

Injuries of the hip are commonly fatal. Of injuries of the shoulder the following satisfactory observations are made:

"The shoulder is rarely implicated directly by injury without a subsequent operation, amputation, or excision of the head of the humerus being required. In eleven,....only two were cured without amputation; seven amputations were performed, six primary and one intermediary: the latter was unfortunate in its result; all the primary recovered." (p. 277.)

We have already stated that injuries of the knee are most frequent, and excepting those of the hip, most fatal to life. Of 35 cases of injury of the knee, 22 died, and of the remaining 13 who were saved, 8 lost their legs. Injuries of the elbow are next in order of frequency; their fatality is in the proportion of 5 to 19. Frequently, it is remarked, that

although divers injuries happen in the neighbourhood of joints, comminuting the bones and producing extensive ill effects, the joints themselves are comparatively little affected, but are useful afterwards. Of 43 fatal cases, the deaths are thus arranged: 23 died under treatment for the original injury; 4 after intermediary amputation; 5 after secondary amputation; 11 after primary amputation. Of the first 23, half died, chiefly from hectic; the other half "from supervening irregular actions, such as mortification, delirium tremens, tetanus, affections of the chest complicating the hectic state; from accidental occurrences, such as secondary hemorrhage, and from other complicating wounds." Mr. Alcock has not found in his own experience that in these cases there is any particular tendency to purulent depositions in other organs or parts of the body; and when these have taken place it has been in the majority of cases where amputation has been performed after injuries, where "the two shocks of the injury and the operation combine to produce this fatal effect." Mr. Alcock's view of this matter is different from that which has been put forward, that purulent depositions in distant organs are distinctive of secondary amputations. It is probable that other considerations besides those admitted by Mr. Alcock are of great importance in relation to any decision on this point. The determination of the connexion of purulent depositions in various parts of the body subsequent to injuries and operations, is perhaps the determination of the circumstances altogether the most favorable to the production of phlebitis or whatever may be the cause of these depositions. To take any number of similarly injured joints, in men differently circumstanced as to climate, season, comforts or the contrary, epidemic or other influences, and then to make the fact of primary or secondary amputation the one essential fact in determining the connexion of purulent deposition and primary or secondary amputation, is, in our view of the subject, entirely erroneous. It is characteristic of the uncertainty attaching to all attempts at accurate deductions from the majority of facts with which it is in our power to become acquainted, that they will not bear the strict examination which we should wish to be able to apply to them, but at the best allow us to conclude only from the greater amount of probability. On the recapitulation or summary* of the facts related, Mr. Alcock remarks:

"The chief danger and cause of death in cases treated to the end without operation is hectic fever, and a variety of accidental or irregular complications, such as secondary hemorrhage, epidemics, erysipelas, gangrene, &c. combined, form the remaining half. The cases in which amputation is performed in the first instance, with fatal result, present a very different cause of mortality: the chief agent being purulent disease of lungs or liver, and occasionally inflammatory affections of the lungs or pleura. Fevers, irritative or bilious, destroy more than one third. The deaths after intermediary amputations are chiefly caused by febrile action, irritative or bilious, and in secondary amputations, the shock of the operations, hectic and some accidental complications carry off the patients already much reduced by suffering and the long continuance of wasting discharge. The results of secondary amputations when fatal, and their causes of

* We wish Mr. Alcock would not employ the Gallic barbarism *resumé*, when his own language, which he knows how to use so well, supplies so unexceptionable substitutes as the words in the text.

ortality, are in some degree assimilated to those predominant in cases treated the end without operation." (p. 287.)

After some remarks on the influence of favorable and unfavorable rcumstances on the class of injuries here treated of, the paper termiates by a classification of the kinds of injury in wounds of the articulaons. In this classification are noticed some important points and rules practice, which we regret not to be able to refer to at greater length. We cannot concur with Mr. Alcock in supposing that some of his nclusions from cases related are anything more than mere hints the proper mode of practice; there is such a want of uniformity in the mediate and remote effects of apparently similar injuries, that it would quire a large class of cases to derive anything like a rule which should a satisfactory guide. These remarks we would apply to the concluon drawn from cases 4, 5, 6, as well as to other conclusions which n only be regarded, as we have said, as hints to proper treatment; at e same time they are valuable hints, and such as we shall do well to udy and consider. All the cases related by Mr. Alcock are well worthy attention, and some especially, as giving reason to hope that many nbs, the joints of which have been extensively injured, may be saved d rendered serviceable, under circumstances which would generally be garded as demanding their removal. We cannot conclude our notice Mr. Alcock's paper without most especially recommending it to our aders, and without expressing our regret that our notice of it is necesrily so limited.

Our notice of the papers in this volume has already extended so far at we can only give the title of the remaining communications.

XVIII. The first is a very elaborate and valuable one, "*On aneurisms, d especially spontaneous varicose aneurisms of the ascending aorta d sinuses of Valsalva, with cases;*" by John Thurnam, Esq.; and hich we recommend to the notice of our readers.

XIX. Mr. Curling has described a "*Rare species of hydatid (the hinococcus hominis) found in the human liver.*"

XX. By a series of experiments on animals, Mr. R. H. Meade has own the similarity between the processes of repairing fractures in long d flat bones.

XXI. Mr. Wickham next relates a case, in which for aneurism of the teria innominata he tied both the carotid and subclavian arteries, and ith some apparent temporary benefit. The patient, however, died ortly afterwards from rupture of the aneurismal sack.

XXII. The volume terminates by a case communicated by Mr. Lever, owing the advantage of evacuating, through an incision made in the gina, encysted tumours occupying the pelvis, and acting as impediments parturition.

ART. X.

Beobachtungen und Bemerkungen über Gehirnerweichung. Von Dr. C. H. FUCHS, Professor der Medicin zu Würzburg.—*Leipzig,* 1838. 8vo, pp. 245.

Observations and Remarks upon Softening of the Brain. By Dr. C. H. FUCHS, Professor of Medicine at Wurzburg.—*Leipsic,* 1838.

ENCEPHALOMALACIA, ramollissement, or softening of the brain, notwithstanding the researches of Rostan, Lallemand, Herbst, Gendrin, Abercrombie, and Carswell, seems scarcely to have met with that attention which the frequency of its occurrence, in connexion with the paralysis of old people, and the extreme fatality attending it, require. The work of M. Rostan was the first in which it was made known to the profession that there is a peculiar disorganization of the cerebral structure, consisting in a greater or less degree of softening or breaking down of some portion of the brain, which, in many instances at least, is capable of being recognized during the life of the patient by the symptoms accompanying it. M. Rostan's cases occurred in persons of advanced age, and present many features in common with other cerebral affections to which those in the decline of life are liable; but Dr. Abercrombie showed that the pathological change occurs more frequently, according to his experience, in the young, and especially in connexion with acute hydrocephalus, either as a complication or as a specific form of that disease. The author of the work before us also describes cases of softening of the brain occurring in young persons as well as in the aged, and from his observations it would seem that, while the cases of the latter description resemble more nearly those related by M. Rostan, those of the former are for the most part of a somewhat different character, and to be considered rather as secondary affections consequent upon the development of some other diseased process.

The treatise before us embraces nine chapters, severally devoted to the following subjects: 1, The Pathological Anatomy; 2, Cases of idiopathic or primary Softening, and its complications with sanguineous and serous effusion; 3, the Symptoms; 4, the Diagnosis, and cases of secondary softening; 5, the Causes; 6, the Duration, Progress, and Terminations; 7, the Prognosis; 8, the Therapeutics; and 9, the Nature of the disease.

In the first chapter, under the head of pathological anatomy, a pretty full account is given of the morbid change which constitutes the essential characteristic of this peculiar affection. This change consists in a breaking down of the structure of some portion of the brain, cerebellum, or spinal cord, by which the cerebral or cerebro-spinal matter loses its natural filamentous appearance, and becomes more or less discoloured, softened, and infiltrated, in some instances being reduced to a completely disorganized semi-fluid pulp. The alteration of colour is described by Dr. Fuchs as varying from milk-white, dull-white, or opalescent, or very pale and dirty yellow, to greenish or brownish yellow, and not unfrequently to reddish, deep, dusky, or brown red, brown, and even approaching to black; of consistence from that in which the usual appearance is presented to the eye and the degeneration of structure is recog-

nized only by the readiness with which the cerebral matter is reduced to pulp by the slightest impression of the finger, to that in which it is found already in a pultaceous or, as has been observed by MM. Andral and Velpeau, an altogether fluid condition. The variations of consistence would seem to indicate either the intensity or the duration of the morbid process, while those of colour are probably derived, in part at least, from more or less injection or infiltration of the softened part with blood, lymph, serum, or other fluids. The whitish and yellow-coloured softening of the brain, is thought by M. Lallemand, who considers this morbid alteration of structure as a product of inflammation, to be owing to purulent infiltration in the cerebral texture, and the red or brown coloured softening as deriving its tints from inflammatory congestion. Dr. Fuchs, however, while he admits that the change of colour in the latter class of instances is owing to the presence of blood in the altered and disorganized structure, does not consider that the congestion is necessarily inflammatory in its origin.

"Every instance of softening of the brain," he observes, "must, according to the views of Lallemand, Gendrin, and others, be red coloured at its commencement, that is, inflammatory; and the pale, faded, yellow softening, which is synonymous with suppuration, should occur only as a consequence of the former state. But to me the colourless, pale, or faded softening clearly seems the most simple condition; while, on the contrary, the reddish, red, wine-lees discoloration is formed, when parts of the brain abounding in blood are affected, or where the softening, from whatever cause, coexists with congestion, through destruction of the vessels in the softened part, and more or less infiltration of the disorganized matter with extravasated blood; finally, the dark discoloration is a consequence of the sanguineous softening." (p. 5.)

The extent of the morbid change is liable to great variation. Dr. Fuchs states that he has seen instances in which the softening did not exceed the size of a cherry-stone, while in others almost the whole of one of the cerebral hemispheres has been involved in the disease. Other writers speak of the entire brain as having been softened, but it has not fallen within the experience of the author of this treatise to observe a case of idiopathic encephalomalacia of such extent. The softening spreads itself sometimes superficially, sometimes deeply in the substance of the brain, and sometimes equally in every direction. Occasionally it is diffused throughout the different cerebral structures; in other instances it is found either in the white or cineritious portions, or confined to a single convolution, or to the corpus striatum or thalamus nervorum opticorum of one side. M. Rostan states that the corpora striata and optic thalami are the parts which are most frequently the seat of the softening; but if the cases in which this diseased condition is complicated with other diseases of the brain be excluded, it will be found that some part of one or other of the hemispheres, chiefly the right, is most usually affected. Of the first thirty cases mentioned by M. Rostan, which are those of the simple uncomplicated disease, both hemispheres were affected in four; in thirteen the right hemisphere alone was the chief seat of the morbid alteration, and the left in seven. The cases narrated by Dr. Fuchs afford a similar result; the right side of the brain being the seat of the destructive process in nine cases, the left side suffering only in four.

A frequent morbid change of structure found by M. Rostan, and one which is regarded by Dr. Carswell as a well-ascertained and frequent

cause of the disease as it occurs in advanced life, is obstruction of the circulation in the brain from the deposition of osseous or fibrous matter in the arteries at the base. Dr. Fuchs, however, has had occasion to observe this structural derangement, in this situation, in two only of his cases; but in two others there was ossification of the arch of the aorta; in a fifth case there was deposition of bony matter in the valvular apparatus of the left side of the heart as well as in the arch of the aorta; and in a sixth the abdominal portion of this vessel was ossified.

The cases related in the second chapter are fourteen in number, of which six are examples of the simple and uncomplicated disease, the softened portions of the brain being but slightly tinged with blood, and serous effusion, if present, in inconsiderable quantity. In two cases the infiltration of the disorganized cerebral structure with blood was considerable, and was attended with a corresponding variation in the symptoms; in two others there was sanguineous effusion into the softened part producing genuine apoplexy; and in another the disease was complicated with copious effusion of serum. The three remaining cases are related as instances of a favorable termination of the disease; the subject of one of these, however, subsequently died from an attack of pneumonia, and on examination the softening of the brain was distinctly recognized, so that, although the symptoms were so far relieved as to allow of the dismissal of the patient from the hospital, the disease can scarcely be said to have been cured. In the other two no verification of the diagnosis having taken place some doubt must remain as to their being genuine instances of the affection.

Dr. Fuchs, as others have done before him, recognizes three stages of the disease: 1st, a precursory stage, in which the symptoms are upon the whole much the same as those which are found to precede many other forms of cerebral disease; 2d, the stage of paralysis; and 3d, a stage of low or torpid fever which follows with greater or less rapidity the occurrence of the paralytic symptoms. These several stages are, however, not always to be distinctly made out, temporary attacks of paralysis of slight and partial character sometimes arising in the precursory stage, and gradually passing into a more extensive or more permanent affection of this character before the loss of power or sensation becomes complete; while, in other instances, the typhoid state very rapidly follows upon that of paralysis. In Dr. Fuchs's cases the first stage was altogether wanting in one; and in three others it could not be distinctly ascertained whether the attack of paralysis had been preceded by premonitory symptoms or not; in the remaining cases the duration of this period was found to vary much, the extremes being twenty-four hours and six or seven months. In some of M. Rostan's cases the precursory stage would seem to have been of much longer duration.

Pain of head, usually but not always accompanied with confusion and giddiness; pale and sunken appearance of the countenance; dull and muddy eye, without heat of scalp, and a small and weak pulse, are enumerated among the most frequent symptoms of this stage, although they are by no means always present. Dr. Fuchs remarks that in almost all the cases in which the pain of head occurred, there was, in addition to the softened state of the brain, either extravasation of blood or effusion of serous fluid. In two cases only was the headach fixed, continuous,

and severe, in both of which chronic rheumatism (arthritis) had preceded the softening, and examination after death proved the existence of cephalic rheumatism "in the thickened membranes, ossified arteries, and small exostoses, the products of this affection in the head." Disturbance of the intellectual faculties was not common in this stage, and a disordered condition of one or more of the senses only observed as accompanying the headach. The most constant precursory symptom was some irregularity, diseased sensation, or deficient power in the organs of voluntary motion, in other words, incipient paralysis. "Almost every patient," says Dr. Fuchs, "experienced, sooner or later before the commencement of the second stage, if only occasionally, a sense of weight, weakness, numbness, and aching in the extremities of one side, that which subsequently became paralysed, and usually more marked in the leg than in the arm." Contraction of the muscles of one of the limbs, or of the extremities of one half of the body, was not observed by Dr. Fuchs among the premonitory symptoms, but convulsive motions of the muscles of the face and extremities, with strong tremor, were noticed in one case several months before the appearance of the fatal symptoms, and slighter and more partial attacks of the same nature occurred in two other instances. A diagnostic symptom to which the author attaches considerable importance was observed in three of the cases; it consists in a sudden loss of power in the extremities of one side while walking, so that the patient is compelled to sit down, or falls, without suffering any loss of consciousness. The circulation is but little affected in this stage, and the functions of nutrition and assimilation scarcely if at all disturbed.

The second stage of the disease is ushered in by an attack of decided paralysis, the paralytic symptoms being continuous and usually confined to one side, although in one of Dr. Fuchs's cases in which the softening was ultimately found to be seated in the *corpus callosum*, it extended to both sides of the body. Of the cases related in the work before us, in nine the paralysis was on the left side, and in four on the right, corresponding to the situation of the softening on the opposite sides of the brain. The attack was usually sudden, and in one case only did the paralytic symptoms come on so gradually as to render the line of demarcation between the first and second stages difficult to be distinguished. In two of the cases the attack occurred in the middle of the night, in nine in the early part of the day, and in two only, in which the softening was complicated with effusion of blood, in the afternoon.

The paralysis is not always complete at the outset, and hence the author has been led to recognize two varieties of the disease, a more acute and a more chronic form, and he thinks that the complication with sanguineous or serous effusion will usually be found to belong to the more acute variety. The first attack of paralysis also does not in all cases continue without change until the fatal termination of the disease, an evident amelioration being occasionally observed, and sometimes a complete restoration of the paralysed limbs. Loss of sensation as well as of motion, as in other forms of paralysis, is occasionally observed. Contraction of the muscles of the paralysed limb, a symptom considered by M. Rostan as pathognomic, was noticed by Dr. Fuchs in three cases only; in these the paralysed limbs were bent, the flexor muscles feeling firm and contracted. In each case this symptom commenced at the same time with

the paralysis; in one of the cases the muscles did not become relaxed until death, in a second the contraction together with the paralytic symptoms ceased a short time before the fatal termination, and in the third, one of those the result of which was favorable, the relaxation of the muscles took place a few days after the first traces of returning power were observed. The paralytic affection is not confined to the limbs, but extends also to the face and other parts of the body, in the majority of instances the mouth being drawn to one side, the cheek hanging, the eyelid dropped, the speech stammering and inarticulate. In one case there was slight difficulty of swallowing, and in three others complete dysphagia. Usually when the paralysis of the extremities was or had been complete the urine passed involuntarily, and the bowels were obstinately constipated. The consciousness, on the contrary, was seldom entirely extinguished at the outset of the paralysis, and, as it would appear, only in those cases in which there was complication with extravasation of blood; in some of the cases, on the contrary, the intellects seemed at first but little impaired for some time; and in one instance the loss of consciousness was not complete until about the fourteenth day from the attack of paralysis. The speech was frequently affected, but those who could make themselves understood complained of pain of head, especially on the side opposite the paralysed limbs; the pain was described by one patient as lancinating, shooting through the head, and intermitting; by others as pressing or stabbing. In only one case there was no pain of head. All who were able to speak complained of giddiness and great confusion of the head, and many of muscæ volitantes and sparks before the eyes and of noises in the ears. In every case of uncomplicated softening the face was pale, the expression of countenance troubled, the head did not feel hotter than the rest of the body, the eyes were dull, muddy, and hollow, neither vascular nor suffused, and the pupils of the natural size. The respiration was free and without rattle, although, in the more intense cases, quickened; the pulse was somewhat more frequent, never slower than natural, small, feeble, occasionally unequal, and in one case weaker in the paralysed arm than in the other. The temperature of the skin was usually unchanged, the tongue for the most part clean and moist, the thirst moderate, the appetite not much impaired, and the secretions and excretions both in quality and quantity commonly but little disordered. In the cases complicated with extravasation of blood, there was, on the contrary, more or less evidence of a state of congestion,—such as heat of scalp, pulsation of the arteries, and swelling of the veins of the head, lividity of the lips and tongue, stertorous respiration, &c.; and, when there was effusion of serum, loss of consciousness, puffiness of the face, dilated pupils, with impeded respiration, and rapid, small, irregular pulse.

The duration of this stage varied in Dr. Fuchs's cases from a few hours to twenty-five days, but the distinction between the second or paralytic and the third or febrile stage does not appear to be always clearly marked. Febrile symptoms indeed sometimes occurred in the premonitory stage, and occasionally death supervened in a few hours from the first onset of the paralytic state. In two of the three successful cases also the febrile symptoms never appeared at all, the one recovering in nine days, the other in a few months. The third stage is characterized by febrile irrita-

tion; small, weak, irregular pulse, with burning heat and dryness of skin; darkly furred, dry tongue; delirium and stupor, &c. This state commonly terminates fatally in from two to twelve days.

Dr. Fuchs sums up the characteristics of the simple idiopathic softening of the brain as follows:

1. *Premonitory stage.* A sense of debility over the whole body, of heaviness, numbness, and loss of power in the extremities, usually of one side, mental disquiet, and muddy pallid complexion are its most constant symptoms. The morbid sensations in the extremities are sometimes more lasting, the patients having a constant sensation as if the limbs were asleep; they drag more in walking, and are unable to use the arm and hand with as much freedom and strength as usual; but more frequently these symptoms are only occasionally present, coming on rather in paroxysms, as it were, in which the extremities of one side suddenly fail, and the patient must either sit down or fall but in a few seconds or minutes is again able to rise and pursue his way. In addition to these symptoms the patient complains occasionally of headach, giddiness, stammering, sparks before the eyes, noise in the ears, &c. and the powers of the mind are impaired, though all these last symptoms are often absent. The circulation and vegetative functions are undisturbed during this stage.

2. *Paralytic stage.* This stage usually comes on suddenly and for the most part early in the morning, the precursory symptoms but seldom passing by degrees and without a marked paroxysm into the state of paralysis. The patient generally becomes hemiplegic, and falls down with his mouth drawn and deprived of the use of the limbs of one side, more frequently of the left than of the right. In many cases the paralysis of the limbs is complete immediately on the first attack; in others, on the contrary, there is at first more or less limited motion, without strength or power of support, and the loss of power passes gradually with the further progress of the disease into perfect paralysis. Occasionally the paralysed limbs are at the same time without sensation, sometimes the sense of feeling continues strong; in many cases they are the seat of severe lancinating pains, and in others the flexor muscles are tense, hard, and contracted by tonic spasm or cramp, but seldom do they become affected in clonic or convulsive spasm. The consciousness is occasionally lost immediately at the onset, but in the greater number of cases it becomes extinct later towards the end of the second or during the progress of the third stage. All the patients, however, complain of confusion, giddiness, wandering, and many point to the head, especially to the side opposite to the paralysed one, as in pain. All are more or less confused and deaf, comprehending the questions addressed to them more slowly than in health, stammering and faltering in their speech, and often inarticulate and not to be understood; in several the speech fails altogether. The power of swallowing is occasionally lost, and from paralysis of the organs of the pelvis, the stools are for the most part retained, and the urine passed involuntarily. All appearances of congestion and determination of blood are absent; the face is pale and collapsed, the temperature of the head and of the whole body normal, the eye dull without suffusion or vascularity and hollow, the pupils unchanged; the respiration is easy and tranquil, and the pulse small, feeble, sometimes of the usual frequency, often somewhat accelerated, unequal, and irregular.

3. *Febrile stage*. The fever sets in with dry, burning-hot skin, small irregular pulse, darkly-coated dry tongue, dirty incrustation of the teeth, fuliginose discharge from the nostrils, and with great prostration of strength. If the consciousness had before been but little disturbed, wandering delirium takes place, which soon terminates in sopor: if the confusion had been previously considerable, the patient immediately becomes comatose. He is speechless and no longer capable of being roused; the paralysis is complete, and all perception of external impressions lost. Finally, the respiration becomes accelerated, laborious, and occasionally noisy, and death, generally in a few days, terminates the scene. (p. 115.)

In the fourth chapter the diagnosis between softening of the brain and various allied diseases, more especially apoplexy with sanguineous and serous effusion, inflammatory affections of the brain and its membranes, and certain chronic cerebral diseases, tubercle, cancer, &c. is pointed out. Dr. Fuchs draws an admirable parallel between the symptoms of softening on the one hand, and those of sanguineous apoplexy on the other; and were the softening always uncomplicated with congestive symptoms, as in the summary above given, little difficulty would be experienced; but here, as in almost every department of practical medicine, disease seldom presents itself at the bedside with the same simplicity as in the descriptions of authors. Much nice discrimination is therefore necessary; close examination of the symptoms, and careful weighing of their respective indications, before the complications of disease can be accurately unravelled. Still there are certain leading features by which the thoughts of the observant practitioner may be directed into the right channel; thus, with respect to the discrimination between obscure cases of primary softening of the brain complicated with extravasation of blood, and cases of sanguineous apoplexy in which softening of the cerebral structure becomes a secondary morbid degeneration consequent upon the extravasation of blood, the circumstances of age, time of attack, and order of symptoms, will afford good grounds whereupon to found a conclusion, although the general expression of the disease may be obscured.

The form of softening to which our attention has hitherto been exclusively directed occurs, for the most part, in persons of very advanced life, and chiefly among females of irritable or debilitated constitution. Of M. Rostan's first thirty cases, which are those of simple uncomplicated ramollissement, the ages of seven were between sixty and seventy; of fifteen between seventy and eighty; and of eight, eighty and upwards; three only were below the age of sixty-seven. Of Dr. Fuchs's cases, there were four under seventy and ten above that age. The attack of paralysis marking the commencement of the second stage usually sets in in the earlier part of the day; and, as was pointed out by M. Rostan, coma, which is the first symptom of apoplexy, is commonly the last in encephalomalacia. In apoplexy, in addition to the deep coma and stertorous respiration which characterizes the attack, those who are the subjects of it are usually males of less advanced age and more robust habit, and the time of seizure is commonly in the afternoon or at night after a full meal.

The distinctive marks between softening and other forms of apoplexy

re then pointed out. The greater number of recorded cases of the so-alled nervous apoplexy are considered by Dr. Fuchs, in accordance with ie opinion expressed by M. Rostan, to have been owing to softening of ome portion of the brain. Serous apoplexy, which he terms hydroce-halus senilis, may readily be recognized, he thinks, by the absence of ie peculiar premonitory symptoms before described, of the contraction f the muscles, pain of the limbs, &c. Of far greater difficulty and im-ortance is it to distinguish between this affection, when it occurs in hildren or young persons, and the hydrocephalus of early life, if indeed iey are not rather to be considered as different terminations of the same isease. It is certain that softening occurring in the young is preceded y or accompanied with symptoms far more nearly allied to those of in-ammation of the brain in its membranes than those usually observed in he same affection of the aged; and those who have attentively studied)r. Abercrombie's cases and compared them with the results of their wn experience, cannot fail to perceive that the earlier symptoms of acute ydrocephalus and encephalomalacia in the infant or young adult are ab-olutely the same; in short, both the effusion of serum on the one hand, nd the softening on the other, are, in these cases, merely consequences f a previous inflammatory state, and not unfrequently found combined: hey are both secondary affections arising out of a previous disease.

Dr. Fuchs remarks upon this subject, that in children and young ersons who die of acute hydrocephalus, the walls of the distended ateral ventricles, the septum lucidum, the fornix, &c. are, in the greater umber of cases, reduced to a milk-white disorganized (faserlose) pulp; and tates that he never saw this morbid change absent when the stages of con-estion and effusion were followed by a third stage, in which the hitherto-low pulse became hurried and not to be counted, the skin burning hot, he tongue dry and darkly coated, and the last traces of consciousness rere lost. This state is commonly characterized by convulsions ushering a paralysis, sometimes confined to one side of the body, but usually eneral, to which death for the most part in a short time succeeds. On he other hand, two cases are mentioned in which death took place in he second stage, the pulse being still slow and the skin cool, where pon inspection a considerable collection of water was found but no oftening of the brain.

We have already expressed our opinion as to the difficulty of dis-inguishing the complications of softening from other affections of the erebral organs. Dr. Fuchs, however, is sanguine enough to conclude hat not only may primary or idiopathic encephalomalacia be with cer-ainty detected by the character and succession of the symptoms, but hat it may with tolerable accuracy be predicted where secondary softening as taken place in other affections of the brain. He proceeds to point ut at some length the symptoms and marks by which, as he conceives, he existence of this peculiar morbid degeneration may be conjectured, ut as these are confessedly derived from a very limited experience, and re not altogether in accordance with what has been observed by others, t is unnecessary here to refer to them further.

The fifth chapter is devoted to the consideration of the causes which, n the author's opinion, exert an influence in producing or favouring the

disease. Among these he enumerates advanced age, debility of constitution, however induced, irritability of temperament, a state of poverty and wretchedness, mental emotions, the abuse of spirituous liquors, extremes of temperature, &c. A diseased state of the vessels of the brain, as we have before remarked, does not appear to him of the same importance in relation to the disease as it occurs in those of advanced age, as to some other observers; and with respect to the secondary disease, although its frequent occurrence in connexion with the hydrocephalus of children and the inflammatory conditions of the brain in young or middle-aged adults is admitted, the author does not consider it as otherwise connected with inflammation than as a consequence of the debilitating influence of that affection, or of the treatment necessarily had recourse to in its progress. To him, softening of the brain, whether idiopathic or secondary, appears to be induced, "by debilitating, paralysing influences, namely, by such as circumscribe the vegetative life of the brain," (p. 189;) and he concludes the treatise by declaring that with Hopfengaertner he considers "encephalomalacia as a peculiar asthenic injury of the brain with annihilation of the local organic life (vegetation)." (p. 245.)

If there is any difference between the sentiments here expressed and those of other observers who recognize the occasionally inflammatory nature of this affection, beyond that of mere words, we are ourselves completely at issue with Dr. Fuchs; but if he merely means to say that the disorganization of structure where it arises in connexion with a previous inflammatory state, results from the destructive effects of such a state upon the powers and organization of the affected part, no one, we believe, will be inclined to contest his position. The fact appears to be, that softening of the brain, as explained by Dr. Abercrombie and Dr. Carswell, is a form of gangrene, or rather sphacelus, which may arise in this organ as elsewhere from various causes acting injuriously on the local vitality and organization. The two principal of these causes are inflammation of the cerebral substance, and obstruction of the circulation from obliteration of the cerebral vessels. In the latter class of cases we hold the non-inflammatory character of the affection to be completely established. It is, in fact, a form of gangrena senilis. The following case, related by Dr. Abercrombie and occurring in a young person, is an exceedingly instructive one, as the symptoms were chiefly those of simple idiopathic softening, as described by MM. Rostan and Fuchs; and the peculiar diseased condition of the basilar artery affords a very sufficient explanation of the occurrence of the softening at so early an age without marks of inflammation:

"A young man, aged eighteen, had been for six or eight weeks affected with cough and pain of chest, and was supposed to be phthisical; but for several days he had been much better, when on the 15th December, 1819, he suddenly fell down deprived of sense and motion, and paralytic on the left side, with twisting of the mouth. When partially recovered, he complained of severe pain in the right temple; his speech was very indistinct; countenance expressive of great stupor. The usual treatment was actively employed, but without much benefit, and he continued for about ten days with little or no improvement; the left side perfectly paralytic; a great degree of coma; the speech very indistinct; but he still pointed to the right temple as the seat of fixed uneasiness. During

this time his pectoral complaints had completely disappeared. In January, 1820, he began to improve, so as to have less uneasiness in his head and considerable motion of the leg; but the arm continued entirely paralytic. His cough now returned, with considerable pain in the right side of the chest. He continued without further change till the 15th of February, when he complained of pain in the back of his head, and was seized with loss of speech and of the power of swallowing. He soon recovered his speech; but the power of swallowing was permanently lost, so that from this time he was constantly fed by liquids introduced into the stomach through an elastic gum tube. He was now quite distinct, and did not complain of any pain; the cough again abated; pulse of natural frequency but feeble. In the beginning of March he seemed to improve a little in strength, so that he was several times taken out in a carriage: there was considerable motion of the left leg, but the arm continued perfectly paralytic; no return of the power of swallowing; speech and intellect entire. He died rather suddenly on the 20th of March, having the day before become extremely weak and pale without any obvious cause. *Inspection.* On removing the dura mater, there appeared on the middle of the right hemisphere a remarkable depression, which, when cut into, was found to arise from an extensive mass of pure ramollissement; the part being in the state of a soft white pulp, without any appearance of pus and without fetor; it extended the whole depth of the hemisphere. In the cerebral matter adjoining to this disease, there was a small abscess no larger than a bean, lined with a firm soft cyst of coagulable lymph. There was very little effusion in the ventricles, and no other disease in the substance of the brain. On raising the brain a remarkable appearance was found in the basilar artery; through the extent of about an inch it was very much enlarged and hard, and this portion was found to be completely filled up by a firm white matter without any appearance of blood. Anterior to this portion there was a small coagulum of blood in the artery. The lungs were tolerably healthy; but there was a considerable deposition of coagulable lymph, forming a thick firm mass betwixt the right lung and the pleura costalis at the lower part immediately above the diaphragm."

This case, as it seems to us, affords an example of this peculiar degeneration arising from obstructed circulation, and is analogous, therefore, to the same affection as it occurs in old people; the greater number of Dr. Abercrombie's cases, together with some few of those related by M. Rostan, the majority of M. Lallemand's, and those described by Dr. Fuchs as secondary encephalomalacia, are, on the contrary, of inflammatory origin, and present an alteration of structure analogous to the destructive effects produced by inflammation in other parts where the vitality is destroyed.

The observations in the sixth chapter on the duration, progress, and terminations of the disease, are anticipated by what has already been said; and with respect to the prognosis and treatment, which form the subject of the seventh and eighth chapters, it is unnecessary to say more than that the former is for the most part in the highest degree unfavorable, and the latter hitherto inefficacious. The nature of the disease we have taken occasion to point out in the course of the preceding observations; and it only remains for us to express our approbation of the manner in which Dr. Fuchs has treated his subject.

ART. XI.

1. *De Strabismo Dissertatio, quam scripsit et pro licentia summos in arte medica honores postea rite capessendi publico eruditorum examini modeste submittit* NATH. GERS. MELCHIOR, Medicinæ et Chirurgiæ Candidatus.—*Hauniæ*, 1839. 8vo, pp. 75.

Dissertation on Strabismus, &c. By NATH. GERS. MELCHIOR, Candidate for the degree of Doctor of Medicine and Surgery.—*Copenhagen*, 1839.

2. *A Practical Treatise on the Cure of Strabismus, or Squint, by Operation, and by milder Treatment; with some new views of the Anatomy and Physiology of the Muscles of the Human Eye.* By P. BENNETT LUCAS, Member of the Royal College of Surgeons, &c. *Illustrated by Plates.—London*, 1840. 8vo, pp. 91.

3. *Practical Remarks on the New Operation for the Cure of Strabismus, or Squinting. Illustrated with lithographic Engravings.* By EDWARD W. DUFFIN, Graduate in the University of Edinburgh, and Member of the Royal Colleges of Surgeons of London and Edinburgh, &c.—*London*, 1840. 8vo, pp. 147.

4. *Die Behandlung des Schielens durch den Muskelschnitt. Ein Sendschreiben an Herrn Geheimrath Professor Ritter Dr. Dieffenbach zu Berlin.* Von Dr. F. A. v. AMMON, Sr. Majestät des Königs von Sachsen Leibartz und des C. V. O. Ritter. *Mit einer lithographirten Tafel.—Leipzig*, 1840. 8vo, pp. 38.

The Treatment of Strabismus by Myotomy. An Epistle to Professor Dieffenbach, &c., of Berlin. By Dr. F. A. Von AMMON, Physician in Ordinary to the King of Saxony, &c. *With a lithographic Plate.—Leipsic*, 1840.

5. *Practical Hints on the Cure of Squinting by Operation.* By F. W. GRANT CALDER, Licentiate of the Royal College of Surgeons of Edinburgh, and Assistant Surgeon to the Second Regiment of Life Guards.—*London*, 1841. 8vo, pp. 96.

6. *Du Strabisme.* Par Le Docteur CHARLES PHILLIPS, de Liège, Chevalier de l'Ordre Imperial St. Stanislas.—*Paris, Bruxelles, Liége.* 8vo, pp. 126.

On Strabismus. By Dr. CHARLES PHILLIPS, of Liege, Knight of the Imperial Order of St. Stanislas.—*Paris, Brussels, and Liege.* No date.

7. *Division of both Internal Recti in Squinting, in preference to that of any other Muscle, when the Division of one Adductor only is not instantly and completely successful.* By THOMAS ELLIOT, M.R.C.S.L. (In the Lancet for September 19, 1840.)

Division of the Corresponding Recti Muscles of both Eyes in Strabismus. By THOMAS ELLIOT, M.R.C.S.L. (In the Lancet, Oct. 31, 1840.)

8. *Some Remarks on Strabismus, including an Analysis of Two Hundred Cases.* By C. RADCLYFFE HALL. (In the London Medical Gazette for January 22, 1841.)

SQUINTING has so marked an effect on the physiognomy that, like other personal peculiarities, it has not unfrequently given origin to surnames.

Strabōn and *Strabo* were names among the Greeks and Romans given to those whose eyes were distorted. Pompey's father was so denominated; and the celebrated Italian painter, Giovanni Francesco Barbieri, was called *Guercino*, the name by which he is best known, from a cast he had in one of his eyes. This affection, now commonly known by the term STRABISMUS, used seldom to come under the notice of the surgeon, as, in a large proportion of cases, it was held to be beyond the reach of art. Since, however, it was found that a comparatively simple operation promised to rectify the distortion, hundreds of patients have presented themselves to medical practitioners everywhere.

The division of muscles in the orbit had been by several persons doubtingly suggested as a means of curing strabismus, but Dr. Stromeyer* gave consistency to the proposal by trials on the dead body, and Dr. Dieffenbach was the first who had the boldness to put the operation into practice on the living.†

Strabismus and luscitas are equally characterized by a loss of the natural correspondence of the optic axes; but in the former this is owing to a want of harmony in the motions of the two eyes, whilst in the latter it is owing to the eye being fixed more or less immoveably in one direction. In pure strabismus there is not a loss of motive power, but the distorted eye becomes straight, and is capable of being directed to any object when the other is closed. In luscitas, on the contrary, the affected eye cannot, under any circumstances, be turned right.

The following are the principal forms of strabismus: 1, Strabismus convergens; 2, strabismus divergens; 3, strabismus sursumvergens; 4, strabismus deorsumvergens. Of these different forms, the most frequent is *strabismus convergens.* Among 200 cases of strabismus in general Mr. C. R. Hall noted 168 of convergent strabismus of one eye, and 19 of both; Mr. Liston met with 119 cases of convergent strabismus of one eye, and 4 of both eyes in 125 cases of squinting; among 100 cases, M. Phillips found 79 of one eye and 17 of both; and in 61 cases, Melchior found 47 of strabismus convergens of one eye, and 5 of both eyes.

Divergent strabismus, though rare, is next in frequency. Out of nearly 500 cases of strabismus which Mr. Duffin has examined he met with 23 examples only of divergent squint. Mr. Guthrie met with this form in the proportion of one in thirty cases. In M. Phillips's 100 cases there were 10 of strabismus divergens, and among Melchior's 61 cases there were 6, in two of which both eyes were affected. In Mr. Liston's 125 cases there were only two of divergent squint. In speaking of the comparative rarity of divergent strabismus, Mr. Lucas says, (p. 47,) that out of some hundreds of cases of squint which he has seen within the last six months, only one was of the divergent kind; but he says he does not mean it to be inferred from this statement that divergent strabismus may not be more frequent than in the above proportion.

* See his Beiträge zur operativen Orthopaedik.—Hannover 1838; p. 22. Melchior, in the work at the head of this article, says, "Etiam sectio musculi prævalentis commendatur, atque fieri potest, ut in casibus, ubi vitium hæret in musculis, hæc cum usu institui possit." (p. 65.)

† We find it stated in a German journal that the operation was performed once at Dresden, previously to its performance by Dieffenbach at Berlin.—ED.

In regard to distortion of the eyes upwards or downwards, there were in M. Phillips's 100 cases, 2 of "strabisme supérieur" and one of "strabisme inférieur;" and Melchior tells us, that out of 61 cases of strabismus in general, there were 2 of strabismus deorsumvergens of both eyes, and 1 of strabismus sursumvergens also of both eyes.

It is to be remarked that the affected eye is not always turned exactly inwards, outwards, upwards, or downwards, but may be inclined in the intermediate directions; accordingly, Melchior remarks (p. 12), that in some of his cases the eyes were in a state betwixt strabismus sursumvergens and strabismus convergens, and some betwixt strabismus sursumvergens and strabismus divergens.

Strabismus most frequently affects one eye only, and that is generally, though not always, weaker than the other; sometimes the distortion changes from one eye to the other; rarely both eyes squint. It has been just mentioned that there were in Mr. C. R. Hall's 200 cases, 19 in which both eyes were affected with convergent strabismus, and 17 in M. Phillips's 100 cases, and 4 in Mr. Liston's 125. In Melchior's 61 cases, there were 10 of double strabismus, and of these 5 were convergent.

In regard to the relative frequency of strabismus in the right and left eye singly, it appears that the left eye is rather more prone to squint than the other. Melchior says that of his 61 cases, the left eye was affected in 27, and the right eye in 24. Mr. Lucas says (p. 44), that the record of the cases he has kept of permanent strabismus shows that the left eye is affected in the proportion of three to two. M. Phillips's cases give exactly an opposite result. Of 200 cases, says Mr. C. R. Hall, the left eye was exclusively affected in 110, and the right eye in 71: of these the strabismus was single convergent of the left eye in 103 instances; single convergent of the right eye in 65; single divergent of the left eye in 7, and single divergent of the right eye in 6. In Mr. Liston's 125 cases left eyes were affected in 67 and right eyes in 54.

Strabismus convergens being the most frequent form of the affection, we shall make it the principal subject of the following remarks.

The eye affected with convergent strabismus has the pupil habitually more or less turned in towards the nasal canthus, whilst the other eye looks straight forward and is capable of being directed to the various objects on which the person fixes his regard. It is only when the habitually well-directed eye is closed, that the inverted eye becomes straight and falls under the command of the patient to be turned in any direction; but as soon as the sound eye is again opened, the person loses all command over the affected eye, and it falls back into its original state of inversion.

In many cases the habitually well-directed eye squints, while it is covered and while the previously squinting eye is properly directed; this Melchior says (p.4), was first pointed out by Fischer, (Theorie des Schielens veranlasst durch Buffon's Schrift,—Ingoldstadt, 1781, p. 70;) and again by Purkinje, (Beobachtungen zur Physiologie der Sinne,—Berlin, 1825, 2 B. pp. 166,) and observed in most cases by himself. The experiment, he says, any one may easily institute, if he closes the sound eye with his fingers and a short time after suddenly raise the upper eyelid.

This tendency of the previously well-directed eye to turn in when the previously squinting eye becomes straight, is a circumstance of great importance to be attended to before operating lest, as Mr. Lucas says

(p. 48), when one eye was restored to a natural position the other should become inverted. This, indeed, is actually what occurs in such cases where the previously well-directed eye has been kept long bound up, or when the inner rectus muscle of the squinting eye is divided. We shall return to this point when considering the operation.

The cases in which the strabismus passes to the previously well-directed eye when this is covered, Mr. Lucas describes under the head of double convergent strabismus. We would rather call them cases of *alternating strabismus convergens*, and confine the appellation of *double strabismus convergens* to cases in which both eyes are habitually more or less turned in at the same time. In such cases, however, it is to be remarked that one eye is always more inverted than the other; one cornea perhaps being, as Mr. Lucas observes, more than half hid in the inner canthus, while the other has a slight inclination inwards.

In some cases of alternating strabismus, the patient has the power, immediately and voluntarily, to direct either eye properly; but while this is done, the other falls into the state of inversion.

In other cases the habitually squinting eye becomes straight, and the opposite eye squints without the will of the patient; and while both eyes are open, there is power to direct properly one eye only.

The varieties of convergent strabismus just described—single, alternating, and double—appear to run into each other by intermediate gradations. Between cases which may be called *luscitas* and pure strabismus, there are also gradations in which the patient still has the power to turn the eye somewhat out from the nasal canthus.

In many cases of well-marked double strabismus convergens, there is some degree of luscitas of one eye.

The vision of an eye that squints is usually, though not always, imperfect. An early symptom of strabismus is double vision, though of this the patient does not continue long sensible. In numerous trials, however, on persons who habitually squinted, Dr. Alison* always found, if the *vision of both eyes was tolerably good*, that, when the *attention was fairly fixed on the sensations of both eyes*, single objects held directly before the face were seen double.

For remarks on single and double vision with two eyes, we would refer to an article in the Nineteenth Number of this Review; here it is sufficient to observe, in a dogmatical way, that the double vision usually attending strabismus is owing to the circumstance that dissimilar or non-corresponding parts of the two retinæ are impressed by the rays of light proceeding from the same object.

The image seen by the properly-directed eye appears clearer than the other; which is owing not only to that eye being the stronger, but especially to the circumstance that in it the impression is made on the central part of the retina, which is more sensible than any other; besides, that the adjustment of the properly-directed eye corresponds with the distance of the object looked at.

The image of the affected eye is clearer, and, in consequence, the diplopia more striking the less the *cast* of the eye; hence the double vision will be noticed by the patient before the misdirection of the eye attracts the

* On Single and Correct Vision, &c. in the Transactions of the Royal Society of Edinburgh, vol. xiii. Edinburgh, 1836.

attention of those about him. Double vision ceases in many cases, because the impression on the sound eye is much more vivid than that on the distorted one; and we know by experiment, that of impressions dissimilar in force on the two eyes, the mind perceives the stronger to the exclusion of the weaker.

Causes of strabismus. Under this head Mr. C. R. Hall remarks that he is obliged to enumerate the circumstances assigned and believed to be the causes of strabismus by the patients themselves, or their parents, without being able to vouch for the correctness of the testimony, except where physical conditions yet remained to substantiate the opinion given.

"1. Convulsions during infancy in 9 cases; falls on the head in 7; severe concussion of the brain in 1; difficult dentition in 3; hooping-cough in 2; intestinal worms in 3; epilepsy in 2; a severe thrashing in 1; excessive fright in 1;

"2. Ophthalmia, which had left no opacities, in 14; opacity of the cornea in 5; opacity, said to have existed formerly, in 1; wound of the cornea by a stocking-needle in 2, by a fork in 1, by a thorn in 2; blow on the eye in 5; burn of the eye, from a piece of metal flying into it, in 1; a habit of looking at the sun in 2; crush, from a cart-wheel going over the orbit, in 2; amaurosis in 2; imperfect cataract in 3; exposure during infancy to the light and heat of a blazing fire in 3.

"3. Imitation of a squinting person in 39; watching the motion of a shuttle in 1; voluntarily trying to squint in 1; a habit of looking at a scar on the eyebrow in 1, at a scar on the nose in 2, at a scar on the cheek in 2, at a small encysted tumour at the inner canthus in 1, at a small nævus in the same situation in 1, at a mole on the nose in 1; a habit of sucking the thumb, and looking steadfastly at it at the same time, in 1; holding the head sideways whilst knitting in 3.

"4. Measles in 4; smallpox in 6.

"5. Severe burns of the abdomen in 2.

"In 4 instances I was assured that the squint was congenital. In the remaining cases of the 200 no causes were assigned." (Medical Gazette, vol. xxvii., p. 642.)

The causes assigned in Mr. Liston's cases are similar to the above—thus:

"Measles 10; looking fixedly or suddenly at objects in youth 16; convulsions or diseases of the head in childhood 16; fright 3; imitation 10; from falls 5; severe fit of crying 1; inflammation and disease of eyes 16; congenital, though many of the cases are doubtful, 10; dentition 5; bad state of health 1; smallpox 5; hooping-cough 4; injury of the head 2; unknown 21."

Is defective vision of one eye a cause of strabismus? In most cases the vision of the squinting eye is imperfect; but, it may be asked, is this cause or effect, or are not the defective vision and strabismus both effects of one and the same cause?

As very often both eyes have a tendency, the one to turn in while the other remains straight, imperfect vision of one eye will operate as a cause of rendering the squint habitual in that eye, for the reason that, as one eye only can be directed straight at one time, it is naturally the stronger eye which is so. In this case it is to be remarked, however, that the imperfect vision is not the cause of the squint itself; it is merely the cause of determining it to one eye rather than the other. The justness of this view is illustrated by the fact, that by binding up the stronger eye and strengthening the weaker by exercise, the strabismus has shifted from this to that.

Supposing defective vision of one eye to have some causal connexion with the origin of strabismus itself, it can scarcely ever be the efficient cause, as much more frequently all degrees of defective vision of one eye exist without the occurrence of strabismus; and blind eyes, as Melchior remarks (p. 8), are not more prone to squint than sound ones. "Ita in instituto cœcorum hujus urbis," says Melchior, "solos 4 strabismo insigni affectos vidi, idemque observavi in aliis intitutis Germaniæ, uti Dresdæ." "Instead of an imperfect state of vision," says Mr. Duffin (p. 3), "being a cause of strabismus, ninety-nine times out of the hundred we find that impaired sight is a result of this affection; and that as soon as the deformity is removed, the functions of the eye improve." This, perhaps, is a rather too sweepingly favorable report. Mr. Wardrop's remarks on this subject are very appropriate. "There is nothing more remarkable," he says, (Morbid Anatomy of the Eye, vol. ii., p. 213,) "in the history of squinting, and it is the same with regard to double vision, than that, as far as the pathologist can discover, the same disease in the eye does not always produce a squint. Many have a considerable disparity in the vision of the two eyes who have no squint, whether this disparity arises from a speck on the cornea—from any particular species of cataract—from differences in the sphericity of the cornea—or from some difference in the sensibility of the two retinæ."

"Whatever be the remote cause of strabismus," says Mackenzie, "we cannot doubt that its proximate cause consists in some affection of the muscles of the eyeball." The question which this conclusion naturally suggests is, what is the nature of this affection of the muscles of the eyeball?

In investigating this point, it is important to keep in mind the distinction between strabismus and luscitas.

The various remote causes of strabismus which have been remarked, such as imitation, a bad habit of misdirecting the eyes, affections of the mind*—as anger, fear, &c., disease of the brain, abdomen, and other parts, &c., together with the phenomena of strabismus in general, all point to the muscular affection being dynamic—being a "perverted action." Organic change of the affected muscle or contraction of surrounding parts may, however, supervene.

Morbid Anatomy. Rossi (Memorie delle Scienze de Torino) found, in post-mortem examinations of persons who had been affected with squinting, the orbit not presenting its axis directed as in the natural state, but more or less obliquely, inclining upwards or downwards, or to the external or internal side. In one case only did he observe an abnormal insertion of muscles.

Mr. Middlemore mentions that on examination after death of the body of a child which had been affected with divergent strabismus of the right eye, he found the external rectus certainly "much larger than it ought to have been, much larger relatively to the size of the other muscles of the same eye, and comparatively with those of the opposite organ.† A young man under the care of M. Guersent, squinted and had a speck on the cornea of the squinting eye, when he was seized with typhus fever,

* Sometimes from a strong mental impression the person will cease to squint. Dr. Ammon has remarked this during the preparation for the operation. (p. 5.)

† Treatise on the Diseases of the Eye, vol. ii. p. 561.

of which he died; Dr. Cavarra dissected the muscles of the eye with great care, their vessels, and their nerves, but found no appearance of disease about any of them. The brain seemed healthy, except only that the lateral-external part of the crus cerebelli, on the same side as the strabismus, presented a loss of substance for some lines, exposing the medullary matter.* According to Dr. Cavarra's dissections, there is in general nothing unnatural observed in the muscles or their attachments of a squinting eye.

The following communication was recently made verbally to the Royal Academy of Medicine of Paris, by M. Bouvier: A woman eighty-two years of age, having died at the Salpêtrière, with divergent strabismus of the left eye, with which, according to her own report, she had been affected from infancy, M. Bouvier seized the opportunity to examine what changes the muscles of the eye experience in strabismus.

"I remarked," he says, "first that the eye, which still remained everted, could be easily pushed inwards. To this the external straight muscle offered not the least resistance, although during life when the sound eye was closed, the patient could turn the eye from without inwards no farther than to the middle of the orbit. The dissection of the muscles which I present to the Academy demonstrates that the external rectus is in a state of complete relaxation, and that its length is sensibly the same as that of the other straight muscles, so that the eye can be easily moved in all directions. Nor is the texture of the external rectus altered in any way."

M. Bouvier's reflections on this case are so just, that we do not hesitate to quote them at length:

"If, as may be supposed, the disposition which existed in this case is general, it would follow that strabismus would not be owing, like club-foot, to a permanent muscular contraction. It would depend solely on the physiological action of, or a habit of contraction in certain muscles, an opinion which is confirmed by the instantaneous return of the eye to its proper situation and movements, in most cases, as soon as the sound eye is covered. From this it would result that, although it was the success of tenotomy in the contractions of limbs which led M. Stromeyer to propose, and Dieffenbach to perform the section of muscles of the eye in strabismus, there is no analogy in the two cases, since the effect of the operation in strabismus would not consist simply in the destruction of a physical resistance but rather in the modification of a physiological action, and in the establishment of harmony in the muscular contraction of the right and left sides. Such would be the condition of the success of the operation, if this be confirmed by time." (Bull. de l'Acad. de Méd.)

The most valuable information we possess, however, on the state of the muscles of a squinting eye have been derived not from *post-mortem* examination, but from observations made during operation. "In the operation for squinting," says Mr. Guthrie, "the muscle usually divided does not appear to be in the least diseased, but on the contrary quite in its natural state." In many instances an hypertrophied state of the muscle is reported to have been met with. Thus, Mr. Lucas tells us (p. 28), he has "had many opportunities of witnessing the great development of the inner rectus muscle in convergent strabismus. . . . In such cases the muscle was not merely increased in bulk, but was also much more vascular, and of a deeper colour than natural—conditions which

* Quoted in Mackenzie's treatise on the Eye, from Journal Hebdomadaire, tome i, p. 309; Paris, 1836.

remarkably contrast with the appearance the muscles of the eye present in their healthy state."

Is such hypertrophy of the muscle cause or effect?

The following account of the state of the conjunctiva and muscles of the eye in strabismus observed by Dr. von Ammon during operation, corresponds much with what is related in the other works before us, but as it is so methodically and concisely stated, we think it worthy of being quoted in full.

"Besides the contraction of the inner part of the conjunctiva bulbi met with in cases of convergent strabismus, I have always observed a remarkable blanched appearance and not unfrequently dryness of it or diminished mucous secretion, and perceived on dividing the membrane an abnormal density and thickness of it. This is owing either to an actual thickening of the conjunctiva itself, or to separate layers lying close over each other. Not unfrequently, however, I have seen, after the division of the conjunctiva, several bands of membrane going from the sclerotica to the inner wall of the orbit, so that I must consider this as a distinct pathological appearance of frequent occurrence. In other forms of strabismus, such as divergent, I have not perceived anything similar at other parts of the conjunctiva. As to the morbid condition of the muscles in strabismus: when the inversion was very great, the insertion of the internal rectus was often found farther back than usual. I have observed the same thing in divergent strabismus; in the majority of cases, however, the insertion of the muscle was normal. As regards the muscular substance itself, it appeared to me sometimes thick, gorged with blood, pouring out after division a considerable quantity of blood, and less easy to divide than a sound muscle: in such cases it was rather round than flat... Sometimes it appeared as if the muscle was very tendinous, and then either very thin and shrunk, or tough and pretty thick; it had in that case lost the peculiar muscularity almost entirely, so that the division of it was attended with a creaking sound. Very often, however, I have observed nothing abnormal in the muscle the subject of operation, whether in colour, consistence, or length." (§ viii. pp. 15-6.)

Mr. C. R. Hall has met with cases of double convergent strabismus in which the internal rectus of the less inverted eye was much larger and stronger than that of the more inverted eye.

The more marked organic changes which have been described occurred in cases obviously of luscitas: thus, in two cases, Mr. Duffin found the investing membranes of the muscles almost cartilaginous, and so contracted and unyielding, that the patients were wholly unable to move the pupil out of the inner canthus previously to operation. (p. 78.)

In strabismus convergens, is it the action of the adductor or abductor which is at fault? If the adductor, it must be in a state of tonic spasmodic contraction, with this peculiarity, that the spasm goes off when the other eye is closed, and immediately returns when it is again opened; and in many cases with this further peculiarity, that when by closing the previously well-directed eye the spasm goes off in the habitually squinting one, it comes on in the other.

Is it the abductor which is at fault? The abductor "is not absolutely paralysed, for on closing the sound eye, it evidently exerts its proper function; but from some cause, to us unknown, as soon as the sound eye is again opened the muscular force of the abductor is no longer able to support the eye in its natural direction, so that the distortion immediately

Unnatural prominence of the eyeball. After the division of the internal rectus muscle the eye becomes prominent, and if the eyes are naturally sunken, the eye operated on contrasts much with the sound one which remains sunk, the effect of which on the whole physiognomy is very striking. For the mere purpose of rendering the sunken eye which did not squint prominent, like its fellow which had been subjected to operation, Mr. Lucas has divided its inner rectus muscle "with," he says, "the most satisfactory results." (p. 53.)

Dr. Ammon says that the prominence of the eye is much more considerable after division of the internal rectus than after division of the external. Mr. Duffin conceives (p. 22) that the increased prominence of the eyeball after operation depends very much on too free incision of the conjunctiva, and dissection of parts from over the surface of the sclerotica during the operation; and Dr. Ure, in a private communication to us, remarks that he has met with much fewer cases of prominent eye in his later operations, in consequence, he believes, of taking care not to divide more of the conjunctiva than is absolutely essential for the section of the muscle.

The following is the test laid down by Mr. Duffin by which to decide whether the operation is completely performed: "When the operation is complete in every respect, and only *one eye is affected*, no other muscle being implicated than the adductor, the patient is wholly incapable of directing the pupil inwards beyond the axis of the orbit, either in a horizontal or in an oblique line." (p. 9.) Mr. Duffin further says (p. 66), "when an attempt is made to look at the nose, if the operation be complete, the pupil, instead of being inverted, is depressed, the eyeball being drawn directly downwards by the action of the inferior rectus muscle."

Myotomy in alternating and double convergent strabismus. In a preceding part of this article we noticed the tendency which the apparently sound eye has to turn in when, on account of its being covered, the habitually distorted eye becomes straight. We also mentioned that this circumstance has a powerful influence on the result of the operation. It is therefore of great consequence to examine whether or not both eyes are inclined to turn in, lest, as Mr. Lucas says, "when one eye was restored (by operation) to a natural position, the other should become inverted; and although this is actually what occurs in such cases when the inner rectus muscle of one eye in double convergent strabismus is divided, yet, if the surgeon was not aware of the fact, and did not prepare both the patients and their friends for the consequence on the eye opposite to that operated upon, he would get the credit of substituting one squint for another." (p. 48.)

To ascertain the state of both eyes and the actual amount of deformity which exists, Mr. Lucas has found the following method most satisfactory:

"If it is the right eye which is inverted, and the patient is employing the left for vision, I place my hand obliquely over the left eye in such a manner as to hide all objects in front of it, but keep the hand sufficiently raised at the temporal margin of the orbit to enable me to watch its movements. I then desire the patient to exercise the eye which is uncovered, and if at the same time he brings it to the centre of the orbit, the covered one retreats into the inner canthus, the case is one of double convergent strabismus, and both eyes will require to be operated upon; but if the contrary occurs, if both eyes are at this period straight, or even if the covered eye has but a slight inclination inwards, the case

is one of single convergent strabismus, and the inner rectus muscle of one eye only will require to be divided." (p. 49.)

In double convergent strabismus, when the internal rectus of one eye is cut, the opposite eye becomes still more inverted; both internal recti therefore should be cut at the same time. Cases of double convergent strabismus have been reported, however, by Zeis and C. R. Hall, in which the operation on one eye was followed by the cure of both.

In cases of single convergent strabismus, the eye sometimes remains inverted notwithstanding the section of the internal rectus.

"Notwithstanding," says Mr. Lucas, "that the muscle be divided in the most satisfactory manner, and even a portion of it cut away, the eye in many cases will be found still inverted. I have ascertained beyond all doubt that this inversion is owing to the condition of the submuscular and subconjunctival fasciæ; and upon freely dividing these, both upwards and downwards, with a forceps and a pair of scissors, the eye in most cases will become perfectly straight."

Other operators insist much on the same circumstance. Mr. Duffin says (p. 7) that though the muscle may be properly divided, still if some portion of contracted fascia, condensed cellular tissue, or perhaps some apparently insignificant band of fibrous adhesion be left, the globe will remain *tethered*, and, though no longer under the influence of the internal rectus, may, when moved downwards or upwards by the superior or inferior rectus, be at the same time drawn somewhat inwards by the dragging of the frenum which is left; so that the operation will be but half successful. In cases such as the above, if the frena have not been divided, Mr. Duffin has not found that time has effected any amelioration on the malposition of the eye.

It sometimes happens, especially when both eyes are implicated, that the patient still has a slight cast when he looks with both eyes at an object placed directly before him. This remaining power of inverting the pupil is believed to be owing to the action of the inner fibres of the superior and inferior recti, aided, it is said, in a few instances by the superior oblique, which, when the squint is considerable and has been of long duration, it has been supposed may likewise have contributed to its production. Mr. Liston, in order to remedy the remaining inversion, cuts the inner fibres in question; but Mr. Duffin prefers, when the opposite eye is affected, to liberate it when he finds that both go right. By dividing the inner fibres of the superior and inferior recti, the eyeball is apt to protrude.

When the internal rectus has been divided and the eye still remains turned in, Mr. Elliot, of Carlisle, recommends the internal rectus of the opposite eye, although sound, to be cut across in preference to the dividing any other muscle or muscles of the affected eye. He speaks strongly in favour of this plan, and says he has had several cases completely cured by it. (Lancet, Oct. 31, 1840.) In further illustration of the influence of division of the muscle of the opposite eye, we may cite the remark of Dr. Ammon, that he has never found the strabismus return in a high degree in cases in which both eyes were operated on at the same time, whilst he has frequently seen complete relapse when one eye only was subjected to the operation.

What is here supposed by Mr. Duffin, it will be seen, are realized in Mr. Calder's case.

Divergent strabismus. In divergent strabismus the patient is incapable of directing both eyes inwards simultaneously, though he has power over the adductors separately. This, Mr. Duffin remarks, is precisely the degree of power over the eyes observed in persons who have had one or both eyes successfully operated upon, for convergent strabismus, a sufficient time having elapsed to admit of a reunion of the divided tendon to the sclerotica. (p. 102.)

Of the operation on his 23 cases of divergent strabismus, Mr. Duffin says, "very few have been strictly speaking *perfectly* successful, although they have nearly all been improved in a considerable degree." (p. 100.) Mr. Duffin further says:

"When the external rectus is cut across, we do not find the eye instantly start into its proper position, as it does when the internal muscle is divided. The pupil goes more gradually to the axis of the orbit, as if it were not drawn there by an active force, but went there because the restraining power no longer opposed the movement. It requires, indeed, several days in some instances before it is reinstated. After the operation, moreover, the capability of moving the eyes towards the nasal canthus by an effort of the will remains precisely the same as before. Nothing has been gained in this respect; the patient has not acquired the power of directing both eyes inwards simultaneously; in fact, all the apparent advantage obtained is that, when quiescent, the pupils of both eyes occupy the visual axis of their respective orbits, instead of only one doing so, while the other is directed outwards." (Duffin, pp. 104-5.)

Mr. C. R. Hall's treatment of divergent strabismus appears to have been more successful. "In two instances only," says he, "(out of 13) has division of the rectus externus failed to remedy divergent strabismus. But in 6 of the 11 successful cases, the cornea has only by degrees attained its proper situation."

Mr. Elliott, of Carlisle, has applied his plan of dividing the corresponding recti muscles of both eyes to two cases of divergent strabismus with success. The first case was that of a fine girl, aged thirteen, whose left eye on her looking at any object placed before her was straight, while a portion of the cornea of the right eye was concealed within the *outer* canthus. Mr. Elliott divided the tendon of the right abductor, and on the girls then looking straight forward with the left, a small portion of the albuginea of the right eye was seen *external* to the cornea, which was still far from occupying the axis of the orbit, the eversion continuing confined to the right eye; the left abductor was then divided and the cure was at once perfect. An equally happy result followed the adoption of this operation on a gentleman whose left eye had been thirty years divergent. In both cases the abductors were found small and weak; in both the division of the abductor of the everted eye failed in entirely removing its deformity, and in both the division of the abductor of the sound eye effected the instant and complete straightness of both globes. The power of abduction of each eye was, to all appearance, perfect and natural after the operation. Mr. Elliott has also found that the power of inverting the eye, though in different degrees, indisputably exists, after the division of its internal rectus, in every case on which he has operated, and in which, nevertheless, the strabismus was quite removed. This, it will be seen, does not coincide with the experience of Mr. Liston, Mr. Duffin, and others.

Dieffenbach, Ammon, and Baumgarten mention that they have observed nystagmus bulbi removed by *myotomia ocularis.*

Changes which take place in the ocular muscles after their section. The muscle, upon its tendon being fairly divided, retracts within the orbit, and afterwards unites firmly to some part of the sclerotic coat. In six cases in which Mr. Lucas had to operate a second time, he ascertained this to be the case. (p. 79.)

In a case in which the external rectus muscle was cut by Mr. Babington on the 1st of December, the operation was followed by rather more inflammation than usual, but this subsided in a few days. A month after, the patient died of tuberculous pneumonia, and the following was the state of the muscle implicated in the operation, observed on dissection by Mr. P. S. Hewitt, curator of the museum of St. George's hospital:

"It was found that the external rectus had been completely divided, just at the point where it is beginning to be tendinous; that the muscle itself had retracted to the extent of about three quarters of an inch from its natural attachment, but that it still remained connected with the globe by a strong band of cellular tissue. This band was about three lines in width and about six lines in length, and was attached to the ball of the eye about two lines behind the original insertion of the muscle; and such was its strength, that it allowed of being pretty forcibly pulled upon without giving way. There can be no doubt that this band consisted of the loose cellular membrane which naturally connects the muscle with the globe, stretched out into an elongated form by the retraction of the muscle, and afterwards condensed by inflammation."*

When, after the operation, the squint comes on again, Mr. Lucas has ascertained, he thinks beyond all doubt (p. 81), that this second inversion arises from the muscle forming new adhesions to the tunica sclerotica, and that these adhesions were anterior to the transverse axis of the eyeball. In those cases in which he had to operate a second time, the mark of the original attachment of the muscle was seen, and a little behind this point its new attachment, on dividing which, the eye again became straight and remained so. The readherence of a muscle, after its division, to an unfavorable point of the sclerotica, can be best guarded against by excising a portion of it, and this particularly where the muscle is unusually developed.

In twelve cases in which Dr. Baumgarten, of Dresden, (Ammon's Monatsschrift, vol. iii, p. 474, Leipsig, 1840,) operated a second time, he found in eight *immediate reunion* of the muscle, and in four *mediate reunion.* In the latter case the intervening substance was about two lines broad.

Statistics. The operation of myotomy for strabismus appears from the reports to have been successful in the majority of instances; still many cases have occurred which from their want of success and the phenomena attending them, show that we have still something to learn on the nature and treatment of strabismus.† Besides we have yet to be informed of the

* Medical Gazette, Jan. 22, 1841.

† In a communication to the British Association, September last, Dr. Ure stated that the result of the operation was more satisfactory in those cases in which the distortion had originated from some external cause, as irritation, local injury, ophthalmia, and the like, than when it arose from some disorder of the parts within the head.

permanency of the cure in the many cases which have been reported as successful, but a *few days* after the operation.

In regard to the ages of patients operated on:—The operation has been performed at almost all ages. The following were the ages of Mr. C. R. Hall's 200 patients. From two to three years, 1; from three to four, 3; from four to eight, 11; from eight to fifteen, 48; from fifteen to twenty, 44; from twenty to thirty, 58; from thirty to forty, 26; from forty to fifty, 5; from fifty to sixty, 3; sixty-two years, 1.

In young persons, Mr. Duffin judiciously remarks, the squint should always have subsisted for at least a year or two before an operation is attempted. In Mr. C. R. Hall's cases the duration of the strabismus was as follows: From one to three years, in 7 cases; from three to six, 11; from six to ten, 20; from ten to fifteen, 41; from fifteen to twenty, 49; from twenty to thirty, 32; from thirty to forty, 12; from forty to fifty, 6; from fifty to sixty, 1; sixty-one years, 1. The duration could not be ascertained in the remaining 20.

In Mr. Liston's cases the date of the deformity was as follows: under five years, 14 cases; under ten, 17 cases; under twenty, 48 cases; under thirty, 24 cases; under forty, 7 cases; under fifty, 3 cases; under sixty, 1 case; unknown, 9.

The subjoined statements are the most definite we have met with in regard to the proportion of success attending the operation:

In a private communication Dr. Ure states that "the following numbers represent approximately the results in a hundred of a large amount of operations for strabismus, performed by him since the month of June, 1840. Obliquity removed, 75; obliquity diminished, 16; not affected, 9.

Of 52 cases in which Dr. Baumgarten (Ammon's Monatsschrift, vol. iii, p. 485,) performed the operation of myotomy on one or other eye, 33 were perfectly cured, 17 considerably improved, and 2 rendered worse by the supervention of strabismus divergens. In twelve of the successful cases, the operation had to be *once* repeated before the cure was effected, and in one case it had to be *three times* repeated.

The number of cases in which Dr. Ammon has himself been the operator or which he has seen operated on by Drs. Zeis, Baumgarten, and Warnatz of Dresden, is 72. Of these 45 have been quite successful, 13 less so, and 14 unsuccessful. Of the 45 successful cases there were three in which the operation had to be performed twice. Had the operation been performed in the 13 less successful cases, Dr. Ammon thinks that complete success would have been obtained. In the 14 unsuccessful cases, the highest degree of strabismus or luscitas was, for the most part present, or the persons were very old or very young. Nine times was the external rectus, and sixty-three times the internal rectus divided. In two cases, divergent strabismus came on after the operation. The operation was performed 20 times on the internal rectus of the right eye, 43 times on that of the left; 6 times on the external straight muscle of the right eye and 3 times on that of the left. Six times the internal recti and once the external recti were cut, the one immediately after the other.

Franke of Leipsic operated on 28 cases, of which 23 were of convergent strabismus and 5 of divergent. The results were: perfect restoration of the form and functions of both eyes in 3. Inconsiderable equal con-

vergence of the two eyes, both of which are employed in vision at the same time, in 5; in two of which double vision of some months' continuance, on moving the eyes in the direction of the cut muscle. Normal position of the axes of both eyes, with difficult or hindered motion of the eye operated on towards the side of the divided muscle, 6. Normal position of the eye operated on, inversion of the other more or less, 7. In 4 cases the operation was unsuccessful. Of the 3 remaining cases, one was of convergent strabismus with nystagmus, and one of divergent strabismus; in these some improvement only seems to have been effected. In the last case the result on the squinting is not stated, but severe inflammatory reaction was the immediate result of the operation.

Accidents. Accidents have in some cases attended or resulted from the operation: thus, the eyeball has been cut into, and the vitreous humour evacuated; hemorrhage has occurred to a dangerous extent, rendering transfusion necessary; inflammatory chemosis has not unfrequently occurred. Mr. Lucas mentions (p. 80) that he has heard of abscess within the orbit forming after the operation. M. Phillips (p. 20) refers to similar cases. After speaking of various untoward results of the operation when performed with too much disturbance of parts, Mr. Duffin says (pp. 23-4), " but these are minor evils compared with sloughing of the eyeball and total loss of vision, an event which I know has happened when a clean dissection has been rather too extensively made of the sclerotica." At (p. 51) Mr. Duffin mentions that a girl was shown to him who had been operated upon, and whose eyes afterwards turned spasmodically in every possible direction. On enquiry, it appeared that she had suffered from chorea, and that the squint (which was intermittent) was a sequela of that disorder.

We have passed over entirely without notice the section of the oblique muscles of the eyeball, as in the few cases in which the operation has been performed, this appears to have been but vaguely indicated and followed by no very decided result.

Mr. Duffin relates a case in which the pupil, turned inwards and upwards, was after the section of the internal rectus turned right upwards, and was restored to its natural position only by the section of the superior rectus. Mr. Duffin never met with any other case requiring the section of the superior rectus, but he mentions one which occurred in the practice of a professional friend. In such cases, in which, *prior to operation,* the pupil is turned upwards and inwards, Mr. Duffin conjectures there may exist paralysis, either partial or complete, of the inferior rectus.

The division of the inferior rectus, Dr. von Ammon remarks, is very difficult but perhaps never required.

We must now conclude this article, and will do so by stating, in a few words, our opinion of the several works that have furnished the materials for it.

The dissertation of Melchior, it will be seen from the date, appeared before the introduction of ocular myotomy into practice. He refers to it, however, as applicable to cases of luscitas. The phenomena and nature of strabismus and its cure by exercise are discussed with great care and scientific accuracy, and the dissertation adds another to the

many excellent ones from the medical school of Copenhagen which have come before us. The plan of Mr. Bennett Lucas's treatise is well conceived, and the execution of it at once scientific and practical. There is much less method in Mr. Duffin's book; and although it abounds in excellent observations and just remarks, we think the author has fallen into the error of trying to make too much of his subject. This objection may also in a great degree be urged against the tract of Dr. von Ammon. In regard to Mr. Calder's work, perhaps it would have been as well if it had appeared, as originally intended by the author, merely in one of the journals. The contrary of this remark we might apply to the very excellent article of Mr. C. Radclyffe Hall in the Medical Gazette. Of the production of M. Phillips we cannot speak in terms of any praise, because we think that in "blazoning forth his hundred examples of untarnished success,"* the author's object has been *ad captandum*, an object which, we blush to say, has been but too evident in many of the writings which have appeared in this country on squinting during the past year.

It will have been observed that we have refrained from giving any opinion as to the claims which the treatment of strabismus by "surgical means" may have "to rank as an improvement in modern surgery," because (to continue to use the words of Mr. Duffin, p. 2,) "it would be injudicious to do so unhesitatingly till a sufficient time shall have elapsed to establish its true merit to be considered as a permanent and harmless cure."

Since the foregoing pages were in print, we have received the small volume of "Practical Essays," by Sir Charles Bell.† The third essay is entitled, "On Squinting; its causes: the actual condition of the eye, and the attempts to remedy the defect."

This essay, we must confess, is not characterized by any very close or connected reasoning; and in the endeavour to illustrate his subject by lights drawn from the physiology of vision, we think Sir Charles has not been very successful. As it would therefore be little edifying to attempt either an analysis or critique of the essay, we shall content ourselves with quoting the following statement, in which we in a great measure agree with the author: "The more that any one knows of the fine adjustment necessary to correct vision with both eyes, or the more he thinks of the combination of muscles accessory to vision, the greater must be his surprise that an operation so rude as that of dividing one of the muscles, should have the effect of curing squinting. Reasoning *à priori*, one would say, that the effect must be to produce double vision, by bringing the images on the retina nearly, and not absolutely, to a correspondence; and the surprise is rather increased than allayed by the fact that in some instances it has the effect referred to. Why, then, is it not the same in all? Because the person continues to see with one eye only." (pp. 72-73.)

* Mr. Duffin's preface, p. xiii.

† Edinburgh, 1841, 8vo.

ART. XII.

The Anatomy of the Nerves of the Uterus. By ROBERT LEE, M.D. F.R.S., Physician to the British Lying-in Hospital, &c. *With Two Plates.* —*London*, 1841. Folio, pp. 8.

THIS work is divided into two chapters, the first of which is devoted to a sketch of the history of the subject, and the second to a detail of the author's dissections of the nerves of the uterus. Reflecting on the remarkable functions of the uterus, its nervous system might *a priori* be expected to be much developed; yet, hitherto, little more than a few nervous filaments have been demonstrated in it. We must therefore consider that a worthy labour in which Dr. Lee has engaged, and for the communication of the results of which he is entitled to our best thanks. From the sensibility and contractile power of the uterus, Galen inferred that it must be supplied with nerves, but he says they are extremely small, compared with the size of the organ. The description which Vesalius gives of the nerves of the uterus, is very imperfect. It appears that Willis traced the hypogastric and sacral nerves to the neck of the uterus, but no farther, and that he was unacquainted with the spermatic nerves. In De Graaf's seventh plate, the trunk of a nerve is represented passing on each side into the posterior surface of the neck of the unimpregnated uterus, and ramifying like the branches of a tree over the body and fundus. Saltzman and Reuss, Vater, Rast, Maier, Daventer, and Winslow, all state that the uterus has nerves, but their works contain no new facts on the subject. J. G. Walter's first plate gives a very defective view of the nerves of the uterus in the unimpregnated state; a few small filaments only being seen passing into the lower part of the orifice and cervix from the upper part of the hypogastric plexus. The fundus and body of the uterus are left covered with peritoneum, and the spermatic nerves are not traced. Haller appears likewise to have confined his attention to the nerves of the unimpregnated uterus, and with little more success than Walter and other anatomists before him had done.

Dr. William Hunter was the first anatomist who examined the nerves of the gravid uterus, and who suspected that they enlarge during utero-gestation, in some proportion like the vessels. Mr. John Hunter, however, denies that the nerves of the uterus are increased in the smallest degree during pregnancy; but neither he nor his brother have left any preparations to demonstrate the accuracy of their respective statements. From his assumption that the nerves of the uterus are not enlarged in the time of pregnancy, Mr. Hunter infers that the nerves and brain have nothing to do with the actions of a part.

Dr. Lee is not aware that, except those now to be described, any preparations exist, or ever have existed in this country, showing the nerves of the uterus dissected, either in the unimpregnated or gravid state. Some anatomists believe that it is impossible for the nerves of a part to increase under any circumstances; an assertion confuted in the clearest manner by the dissections by Mr. Newport of the nerves of insects. The same authors think that the intense pain and violent periodical contractions during labour do not depend upon the nerves, but upon some incomprehensible *vis insita* in the organ.

The following is Dr. Lee's account of the nerves of the gravid uterus in the fourth month, as recorded in his fifth dissection.

"In October, 1840, I finished the dissection of a gravid uterus of four months, all the arteries and veins on the right side of which are completely filled with red and blue injection; and the whole nervous system of the uterus more perfectly displayed than in any of the preparations already described. The uterus was removed from the body of a woman who died in St. George's Hospital, from an external injury, and the fœtus and its appendages were expelled a few hours before death. The nerves were traced while the uterus was covered with rectified spirit.* An artery of considerable size filled with injection is seen accompanying the hypogastric nerve, and passing along with its branches through the hypogastric plexus to the ganglion at the cervix. In this course, the artery is seen ramifying upon the trunk of the hypogastric nerve, and the most minute branches of the hypogastric plexus. The sacral nerves passing into the ganglion are also accompanied with an artery, which is likewise injected, and which passes through the centre of the ganglion. These nerves are a little smaller than in the uteri of nine months. The ganglion is thick, large, and distinct, of an oblong form, about three quarters of an inch in diameter, and consisting of gray and white matter. From its inferior border, three large bundles or masses of nervous fibres are sent off, which present an appearance resembling the pes anserinus of the portio dura. The posterior of these subdivides into numerous small branches, accompanied with arteries which supply the rectum and back part of the vagina. The middle of these great nerves proceeding from the ganglion, likewise accompanied with arteries, ramifies upon the side of the vagina, and the anterior upon the bladder, around the entrance of the ureter. From the hypogastric plexus, before it enters the ganglion, and from the inner surface of the ganglion, numerous large and small branches of nerves are given off to the neck of the uterus, some of which accompany the blood-vessels towards the fundus, and others spread out under the peritoneum. All these are likewise accompanied by injected arteries. From the inner border of the ganglion, a broad nervous band is sent off, which passes on the outside of the ureter, and another on the inside, which unite and completely surround the ureter. From these united nervous bands, many large branches are sent to the back part of the bladder, and into the anterior part of the cervix uteri. The course of these branches can be easily traced by their injected arteries. On the lower and anterior part of the cervix uteri, over the mesial line, there is a thick membranous expansion, into which these nerves enter from both hypogastric plexuses and ganglia. From the sides and upper part of this membrane, there are given off innumerable filaments, apparently nervous, which unite on the sides of the uterus, with the nerves accompanying the blood-vessels, and with the spermatic nerves, and some of which pass out with the round ligaments. These are likewise accompanied with minute arteries, as all the nerves are on the right side of the uterus, entering the ganglion and passing out from it. The nerves and ganglion on the left side correspond in appearance with those of the right, and the greater number of the spermatic nerves on both sides accompany the spermatic veins."† (p. 7.)

* It is only under spirit that dissections of this kind can be properly made. So long a time is necessarily occupied, that if waterwere used, the parts would putrefy before the dissection could be completed. To attempt tracing the nerves of such a structure as the uterus except under a clear liquid, would be vain.—Rev.

† "Analogy led me to suspect," says Dr. Lee, (p. 7,) "that many branches of nerves would also be found, on examination, to accompany the veins of the spermatic cord, and to ramify upon their coats. Mr. James Dunn, at St. George's Hospital, undertook, at my request, to ascertain if this were the fact, and in July last he made three preparations, in which the nerves are seen covering the veins as they pass out of the testicle. It appears from these, that a much greater number of nerves accompany the veins, than the artery in the spermatic cord. The nerves in these preparations, form a great plexus around the veins, and are traced into the testicle."

Whether Dr. Lee is correct in considering as nerves all the bands and filaments he has met with in his dissections, and which we believe are faithfully delineated in the plates, it is not competent for us to say, nor is it for any one who has not carefully gone through the investigation of the whole subject himself; a task we are aware of no little pains and labour. In these remarks, we allude particularly to the membraniform plexus under the peritoneum, on the body of the uterus, into which there is an evident continuity of branches from what are indisputably nerves. This structure is evidently the same as that alluded to by Lobstein, in the following quotation which Dr. Lee gives from his monograph on the sympathetic nerve: "Hac occasione," says Lobstein, "monendum esse duco, quod, tunica uteri externa sublata, multæ fibræ occurrunt, quæ vario modo sese decussant, et, ope telæ cellulosæ laxæ, tam inter se, quam substantiæ uterinæ profundiori atque densiori, uniuntur. Hæ fibræ, quorum indolem ignoro, facile pro continuatis nervorum ramulis habentur, a quibus vero, non solum ratione directionis atque crassitiei majoris, sed etiam ratione figuræ suæ magis complanatæ, differunt."

It is evidently also the same structure which is described by Madame Boivin and Professor Duges as "a filamentous tissue between the peritoneum and muscular coat of the uterus, and fibres extending longitudinally 'en avant et en arrière sur la region médiane du viscère de son corps aux moins,'" (see p. 4,) but which, as Dr. Lee remarks, they have not alluded to as having any connexion with the nerves of the uterus. In farther illustration of the history of this subject, Dr. Lee continues: "Dr. Baly states that he met with the following isolated passage in a paper by Schwann—the flat bands under the peritoneum were similar to those described by Lobstein. 'In the uterus of a woman who had borne a child at the full period of pregnancy, I found, immediately under the peritoneal coat, not muscular fibres with transverse striæ, but long very flat bands of different breadth, which presented very indistinct longitudinal striæ, (Langstreifung.) In some few, I could distinguish a flat, pale nucleus, very much elongated, and containing a smaller corpuscle.'"

We have seen from Dr. Lee's historical review that Dr. William Hunter believed that the nerves of the uterus enlarge during pregnancy, but that Mr. John Hunter distinctly denied this. Dr. Lee's dissections can scarcely leave any doubt that the nerves do enlarge during pregnancy, but as to how this enlargement takes place requires yet to be determined.

That the nerves of the uterus, after having performed their proper functions during gestation and labour, gradually return to the condition in which they are found in the unimpregnated uterus, Dr. Lee thinks is made certain by the following observation:

"On the 27th of June, 1840, I examined the uterus of a woman who died suddenly on the tenth day after delivery. The hypogastric plexus, and those both on the anterior and posterior surfaces of the body of the uterus, were very much reduced in size from what they were observed to be in the uteri of six and nine months." (p. 7.)

Dr. Lee concludes the account of his dissections of the human uterus with the following remarks:

"These dissections prove that the human unimpregnated uterus possesses a

great system of nerves, which enlarges with the coats, blood-vessels, and absorbents during pregnancy, and which returns after parturition to its original condition before conception took place. It is chiefly by the influence of these nerves that the uterus performs the varied functions of menstruation, conception, and parturition, and it is solely by their means, that the whole fabric of the nervous system sympathises with the different morbid affections of the uterus. If these nerves of the uterus could not be demonstrated to exist, its physiology and pathology would be completely inexplicable." (p. 8.)

Nine dissections altogether of the nerves of the human uterus, both in the impregnated and the unimpregnated state, are recorded in the work before us; an account of a dissection of the nerves of the uterus in the mare is also given which will be found to present some interesting analogies.

Art. XIII.

Human Physiology. By John Elliotson, m. d., f. r. s., &c. &c. *With which is incorporated much of the elementary part of the Institutiones Physiologicæ of J. F. Blumenbach,* m.d., f.r.s., *Professor in the University of Göttingen. Illustrated with numerous Woodcuts.* —*London,* 1835-40. 8vo pp. 1194.

The large space which we have been of late devoting to the discussion of physiological topics must be our excuse for not entering upon a full review of this work. The peculiarities of its character are such as to forbid our treating it with brevity if we enter at all into detail, and we are therefore compelled to dismiss it with such a general notice as may serve to convey to our readers an idea of its nature, and of the place which it holds in our estimation. We shall begin with a sketch of its history, which is not a little curious. In 1815, Dr. Elliotson translated the third edition of Blumenbach's "Institutiones Physiologicæ," anonymously, with the addition of twenty pages of notes; and in 1817, he published a new edition with his name and the addition of 150 pages of notes; subsequent editions were published in 1820 and 1824, in the last of which, the amount of Dr. Elliotson's notes greatly exceeded that of the text. Finding in the present edition his own matter would greatly exceed that of Blumenbach, and that much of the original would require emendation on account of recent discoveries, or might be better omitted, and that the disjointed nature of the work would be a source of greater inconvenience to the reader than ever, Dr. Elliotson resolved to remodel the whole, omitting many parts of the original, and blending his notes with as much of it as he could retain; and, as the portions of the original retained were of so much smaller amount than his own labours, and of a very elementary character, and as the proportions of Blumenbach's share and his own were thus completely reversed, Dr. Elliotson felt justified in giving his own name to the work. Now on this we must remark, that we do not think the formation of this kind of literary partnership, without the permission of one of the parties, at all justifiable. So long as Dr. Elliotson's additional matter remained in the form of notes, and Blumenbach's text was given unmutilated he had an undoubted right to append whatever he thought proper; but by putting his own name above that of Blumenbach on the title-page, and retaining just what suited his purpose of the original whilst his own new matter is not readily to be distinguished from it, he has

in effect implicated that philosopher (in the mind, to say the least, of the indiscriminating reader) in the expression of his own opinions, with many of which we doubt not that the Professor of Göttingen would have been very sorry to have been identified.

Further, it is evidently impossible that a work thus concocted should have as much unity as one designed and executed by a single mind; and the bulky volume before us bears many indications of deficiency in that method and conciseness which are especially required in medical treatises, and of which Blumenbach's Institutiones were (at the time of their publication) an admirable specimen. Moreover, the long space, no less than five years, indeed more nearly six, which has intervened between the publication of the first and last parts, is fatal to the excellence of a work which, as its learned author doubtless hoped and expected, might, from its size and comprehensiveness, have been a standard treatise on the subject. The evil that has resulted from it is evident from the fact that, when the "small remaining part," which was for some years advertised as "just ready," at length made its appearance, it was found to contain nearly as much as the other two put together. Now this augmentation results (in part at least) from the very laudable desire on the part of the author, to give the fullest and most recent information on the subject to which it is devoted; but have not those discussed in the former parts advanced with equal rapidity, and is not the unity of the whole work thus completely destroyed? It is true that an Appendix might in some degree have rectified the evil, and an Appendix we find; but what does it contain? With the exception of a short account of Dr. Beaumont's experiments on digestion, it is entirely occupied with discussions on the author's two favorite subjects, Phrenology and Mesmerism, to which nearly fifty pages are here devoted, in addition to about eighty in the body of the work; and what is said of the former of these subjects almost exclusively concerns the relative personal merits of Gall and Spurzheim, instead of communicating to the enquiring student the progress of this department of science, and the evidence upon which its conclusions are based.

That the volume contains a vast amount of sound physiological information, and that it exhibits great learning and research, we most readily admit; but we cannot think it on that account a useful guide to the physiological student; for, whilst some of the subjects are fully and ably discussed, others of no less importance are very superficially handled, and the whole exhibits a want of that calm philosophical tone, for the absence of which no brilliancy or acuteness can compensate. The work is, nevertheless, a very amusing one, and is rendered so by the great variety of illustration which the author's various learning has enabled him to supply, (some of which, however, we are obliged to say, is of such a coarse description as a scientific work ought by no means to contain,) as well as by the very laughable egotism which pervades the whole, and which causes the learned doctor to launch out occasionally into tirades of a very ludicrous description, against all who differ from him. There is, for instance, a note of nearly four pages long, under the head of Phrenology, containing the history of all the improvements in medical practice to which Dr. Elliotson has contributed, and of the opposition which they at first encountered among the profession; the *gist* of which is to show that, as phrenology is laughed at by the ignorant and conceited, it is all the more true. A summary of this argument afterwards occurs under the head of Mesmerism; and

we cannot give a better specimen of these amusing digressions than by quoting it.

"I have never yet declared an opinion upon a new truth that I have been obliged to retract. Phrenology has now advanced to its firm establishment; human glanders is universally admitted; auscultation is invariably practised except by the wretchedly ignorant; quinine, prussic acid, and creosote, are now in daily use. I stood abundant ridicule for advocating these, and will now stand more ridicule with the same firmness and the same silent pity or contempt which I have always felt for my opponents, till I see, as I shall, the truth of Mesmerism also admitted, and the world forget that it was ever doubted." (p. 686.)

The following piece of amusing sarcasm will serve as a specimen of the clever illustrations, not so remote from the subject or destitute of fundamental truth, with which the work is enlivened:

"The elderly man no longer likes new lights in science, nor improvements in institutions and methods. If he is a physician, he scorns new-fangled remedies and revolutionary discoveries in physiology and pathology, and tells of the number of follies he has seen prevail and pass away in his time; he acquires more confidence daily in nature's power of curing disease, being left daily more below the point of present knowledge, practising more and more feebly and uselessly, but more greedy than ever of fees, as though he was more informed and did more for his patients than ever. His feelings grow blunt, except his appetite for food and money; remembering his former pleasures, and being unsusceptible now of what he once was, he praises the past only, and condemns the present state of things, '*laudator temporis acti.*'" (p. 1022.)

Dr. Elliotson's work abounds in examples of similar acuteness in the mind of its author in regard to the faults of others, but he appears lamentably blind to his own. He reminds us much of the American original spoken of by Miss Martineau, (to whom, it is elsewhere related, this gentleman was thus purposely presenting a reflection of her own mind,) who told her that a long course of experience had convinced him that he was always in the right, but unfortunately he could not persuade other people to be of the same opinion. At the conclusion of the last part, he calls upon us to yield our assent to a case of transference of sensation, (a phenomenon which confessedly involves so many sources of fallacy, that no philosophic mind would credit it, except upon evidence of the most satisfactory nature,) upon the second-hand authority of his friend and assistant in Mesmeric experiments, Mr. Wood. This gentleman examined in Paris a case of clairvoyance of which the report had found its way to London, and having ascertained it to be a deception, he seems to be considered by Dr. Elliotson as proof against all further imposture, and as entitled to sway the belief of the whole scientific world. Mr. Wood then proceeded to Brussels to examine a case quoted by Mr. Townsend, and not being himself able to detect any fallacy, he came to the conclusion that the supposed phenomenon has a real existence. Dr. Elliotson endeavours to impart additional probability to the inference by a piece of reasoning, which will suffice to show his want of philosophic discrimination in questions of real intricacy. "There appears to me no reason why one part of the nervous system should not, in a new and strange state, acquire properties foreign to it, and possessed by another analogous part of the same system. The nutrient vessels of one organ will deposit in disease what is foreign to their natural function, and one secreting organ in preternatural states will secrete what nature never intended it should. I therefore know no reason why one nerve of sense should not be able to perform the

function of another." (p. 1184.) Now we might admit this, and yet have scarcely advanced a step towards the belief in transference of sensation; for, it must be remembered that the formation of a sensory impression requires, not merely a particular conducting power in the nerve, but a particular apparatus for the production of a change in that nerve. Thus we can very well understand, how a nerve of common sensation might convey the impression of the presence of light, but Dr. Elliotson's explanation leaves us quite in the dark, as to how a definite image of an object can be formed, like that received by the retina, without an eye. Until, therefore, it is proved that this is unnecessary to vision, we must withhold our assent from the whole doctrine, even though Dr. Elliotson should treat *us* with the silent pity and contempt which he bestows so liberally on all who do not coincide with him.

We should not omit to say that, to the general reader, Dr. Elliotson's work will probably be very acceptable, partly through those very faults which unfit it for the professional student.

Art. XIV.

Commentatio de Tumoribus in Pelvi, Partum impedientibus, a gratioso medicorum ordine Heidelbergensi præmio ornata. Auctore B. R. Puchelt, Med. Chir. et Art. Obstetr. Doctore, cum præfatione F. C. Naegele.—*Heidelbergæ*, 1840. 8vo, pp. 253.

A Treatise on Tumours in the Pelvis as a cause of difficult labour; being the essay to which the Prize was adjudged by the medical faculty of the University of Heidelberg. By Dr. B. R. Puchelt. With a Preface by F. C. Naegele, m.d. &c. &c.

This work comes before us with higher claims upon our notice than can be alleged by most prize essays or inaugural dissertations. It is the production of a scholar of Professor Naegele, of Heidelberg, whose recommendatory preface tells us of the diligence, learning, and talent of his pupil; and warns the readers, "ut æqui judices benignam de hoc opusculo ferant sententiam, quippe quæ juvenili auctori præmium sit magnæ, quam impendit, et stimulus incitamentumque futuræ in literis diligentiæ:" nor does the execution of the work discredit this recommendation.

From the great variety of subjects of which the author treats, it will not be possible for us to give anything like a minute analysis of the contents of his book. He divides his essay into two parts; the first treats of tumours of those structures which form the passage for the child, the second of such as are situated in their vicinity, and the whole is preceded by a historical introduction, in which much learning and research are displayed.

These historical notices show how little attention was paid by the early writers to mechanical impediments to labour. The disputes, however, which arose towards the end of the sixteenth century about the propriety of performing the Cæsarean section, led to a more careful examination of those changes in the structure of the pelvis and generative organs which obstruct parturition. Accordingly, Rousset, in his essay on the Cæsarean section, published at Paris in 1581, mentions among

the indications for its performance, tumours and other diseases contracting the natural passages; and Arantius, who wrote a few years later, treats expressly, "de mala ossium pelvis conformatione." He was followed by Guillimeau, who detailed a case of labour obstructed by the presence of a calculus in the bladder, and another in which the same result was produced by excrescences about the os uteri. Many of the observations of succeeding writers are to be found in the works of Bonetus, and Stalpart van der Wiel, which were published towards the close of the seventeenth century, and about the same time the treatise of Sylvius de la Boe appeared, "De exostosibus in pelvi partum impedientibus." Since the time of Levret, Smellie, and Roederer, a more correct estimate has been formed of the importance of mechanical impediments to labour, and each succeeding writer has added something to our knowledge of the subject. Dr. Puchelt's task has been to collect and arrange the labours of his predecessors.

In accordance with the author's plan, *tumours of the generative passages* are treated of first. They may be seated either in the *bones* or in the *soft parts.*

1. *In the bones.* They are of two kinds, being either *true exostoses*, or *osteosteatomata;* but the latter are much more unusual than the former. The author details, in chapter i. several cases of exostoses, and next proceeds to draw conclusions with reference to their seat, causes, diagnosis, prognosis, and medical treatment: an arrangement which he follows throughout each subdivision of the work.

It would appear from the cases of exostosis here related, that the inner surface of the os sacrum is their most usual seat; next in frequency the articulation of the last lumbar and first sacral vertebra, and more rarely the body of the last lumbar vertebra. The causes of these tumours are usually very obscure; in two cases, however, the growth of an exostosis was apparently produced by a fall upon the sacrum. Their diagnosis is very difficult; the prognosis is in all cases unfavorable, though of course modified by the size and shape of the tumour. According to the nature of the case, the application of the forceps, perforation of the child's head, or the Cæsarian section may be necessary; but neither the operation of turning, nor the division of the symphysis pubis has led to a good result in those instances in which it was employed.

2. *Tumours of the soft parts.* A. *Tumours of the uterus.* Chapter i. treats of *sarcomatous tumours*; chapter ii. of *steatomatous growths:* both of which are strangely classed among malignant diseases.

Chapter iii. is on *scirrhus and carcinoma of the uterus.* Thirty-two observations are collected by the author, in which labour was complicated with a scirrhous condition of the uterus.

		cases.
"The entire uterus was affected	in	1
The greater portion of the organ		5
The cervix uteri . . .		11
The cervix and orifice . . .		5
The orifice only . .		6
A portion of the left side . .		1
A portion of the body . . .		1
A portion of the fundus . .		2
		32." (p. 86.)

The great danger to which women are exposed by labour thus complicated is shown by the fact that five out of twenty-seven women died in child-birth, and nine very speedily afterwards; while fifteen of the children were born dead. A scirrhous state of the os and cervix uteri is not only the most common form of cancer of the womb, but also by far the most important in obstetric practice, since the management of the labour involves the difficult question, of how far we may trust to nature, or when and how it will be proper to interfere. Even in cases apparently the most unfavorable, the practitioner must not despair of the powers of nature; for several instances are on record in which delivery has been completed without interference, notwithstanding a scirrhous and perfectly rigid state of the os and cervix uteri. In such cases the head of the child has forced its way by distending and gradually lacerating the neck of the womb; and doubts have been raised whether, as a general rule, the fissures made by nature are not to be preferred to incisions with the knife. Madame Lachapelle objects to incising the cervix uteri, but the late M. Baudelocque was an advocate for the practice, which some years since we saw employed with success by Professor Dubois. In fourteen of the cases related by the author, labour was completed by the powers of nature alone, in three the operation of turning was performed, in four the forceps were applied, and in four rupture of the uterus occurred.

In Chapter iv. the author treats of *cauliflower excrescences from the uterus.* The existence of these growths exposes the patient to danger rather from the profuse hæmorrhage to which it gives rise, than from the mechanical obstacle opposed to the passage of the child.

Fibrous tumours of the uterus form the subject of the fifth chapter. They are important on account both of their frequency and of the serious hazards to which their existence exposes the patient, not only during labour but also in the puerperal state. In some instances these tumours have acquired so large a size that the bones of the fœtal skull have been fractured during the passage of the child. The powers of the mother become exhausted by the long continuance of labour; and fatal hemorrhage has followed delivery owing to the state of atony in which the uterus has been left by its violent action during labour. Fatal peritonitis is another of the accidents of childbed which these tumours are very prone to induce. Mr. Ingleby's paper in the Edinburgh Medical Journal for January, 1839, seems not to have come under Dr. Puchelt's notice; but the observations there made, " On Fibrous Tumour of the Uterus," will well repay a careful perusal.

Chapter vi. *On polypi, or pediculated fibrous tumours.* From the observations related by the author it would appear that the existence of a polypus in the uterus is a less serious complication of labour than that produced by a fibrous tumour seated in the uterine substance.

Chapter vii. *On encysted tumours.*

Chapter viii. *Prolongation of the anterior lip of the os uteri* sometimes results from the pressure of the head or some other part of the child. Three examples of this occurrence are quoted by the author from an essay which M. Duclos published in the year 1818, in the annals of the medical faculty of Paris; and a fourth case is related which came under the observation of Professor Naegele. The œdematous condition

of the os uteri from pressure is the only form of this affection with which the author is acquainted; and, on the authority of a solitary observation of Professor Naegele, he denies that it can offer any impediment to labour, or call for manual interference. In this opinion, however, we can by no means concur, having met with cases in which labour has been seriously impeded by œdema, and other affections of the os uteri, such as are described by Dr. Kennedy in his paper in the Dublin Medical Journal for November, 1838.

Inflammation, and *varix of the uterus*, which occupy Chapters ix. and x. are mentioned as possible causes of obstruction to labour, but no instances are related in which they actually impeded parturition.

B. *Tumours of the vagina* are treated of in six chapters, in which the author follows the same arrangement as in his remarks on tumours of the uterus. We pass over these, however, and come to the second part of the work, wherein are considered *tumours of parts near the generative passages.*

In Chapter i. a single case is related of bony *tumour of the right fallopian tube*, in which the woman died undelivered.

Chapter ii. *On tumours of the ovaries.* The prognosis in these cases is unfavorable, for although the enlarged ovary seldom absolutely prevents delivery, yet a large proportion of women die in childbed. In one instance related by Dr. Puchelt the patient died undelivered; fourteen women died soon after delivery, three at the end of a longer period, and only thirteen permanently recovered.

A difference of opinion exists with regard to the proper management of labour under these circumstances: some, as Davis and Merriman advise trusting much to nature, but Burns warns of the danger of delaying interference. The nature of the assistance to be given is another point upon which authors are by no means agreed. The reposition of the tumour above the brim of the pelvis has been urged by Burns, Baudelocque, and Moreau; Madame Lachapelle agrees in regarding it as desirable, but thinks that the accomplishment of the object would be attended with great difficulty. It has, indeed, been objected that the reposition of the tumefied ovary is likely to be followed by inflammation, suppuration, and death, but in no instance hitherto recorded can the fatal event be clearly referred to any mechanical injury. The evacuation of the contents of the ovary, either by puncture with a trochar or by means of incision, is the second plan which has been proposed. The opening into the tumour may be made either from the vagina or from the rectum: the latter is preferred by Dr. Merriman, who observes that the tumour is usually inclined towards the curvature of the sacrum, and that an incision of any extent into the vagina would be liable to be enlarged by the head of the fœtus in its descent. Lastly, the extirpation of the ovary has been proposed by Merriman, but hitherto has not been practised.

Chapter iii. is on *distention of the rectum*, by the accumulation of fæces.

Chapter iv. treats of the difficulties produced by *over-distention of the bladder*, by the presence of *calculus*, or by a *scirrhous* condition of the bladder.

The next chapter is on *tumours situated in the cellular tissue of the pelvis*. These tumours may be attached to any or all of the bones of the pelvis, and present the same differences in their character as tumours of the uterus. In arranging the numerous observations of these tumours, the author has classified them according to their density, and relates cases of steatomata, scirrhous growths, encysted tumours, and hydatids.

From a comparison of the seventeen observations here recorded, it appears that three women and nine infants died during labour; five women died soon after delivery, seven women and three infants survived, and the fate of one woman and six infants is not mentioned.

The management of these cases must of course vary much according to circumstances. The author gives the following synopsis of the treatment resorted to in the instances he has quoted:

"I. Delivery was accomplished by nature alone in two instances; one of which occurred to Bertrandi, the other to Denman. In both the difficulty was produced by an encysted tumour.

"II Pelletan pressed the tumour into the right side of the pelvis. Both mother and child did well.

"III. Lachapelle emptied the tumour of its contents and turned the child. Mother and child were lost.

"IV. Burns and Drew extirpated the tumour. Both women and one of the children were saved

"V. In a case related by Meier, the forceps were applied without success, and the woman died undelivered. Siebold applied the forceps in another instance, while at the same time the tumour was pressed up above the symphysis pubis. The child was born dead, but the mother recovered.

"VI. The lever was employed in one instance; but Meissner does not state with what result.

"VII. In one case Osiander turned the child which was still-born, but the mother did well. Merriman resorted to the same practice, but lost both mother and child.

"VIII. A case occurred to Gensoul, in which the feet presented. The mother died in consequence of rupture of the tumour and extravasation of its contents into the abdominal cavity. The fate of the child is not mentioned.

"IX. In one case which terminated fatally, Denman perforated.

"X. Ramsbotham and Coutouly extracted the child by the crotchet, and in both instances saved the mothers.

"XI. Coutouly, Meyer, and Gensoul, performed the Cæsarian section. In each instance the woman died. One of the infants was saved, and it is not stated what became of the others." (pp. 223-4)

The sixth chapter treats of the *different forms of hernia*, which present a mechanical obstacle to labour, and with that the work concludes; nor does the last chapter show any sign of flagging in that diligence with which the author set out. We most cordially recommend the book to all who are interested in the obstetric art: to every lecturer it will be indispensable.

PART SECOND.

Bibliographical Notices.

ART. I.—*A Discourse on the Phenomena of Sensation, as connected with the Mental, Physical, and Instinctive Faculties of Man.* By JAMES JOHNSTONE, M.D., Physician to the General Hospital, Birmingham, &c. —*London,* 1841. 8vo, pp. 264.

THIS treatise, as we are informed by its author, contains the substance of a course of Lectures delivered by him in the spring of 1838; but what was his motive for presenting it to the public, he has not stated; and we find ourselves rather at a loss to account for its appearance. Medical books may be distributed under two classes; those which are the result of an honest conviction on the part of their writers, that they are in the possession of knowledge, (whether this be acquired by original research, or by collecting and comparing the statements of others) which may be advantageously diffused amongst their brethren; and those which are the fruit of a desire to make themselves known as authors, on account of certain advantages which they expect to derive, either in the shape of literary or scientific fame, or of profitable reputation with the public. Dr. Johnstone's character and position in the medical world forbid us to impute to him any selfish motive; and yet we are surprised that he is so far ignorant of medical literature as to suppose that his treatise contains anything either new in itself, or new in its manner of production. It is in fact a mere compilation, which might very well serve as part of a course of lectures, but of which a very small amount of information on the part of its readers would enable them to perceive the sources. As a purely elementary treatise, it may be put into the hands of the student who is commencing the pursuit of physiology; but we must qualify this recommendation by observing that we do not consider that any single system can be advantageously treated of in such an isolated manner, unless an amount of detail respecting its relations to other systems be given, which shall render it complete in itself. This is very far from being the case in Dr. Johnstone's discourse, which is, moreover, not only imperfect in this respect, but much behind the present state of knowledge in almost every department of the subject. Such hypotheses as the following are worse than useless in an elementary treatise: "I do not mean to assert that nervous influence and electricity are absolutely identical; but it is surely far from impossible that the former may be a subtile modification of the latter." Further on, we find that "there are two kinds of sensation, the one of which is unaccompanied with consciousness, and which, for the sake of distinction, we shall call *negative* sensation; while the other, which is attended with consciousness, we shall name *positive* sensation." We must here again protest against the confusion of ideas that the employment of the term *sensation* to designate any change in which consciousness is not involved is certain to produce. In the chapter on Involuntary Motions, we find the *dependence* of the heart's action and of the peristaltic movements of the alimentary canal upon the ganglionic system,

strongly advocated—a doctrine which we regard as overthrown by the latest and best experimental enquiries on the properties of muscular fibre. And in the next chapter, we find the actions excited through the spinal system of nerves designated as *semi-voluntary*, although a very little consideration will show that the will does not in the least degree participate in their production, although they are in various degrees controlled and directed by it. If Dr. Johnstone will hold his breath for a few moments he will find that no effort of his will can prevent respiratory actions; nor will he be able by a voluntary effort to avoid performing the movements of deglutition when the requisite stimulus is applied. So many similar instances of loose and inaccurate generalization occur in the treatise, that we are obliged to regret (which we do most sincerely) our inability to pronounce a more favorable opinion on it.

Art. II.—*Treatise on the Sympathetic Relation between the Stomach and the Brain, and, throughout, between the Digestive and the Nervous Systems, in the Causation and Cure of Diseases. With an Appendix containing a few Observations on certain points connected with the Treatment of Chronic Disease, and its attendant Debility.* By Charles Wightman, m.d., Licentiate of the Royal College of Physicians of London, and Resident Physician in Newcastle-upon-Tyne. —*London*, 1840. 12mo, pp. 192.

This is truly a big title for a book so little! We are told in the Preface that not only no specific treatise upon the subject, has hitherto existed in the English language, but that the only bibliographical notices which Dr. Wightman can discover in reference to it, are of two unpurchaseable Dissertations published towards the end of the last century, by Drs. Rahn and Veegens. "If then," continues the Resident Physician, "there should be any similarity between them and my own treatise, it must be regarded as a circumstance purely accidental." . . . "But I wish it to be distinctly known, that to no author who has written *expressly* upon the subject, have I been indebted for a single remark connected therewith." Nevertheless we will venture to affirm, that there is nothing of any value in Dr. Wightman's book with which the well-educated medical practitioner is not perfectly familiar; and we can scarcely help placing it, therefore, in the second of the classes we mentioned in the last notice; to which, indeed, the superfluous minuteness of the title-page would have predisposed us, before reading it, to refer it.

There is plenty of room for a really philosophical treatise which shall afford a satisfactory elucidation of those remarkable sympathetic relations between the nervous centres and the digestive system, which the phenomena of disease so abundantly reveal; but such an explanation must be founded upon the soundest physiological inductions, and not (as most part of Dr. Wightman's unfortunately is,) upon doctrines that have not stood the test of experience. As Dr. Wightman expresses himself as anxious for any additional observations of interest, we may mention a case which came under our own notice, in which a gentleman, predisposed to derangement of the nervous system by his excessive mental activity, was one day attacked, after eating a rather full luncheon of somewhat indigestible food, with a sudden and general but incomplete loss of muscular power, like

that which takes place in partial syncope, but without any corresponding depression of the mental energies. What muscular force remained was principally exercised in a tendency to turn round to the right side, like that stated by Magendie to follow division of the crus cerebelli, and other injuries of the base of the brain. The stomach was speedily relieved by spontaneous vomiting; and the patient completely recovered in a few hours.

Although we have spoken with disapprobation of the manner in which Dr. Wightman's treatise is put forth, we must do the author the justice to say that we think it may be useful to students and young practitioners, in drawing their attention to the intricacies of diagnosis in the class of cases to which it refers.

ART. III.—*Elements of Physiology, for the use of Students, and with especial reference to the wants of Practitioners.* By RUDOLPH WAGNER, M.D., Professor in the University of Göttingen, &c. Translated from the German, with additions, by ROBERT WILLIS, M.D., Physician to the Royal Infirmary for Children, &c. Part I. On GENERATION AND DEVELOPMENT. *With numerous Woodcuts.—London,* 1841. 8vo, pp. 229.

A TREATISE on physiology, for the use of students, and with especial reference to the wants of practitioners, by the distinguished German physiologist, Rudolph Wagner, rendered accessible to the English reader, we consider indeed a boon conferred upon the medical as well as on the general literature of Great Britain. But we have here more than a translation. Distinguished as this is for the spirit and pureness of language befitting an original English composition, we are indebted to Dr. Willis for additions which have filled up several lacunæ in the original, and which exhibit the practical physician very favorably in the character of an intelligent and candid physiological commentator.

The present part treats of the subject of generation and development, and is copiously illustrated by exquisite wood-engravings taken from the ICONES PHYSIOLOGICÆ, which Professor Wagner has published as a prelude to his treatise. The Icones Physiologicæ being now completed, the other parts of the treatise may be shortly expected, when, as we are promised, they will appear in their English dress with all convenient speed. It may be remarked that the part of the work before us is, as will be also the other parts, complete in itself.

We do not mean at present to enter into any examination of the work, but we hope to do so on a future occasion; in the meantime, we extract the headings of the chapters, in order to give an idea of the way in which the subject is treated.

GENERATION.—I. Analysis of the germ-preparing organs and their products. II. On the form of the organs of generation. III. Phenomena which accompany the generative act.

DEVELOPMENT.—I. The history of the incubated egg. II. Development of the human embryo, with supplements from the history of that of the mammiferous animal. III. History of the development of the various tissues.—Histological development.

We need scarcely say, we strongly recommend to the profession this First Part of Wagner's Elements of Physiology by Willis.

ART. IV.—*A Practical Treatise on the Venereal Disease; founded on Six Lectures on that subject, delivered in the Session of* 1838-9, *at the Aldersgate-street School of Medicine. With Plates.* By F. C. SKEY, F.R.S.—*London*, 1841. 8vo, pp. 195.

WE are in some measure at a loss to explain Mr. Skey's motive in giving to the medical profession at the present day the volume under notice. It certainly is not, as its title would lead us to expect, a *treatise* on the venereal disease, but an imperfect monogragh on some of its primary and secondary forms. The principal object of Mr. Skey's work is to advocate the simple or non-mercurial treatment of primary venereal sores, with the exception of the indurated primary chancre. Mr. Skey comes forward at the eleventh hour as the advocate of a system which has been gaining ground in Europe for the last five and twenty years; upon which hundreds of volumes have been written; and with which all medical men are or ought to be familiar. In limiting the treatment by mercury to the indurated primary sore, Mr. Skey merely recapitulates the rules laid down by Mr. Carmichael, and the foreign writers generally.

The first chapter of the work contains a short historical account of the origin and progress of venereal diseases; and here Mr. Skey speaks of the spontaneous or self-generated origin of syphilis, in which "the elements of the poison lie dormant, and may be developed by the action of a simple irritant." We lament that Mr. Skey, when speaking of the mode of propagation of venereal disease, has omitted all mention of the data which are furnished by inoculation, and the use of the speculum. The latter instrument has shown that primary venereal sores and gonorrhœal discharges are as frequently seated deep in the vagina, on the neck of the uterus, and even in the os uteri, as they are upon the external parts; and hence any opinions on the propagation of primary sores or gonorrhœa, furnished on the examination of the external parts only, must be received with much reservation. Mr. Skey says, "Do not venereal sores almost invariably occupy the external organs?" Most certainly they do not. By neglecting the information furnished from these two sources Mr. Skey would again plunge his readers into that confusion from which the history of the propagation of syphilis had been in some degree rescued by the means in question.

We find nothing to detain us in the second, third, fourth, and fifth chapters. They however contain some valuable cases illustrating the positions already laid down, and which we have said are those of generally well-informed medical men of the present day, at least where Mr. Hunter's dogmas have not been too blindly worshipped. The last chapter is devoted to gonorrhœa, its varieties and treatment. Here the doctrine of spontaneous origin is again broached. On many of the points contained in this chapter we are directly opposed to Mr. Skey's views. We certainly believe that the urethral discharges produced by sexual intercourse are of various kinds, but we also believe the true gonorrhœa virulenta to be a specific disease, capable of propagation under the same forms, and very easily to be distinguished from those discharges we may term simple, which are due to cohabitation with females suffer-

ing from leucorrhœa, or various inflamed and irritable conditions of the vagina. We are sorry our limits will not permit us to examine Mr. Skey's views in this matter more closely. Besides the cases already referred to, there are many other very instructive ones scattered through the work; we would, however, remark that many of these cases cannot be implicitly relied on, being founded only on the examination of the external parts.

ART. V.—*An Enquiry into the Efficacy of Digitalis in the treatment of Idiopathic Epilepsy.* By EDMOND SHARKEY, A.B. M.D. One of the Lecturers on Midwifery in the Hunterian School of Medicine, Charlotte-street Bloomsbury.—*London*, 1841. 8vo, pp. 80.

THE contents of this volume might perhaps have formed as appropriate a subject for communication to one of the journals as for a separate treatise. In order to make a book, the author has been obliged to introduce a general description of Epilepsy, something after the manner common in inaugural dissertations. This we think superfluous. We must add that many of his remarks on the pathological relations of epilepsy, and the operation of remedies, are so vague and inconsecutive, that it is extremely difficult to perceive their drift. He endeavours to establish some analogies, especially one between sleep and epilepsy, in which we cannot say we think he is very successful, and with which we deem it unnecessary to detain the reader. Leaving, therefore, the theoretical part of Dr. Sharpey's treatise, as not demanding any particular notice, we may state the following as the chief practical points on which he insists with reference to the exhibition of digitalis in epilepsy.

1. That it is only to the idiopathic and uncomplicated forms of the disease, that the remedy is generally applicable.

2. That in the treatment of such forms of epilepsy, digitalis has had as much success, in proportion to the number of trials made, as nitrate of silver, or oil of turpentine, and has succeeded in cases in which these have failed.

3. That the best mode of exhibiting it is according to the following formula, used by the author's father.

℞. Foliorum Digital. purp. recentium. ℥iijss.

Contunde in mortario in pulpam; dein adde cerevisiæ fortioris, lb.j. Infunde per horas septem; dein cola exprimendo. Capiat liquoris colati ℥iv, cum pulv. fol. Polypodii quercus siccatorum, aut radicis siccatæ gr. x.

4. That its efficacy probably depends on a specific power, and not merely on its effect upon the circulation.

5. That what is called the *cumulative* effect of digitalis amounts simply to this: that a certain quantity must be taken before any effect is produced; that this is no other than the aggregate effect of the separate doses that have been taken; and hence, that there is much less danger than might be supposed in administering a very large dose of digitalis at once.

6. That the system may assume a state of *tolerance* of digitalis, as of tartar emetic, and other medicines.

7. That the treatment of epilepsy with digitalis should be commenced immediately after a fit, not immediately before one.

According to Dr. Sharkey, the earliest notice of the efficacy of digitalis in epilepsy is to be found in Parkinson's Theatrum Botanicum, London, 1640. The next writer who mentions this use of it, is Withering. After him, Dr. Currie employed the medicine in three cases; and in the year 1807, it was tried by our author's father in conjunction with Dr. Mills of Dublin. Dr. Sharkey, senior, published his cases in the Lancet of May 27, 1831; and his son, in the treatise before us, has given an abstract of them, with additional cases derived from other sources. The cases are not very clearly detailed, and we are left much in the dark as to the intervals at which the remedy was administered. We dismiss the subject with a recommendation to our brethren to make a fair trial of the powers of digitalis in epilepsy.

ART. VI.—*Elements of Electro-Metallurgy, or the Art of working in Metals by the Galvanic Fluid, &c.* By ALFRED SMEE.—*London*, 1841. 8vo, pp. 163.

ALTHOUGH this work scarcely falls within the class of those to which the pages of our Review are properly dedicated, yet the author being a surgeon, and his volume being likely to be more read and better appreciated by the members of the medical profession, than by any other class of persons, we are induced to notice it. There is something highly creditable in the fact, that many of the most ingenious and interesting discoveries in the arts and sciences have emanated from men, either actually engaged in medical practice, or who have devoted themselves to the study of one or other of the special branches of medicine, more especially of chemistry. Whether this has been a profitable course to themselves or not is another qnestion; but at all events, society has derived considerable benefit from their labours.

Our readers, and more especially those who have made chemistry a study, must have heard of the Electrotype, or the art by which impressions in copper may be taken by galvanism from medallions, casts, plates, or models; and many must have had an opportunity of witnessing the admirable perfection with which the most exquisite works of art, may be copied by these silent operations of nature. It is not our purpose to settle who is the original discoverer of this singular process; it is sufficient for us to state that Mr. Smee has, in our opinion, carried it far beyond any other individual; and that he has made it applicable to cases, which judging from published documents, we should hardly imagine the original discoverer could have foreseen.

The treatise before us is short, but it is full of practical information. The author states his experiments clearly, so that they may be easily repeated by others. There is enough matter to have made a volume three times the size, but he has wisely contented himself with condensing his language within the limits necessary to render his explanations intelligible. The subject discloses an entirely new department in chemistry; and we can sincerely recommend the book to those of our readers who take an interest in that science. In the next edition we would recommend Mr. Smee to pay more care to the general qualities of his style than he seems to have done in the present.

Art. VII.—*The Domestic Management of the Sick Room, necessary in aid of Medical Treatment for the Cure of Diseases.* By Anthony Todd Thomson, m.d. f.l.s. &c.—*London*, 1841. 8vo, pp. 586.

We have been both pleased and disappointed with this book. We agree with the author in thinking that something of the kind was much wanted, and were therefore glad when we observed the announcement of a volume on the subject, by himself. On perusing it, however, we cannot but regret to perceive so many traces of haste, and so many departures from the proper scope of such a work. This regret is increased by the very ability with which Dr. Thomson has executed his task when treating of matters really relevant to his professed object: but when he diverges, as he often does, to matters purely professional, and fit for the consideration of medical men only, he not only encumbers his work at the risk of a positive diminution of its interest and even safety to his "domestic" readers, but from adopting a popular dress for really scientific ideas, he often falls into inaccuracies of style and expression which ought never to be indulged in. In one place, for example, he speaks of "irregular chills and heats, displaying themselves and *subsiding into a hot skin*, quick pulse, hurried or otherwise embarassed breathing." (p. 144.) Expressions of this kind are far from being unimportant; because their use raises a presumption against accuracy of thinking in any one employing them; and of all subjects on earth, medicine is that in which the greatest precision and accuracy are required. In another place, again, he says that calomel, when laid dry upon the tongue, "*hangs about the mouth and fauces*, and is more *readily taken into the habit*." This is a very incorrect colloquialism: calomel may *adhere* to the mouth, but it can "hang about" nothing, and when absorbed it is taken into the system, but not into the "habit." But we have no wish to be severe upon Dr. Thomson, and shall therefore content ourselves with having drawn his attention to these occasional blemishes that he may correct them in subsequent editions.

As already mentioned, we have been much pleased with the simple and intelligible style in which Dr. Thomson conveys his most useful information on subjects within the legitimate scope of his work; and if he were to *excise* with a steady and unsparing hand everything by which none but professional persons can be benefited, and were then carefully to digest what is left, with a view to condensation, we feel assured that he might reduce his volume by one half, and give it a triple value to the class of readers for whom it is intended. A true "hand-book" to the sick room must be clear, concise, and to the point. If the subject is overloaded with extraneous matter the mind flags in reading it, and the essential is apt to be overlooked. If the author would keep in mind that the mother, sister, friend, or nurse is supposed to act in the sick room under the direction of the physician, and not from her own independent judgment, he would discover many pages which might be omitted with the greatest advantage. Of the 102 pages of the introduction, a very large portion is inappropriate and irrelevant; and in this respect it contrasts with the first chapter, most of which comes home to the reader as the one thing wanted. The information contained in the introduction is we ad-

mit, interesting and useful: all that we object to is, that it is out of place, and for the specific purpose of the author, is rather an encumbrance than an advantage.

In doctrine, also, we have observed one or two rather startling announcements in Dr. Thomson's book. "*Winter*," he says, "*is a more healthy season than summer*, provided we are well clothed and that we can afford fires and enjoy sufficient vigour to take exercise." (p. 6.) Is it really so? and has mankind been under a delusion for so many thousand years in looking forward to fine summer weather for the enjoyment of life and the restoration of the invalid? If our author is correct, how strongly must we have miscalculated the supposed healthful influences of the mild temperature, pure open air, sunshine, and delights of summer! But unluckily for Dr. Thomson "facts are chields that winna ding," and accordingly, on turning to the Registrar-General's last report for specific data to see whether he or we were in the wrong, we find the deaths recorded in the metropolis to stand as follows:

	In Jan., Feb., and March,	In July, Aug., and Sept.
From epidemic, endemic, and contagious diseases	3792	3141
From diseases of the respiratory organs	4715	2705
,, of the nervous system . .	2255	1924
,, of organs of circulation .	273	177
,, of digestive organs . .	647	836
	11,682	8,783

Or, with the single exception of deaths from diseases of the organs of digestion which in summer exceed those in winter by 189, all the other classes of diseases are more fatal in winter than in summer in the proportion of 11,035 to 7947, or in other words, of 11 to 8; and yet, according to our author, winter is the healthiest of the two seasons, when fire, clothing, &c. are enjoyed! If he is correct, what a fearful proportion of the race must be without these advantages, even in the once "merry England."

Art. VIII.—*Essay on Tobacco.* By Henry Wilson Cleland, m.d., Lecturer on Medical Jurisprudence, &c.—*Glasgow*, 1840. 4to, pp. 68.

There is a class of persons, generally composed of old women of either sex, who have a mania for collecting: some delight in autographs; some in original poetry; some ride their hobby upon coins or crooked sixpences; and the author of "Ten Thousand a Year" tells us, that Mr. Quirk luxuriated in the gathering of locks of hair from the heads of executed or transported criminals. In like manner, Dr. Cleland appears to be "a collector and gatherer of other men's stuff;" but his monomania is tobacco; and, in his work, he seems to have strung together from the pages of his album, all that has been sung or said in praise or dispraise of it, and this, unfortunately, with little deduction or generalization of facts. The work, however, has afforded us an agreeable half-hour's recreation, and impressed us with a very favorable opinion of the author's talent for indefatigable research—a talent which we are quite sure he could profitably employ on less airy matters.

Art. IX.—*Descriptive Catalogue of the Preparations in the Museum of the Royal College of Surgeons in Ireland.* By John Houston, m.d. m.r.i.a., Curator of the Museum. Vol. II. (Pathology.)—*Dublin,* 1840. 8vo, pp. 604.

The publication of this work is extremely honorable to the Dublin College of Surgeons, and the manner of its execution reflects the highest credit on Dr. Houston. The following extracts from the preface give an account of the plan of the work, (which is excellent,) and supersede the necessity of our entering into any details respecting its contents. We regret, however, that want of room prevents us from enriching our pages with a few of the interesting and important histories which abound in it. We will only further add that the "Descriptive Catalogue" ought to occupy a place in every medical library, and deserves to be consulted by every pathologist.

"The book is divided into two essential parts—the index which is placed at the end, and the descriptive part. The index contains a brief list of all the preparations in the museum, and demonstrates as nearly as possible the order of their arrangement on the shelves; it also indicates the page in the descriptive division of the work, at which a further notice of particular preparations may be found.

"The preparations are distributed into six classes, which are severally distinguished, both in the catalogue and in the presses, by letters of the alphabet in Roman characters. The first class, distinguished by the letter A, contains all the organs concerned in the assimilation of food The second, by B, contains the organs of circulation. The third, by C, the organs of circulation. The fourth, by D, the organs of sense. The fifth, by E, the organs of locomotion and prehension. The sixth, by F, the organs of generation and secretion of urine. These classes are again subdivided into orders, the numbers of which vary with the most convenient subdivisions which each admits of, and are distinguished by letters in italics. Thus, the first class, embracing all the series of organs concerned in the assimilation of food, and distinguished by the Roman letter A, admits conveniently of subdivision into four orders. In the first, marked *a*, are contained those parts of the alimentary system which, in the form of preparations, can be most conveniently grouped and exhibited together, viz. the mouth, tongue, pharynx, and œsophagus. The second order, *b*, contains preparations of the stomach. The third, *c*, contains those of the intestines. The fourth, *d*, the glands connected with the intestines. The same mode of subdivision into orders, and of marking by appropriate letters in italics, is observed throughout the other classes.

"Every exertion has been made to secure an accurate report both of the history and recent pathological appearances of every preparation. The account given of each is, in fact, a condensed abstract, made by the author from the most authentic sources, viz.—either from the verbal or written communication of the donor, or from some printed statement authorized by him,—all such abstracts being studiously curtailed to such dimensions, that, while connecting intelligibly the post-mortem appearances with the history of the disease during life, they might not be unsuited to the pages of a work like the present."

Art. X.—*A Lecture on Loxarthus or Club-foot.* By T. D. Mütter, m.d., Lecturer on Surgery, Philadelphia.—*Philadelphia,* 1839. 8vo, pp. 104.

This lecture is very creditable to the author. It contains nothing new or original, but is a most accurate and methodic digest of the opinions of the various authors who have written on the subject of club-foot, tested by Dr. Mütter's own experience in about 30 cases. It would be a very useful book for students, wishing to make themselves acquainted with the theory and practical treatment of club-foot.

ART. XI.—*Elements of Chemistry, including the most recent discoveries and applications of the Science to Medicine and Pharmacy, and to the Arts.* By ROBERT KANE, M.D., M.R.I.A. &c. Part I.—*Dublin*, 1840. pp. 356.

AT a time when so many systematic treatises on this science are before the public, many of them fresh from the laboratories of the most distinguished cultivators of its ample domain, the appearance of a new one may seem uncalled for; yet we are glad to say, that there is a peculiar character about Dr. Kane's Elements, which disposes us to recommend it to our younger friends as perhaps the best guide we can indicate for the prosecution of their chemical studies. The objects which the author has in view may be best stated in his own words: "to present to the student an account of the general principles and facts of chemistry, and of its applications to pharmacy, to medicine and to the useful arts." Keeping this steadily in view, he intends to pass over, very briefly, when treating of the chemistry of inorganic substances, the history of those numerous bodies which, from their rarity, are objects only of scientific curiosity; dwelling rather on the properties and mode of preparation of those which are of real scientific interest, or of importance in medicine and the arts. In like manner, in the department of organic chemistry, the history will be fully discussed of all such bodies as are of importance, from their bearing upon general principles or existing theories, from their use in medicine and pharmacy, their employment in the arts or in ordinary life; whilst the numerous series of bodies which are every day discovered in organic chemistry, but which do not come under the above heads, will be dismissed with only a notice of their existence. The relations of chemical action to the functions of organized matter, the applications of chemistry to physiology and pathology, will be treated of so far as our accurate knowledge extends.

It is evident, then, that the plan of this work renders it much more suitable for the medical student than any yet within his reach; since it will communicate to him a full knowledge of general principles illustrated by facts of a character peculiarly interesting and useful to him. It only remains for us, therefore, to speak of its execution; of which, so far as the portion before us enables us to judge, it would be difficult for us to express ourselves in too high terms. This part comprehends an outline of the sciences of heat, light, and electricty, especially as related to chemical phenomena; and we have been particularly struck with the completeness of the view which is presented within a small compass, not only of the elementary principles and facts of these sciences, but also of some of their most complex phenomena, which are explained with a degree of lucid conciseness that is not often to be met with. The sections on the polarization of light and heat may be particularly instanced; the general phenomena of these two subjects, which are usually regarded as too intricate for ordinary comprehension, being so clearly explained as to be intelligible to every mind of common ability; and several interesting practical applications of principles, whose sphere seems far removed from the ordinary concerns of life, being pointed out. Throughout, we are struck with the very judicious selection which Dr. Kane has made of the scientific novelties that are constantly crowding upon the attention of the physical philosopher; and those which he has introduced are so judiciously combined with the knowledge previously accessible in elementary works, that the treatise may be regarded as truly belonging to the present era, and not formed (as too many such treatises

are) upon the model of those which the rapid advance of science has caused to be antiquated within even a few years. Did our space permit, and were it easy to make a selection, we should have been glad to present our readers with a specimen or two of Dr. Kane's style; but we must content ourselves with expressing the hope that the future parts will not fall short of the promise held out by the present one; and with earnestly repeating the recommendation of the work to our readers, as the one best adapted to give them a luminous view of the present state of the principles of the science, with those applications of them which are most important to the medical man.

Art. XII.—*Observations on the Management of Madhouses.* Part II. *Containing an Account of Susannah Roginson, &c.* By Caleb Crowther, M.D., formerly Senior Physician to the West Riding Pauper Lunatic Asylum.—*London*, 1841. 8vo, pp. 104.

The venerable author of this little work has laid open, with unsparing hand, the alleged negligences of some of the institutions in his neighbourhood; and has forcibly described the qualities desirable in the superintendent of a lunatic asylum. He alludes, in terms of high commendation, to the general management of the large asylum at Hanwell; and his testimony to the zeal, industry, and benevolence of its physician, Dr. Conolly, may be taken as a well-timed set-off to the part taken by a small minority of the magistrates of Middlesex at the late meetings at Clerkenwell. Many of Dr. Crowther's remarks deserve attentive consideration, both from magistrates and physicians. He is very severe upon those whose proceedings he views with disapprobation; but some of the facts stated by him are of a very startling kind, and remind us of similar enormities in another asylum in Yorkshire, which were long denied, long concealed, long protected by great authorities, and proved at last. It is remarkable that Dr. Crowther's charges apply chiefly to an asylum of which the directors seem to pride themselves on keeping the beaten track, and resisting all improvement. Any further notice of this work would engage us in angry discussions in which we have no desire to take a part: but if the facts related by Dr. Crowther are correct, they ought surely to be looked into.

Art. XIII.—*Historisch-pathologische Untersuchungen, als beiträge zur geschichte der Volkskrankheiten.* Von Dr. H. Haeser.—*Dresden*, 1839. pp. 329.

Historico-pathological Researches, being contributions to the History of Epidemics. By Dr. H. Haeser.—*Dresden*, 1839.

This work has enjoyed much reputation upon the continent, but it is needless now to do more than notice our having received it; for in a former Number of our Journal (vol. VII, p. 120,) we reviewed at some length Dr. Jahn's new system of pathology, upon which Dr. Haeser's work is professedly founded. "Diseases," says our author, "obey no less certain and determinate laws than those which govern every other living being. Paracelsus and others suspected this fact; but by no one has it been so fully illustrated as by Jahn." Founding his opinions upon the theories of Jahn, with which our readers are too well acquainted to warrant our again detailing them, Dr. Haeser has traced with great industry and praiseworthy research, the history of the epidemics of the past and present ages; so far the volume is one of much merit; but it contains nothing new, either in doctrine or in practice.

ART. XIV.—*Leçons de Clinique Médicale faites à l'Hôtel Dieu de Paris.* Par M. le Professeur CHOMEL, recueiliées et publiées par F. SESTIER, M.D., Professeur agrégé à la Faculté de Médecine de Paris, &c. &c. Tome troisième. (PNEUMONIE.)—*Paris*, 1840. 8vo, pp. 592.

Lectures on Clinical Medicine. By Professor CHOMEL, reported and published by F. SESTIER, M.D. &c. Third Volume. (PNEUMONIA.)

WHAT claim this volume has to be ushered into the world as a part of M. Chomel's Clinical Lectures we are completely at a loss to divine. The introduction commences by a statement that that Professor has not delivered general lectures upon pneumonia; his occasional observations, therefore, are all the compiler can have availed himself of; and there is positively not an individual of eminence in Paris whose name does not occur more frequently in its pages than that of the clinical teacher of the Hôtel Dieu. We are therefore disposed to think the permission to parade his name on the title-page rather unwarrantably granted by M. Chomel. The work is, in fact, a pure compilation by the professed editor, and a very imperfect one it is. As M. Sestier has understood the office of compilation, this consists in the simple arrangement under proper heads of all the facts and opinions produced by every author within his reach, relieved by an occasional attempt at languid criticism. How the compiler may achieve higher things than this we need not stay to make evident; but even for the humble species of literary drudgery he has chosen to figure in, M. Sestier cannot be said to possess the necessary requisites, for, like the immense majority of his countrymen, his powers as a linguist appear limited to speaking and writing his mother tongue. Notwithstanding all this, we should be sorry to be understood as contesting the utility of this production altogether: as it contains abstracts of the works of the writer's countrymen upon the special characteristics of pneumonia at every period of existence—in new-born infants, in children, in adults, and in subjects of advanced age—it may be referred to with considerable satisfaction by persons unwilling to encounter the labour of perusing the original treatises themselves. With the contents of these treatises we, at the period of their publication, made the readers of this journal very fully acquainted.

Under the head of differential diagnosis we observe the following sufficiently curious remark: "When a very large tumour of the aorta fills a great portion of the thorax, and in consequence of the attenuations of the walls of the sac, a small quantity of its contained blood oozes into the trachea, the dull sound and sanguinolent sputa might lead the observer to admit the existence of pneumonia. A case of this kind was observed in the wards of M. Louis, and another related in one of M. Rostan's works. A woman aged sixty complained of pain at the posterior part of the left side of the chest: the sound, on percussion, was dull, the respiration obstructed, sanguinolent sputa followed coughing, the pulse was frequent and hard, the skin hot, and the patient suffered under considerable thirst. On opening the body the lung was found to be healthy, the aorta the seat of an enormous aneurism." The writer adds, and, so far as we can trust to an *à priori* judgment, we coincide in the opinion, "Nevertheless, ausculation would scarcely allow of any doubt being entertained in a case of this kind; the course and duration of the malady, as well as the antecedent history, would also clear up the diagnosis."

PART THIRD.

Selections from the British and Foreign Journals.

I. THE FOREIGN JOURNALS.

ANATOMY AND PHYSIOLOGY.

Observations and Experiments on the Functions of the Pneumogastric and Spinal Accessory Nerves. By Professor ARNOLD, of Zurich.

THE following observations are abridged from an article in the Archives Générales de Médecine, founded on a work entitled "Bemerkungen über den Bau des Hirns und Rückenmarkes," published at Zurich in 1838, which we have not seen. Our space allows us to give little more than the general conclusions. Arnold's experiments seem to have been contemporary with Reid's and Valentin's, and fully confirm the views of the former so far as they extend, both in regard to the influence of the par vagum on respiration and digestion; but Arnold's analysis of the *mode* of influence is not nearly so complete as Reid's. They also fully coincide with Valentin's statements, as to the *solely sensory* character of the par vagum at the roots, and the acquisition of motor power by inosculation with the spinal accessory. To prevent confusion we adopt the language of the author, who in common with most Germans designates the pneumogastric nerves as the tenth pair, and the spinal accessory of Willis as the eleventh.

The experiments of Arnold were made with great care on fowls and pigeons, by simultaneously dividing the two pneumogastric nerves. We cannot detail these experiments, but group their results in the form of answers to the following questions:

I. *Is the pneumogastric a sensitive or a motor nerve?* Arnold shows that previous experiments cannot decide this question, because the pneumogastric nerve immediately after it passes from the cranium is united with the fibres of the internal branch of the thirteenth [eleventh?] pair. Therefore, on dividing it in the neck, a part of the spinal accessory is also cut, and the consequent phenomena result from the deranged functions of the two nerves. A division of the roots of the nerve within the cranium thus appears the only way of solving the question as to its functions.

The section of this nerve in the neck is not so painful as many physiologists have remarked. But the pain is very acute on dividing the superior laryngeal nerves. This explains the sensibility of the parts to which the pneumogastric is distributed, the mucous membrane of the glottis being far more sensitive than that of the œsophagus and stomach. The true explanation of this fact is found in the formation of the pneumogastric plexus, to which all the branches of the tenth part do not equally contribute. The superior laryngeal nerves have no extended connexion with the plexus gangliiformis, while that part which descends to the œsophagus and stomach principally contributes to form this plexus. Now as it is an *established physiological fact* [?] that the nerves which pass through plexuses conduct impressions to the brain much more slowly and less distinctly than other nerves, the varying sensibility of the stomach and glottis is explained in the distribution of the pneumogastric nerve. Arnold goes on to

argue that this nerve is not a mixed, but a purely sensitive one, and on the following grounds: 1. It takes its origin from the posterior part of the medulla oblongata, like the greater portion of the fifth pair, and it has the same connexion with this part of the brain as the posterior roots of the spinal nerves have with the spinal marrow. 2. The disposition of the roots of the pneumogastric corresponds exactly to that of the spinal nerves, and with the larger portion of the fifth pair; that is to say, the roots remain isolated until they reach the ganglion which results from their union. The roots of the motor nerves, on the contrary, always reunite in the cranium or the vertebral canal into one or many trunks. This difference is very distinctly shown by comparing the larger portion of the fifth pair with its smaller portion, with the third, fourth, and sixth pairs, the tenth, eleventh, and twelfth, etc., etc. 3. The pneumogastric is provided with a ganglion, similar to the posterior roots of the spinal nerves and part of the trigeminal nerve, and this ganglion greatly resembles in its internal and external structure those of the spinal nerves. These combined arguments convince the author that the pneumogastric is a purely sensitive nerve.

II. *What influence has a division of the pneumogastric nerve over the general state of health?* General depression of the vital powers was observed in all Arnold's experiments, which he explains on the supposition that the division induces an incomplete transformation of venous into arterial blood. The influence of the operation on nutrition could not be determined as the animals died too quickly after the division. One dog became very lean, but the author ascribed this to the frequent vomitings, for when they ceased the dog recovered his condition. The nerve does not appear to exercise further influence on isolated organs, for the eyes of the fowls and pigeons preserved their natural state till near death.

III. *Is the sensation of hunger communicated by the tenth pair?* The experiments of M. Arnold convince him that the sensations of hunger and thirst depend on the integrity of these nerves. The voracity observed after their division is not opposed to this opinion: it proves on the contrary that the animals have not a distinct sensation of the state of their stomach. They devour food not so much to satisfy hunger as for the pleasure of eating and tasting it.

IV. *What influence does the pneumogastric nerve exercise over secretion, and the quantity and quality of the gastric juice?* Many physiologists, as Brodie, Wilson Philip, Tiedemann, etc., believe that the gastric juice ceases to be secreted after the division of this nerve. Others, as Ware, Finlay, Mayer, Brachet, etc., have seen the secretion of a juice less acid continued during some time. Arnold has never observed an absence of the gastric juice, and he does not believe that this nerve exercises a direct influence over either the quality or quantity of it.

V. *Are the contractions of the œsophagus and stomach under the influence of the pneumogastric nerve?* In answer to this question it would appear that the influence the division of this nerve has over these contractions is to be attributed to the internal branch of the spinal accessory nerve being also cut off.

VI. *What is the influence of the tenth pair over chymification?* In all the cases where Arnold divided these nerves, he found that the grains in the gizzard were entirely softened, and their internal parts altogether changed; and not only those grains in contact with the walls of the stomach, but also those in the midst. The grains were enveloped in a species of chyme, which fluid was also rejected by many fowls through the beak during life. The function of chymification then is not really changed, though it is deranged because the contractions of the stomach, or of the gizzard in fowls, are diminished. This result is directly opposed to the observations of Brodie, Wilson Philip, and others, who thought that the secretion of the gastric juice ceased after the section of the nerve, although the movements of the stomach continued.

VII. *What is the influence of the tenth pair on respiration and the respiratory movements?* Arnold found that in general the number of respiratory movements diminished after the section of these nerves. In some cases the diminution augmented in a uniform manner as death approached: in others the re-

spirations diminished at once, but afterwards were feebly raised: in others again this elevation was followed by a new diminution. It does not appear that the respiratory movements are determined by the tenth pair, though this nerve exercises an indirect influence in communicating to the brain the sensations [impressions] produced in the lungs. The internal branch of the spinal accessory nerve, on the other hand, exercises a direct influence in determining the contractions of the muscles of the glottis.

VIII. *Is the tenth or the eleventh nerve the nerve of voice?* Rufus of Ephesus and Galen observed that the section of the tenth pair produced extinction of the voice, and Sœmmering called it the nerve of the voice. But Arnold is convinced that the vocal nerve is the eleventh, and not the tenth. At his instigation Bischoff made a number of researches in comparative anatomy to confirm this opinion. In one of these experiments on a goat, the accessory nerve of Willis was cut where it quits the vertebral canal, and the animal lost its voice immediately after the operation. Velpeau and Schellhammer are quoted in support of Arnold, who, from anatomical researches, experiments on animals, and pathological observations, concludes that the eleventh is the vocal nerve.

IX. *What is the influence of the pneumogastric nerve over the transformation of black into red blood?* The comb of all the fowls in which this nerve was divided sensibly lost its redness, and became blue and blackish. On post-mortem examination the lungs were observed redder than natural; the arteries, veins, and the heart were filled with a blackish blood, generally coagulated. The same results were observed in the rabbits. Probably this may be explained by the diminution in the number of the respirations. Legallois explained it on the supposition of occlusion of the glottis, but in some of the fowls and in young mammiferæ the glottis was never closed.

X. *What is the influence of the pneumogastric nerve over the movements of the heart?* The ancient physiologists attributed to this nerve a great influence over the movements of the heart. Bichat, Emmert, Legallois, and others, have demonstrated the fallacy of this opinion. Arnold observed that the movements were greatly accelerated, and very strong after the operation, and that they did not cease for some time after death, probably causing the accumulation of blood in the organ. Cooper found the pulsations of the heart feeble, but accelerated.

XI. *What is the change of temperature in animals after section of the pneumogastric nerve?* We find from the experiments of Arnold that the temperature diminished from one to two degrees (Reaumur) immediately, or at the latest an hour, after the section; that this diminution in the proper temperature of fowls and pigeons is from two to four degrees in the first twenty-four or forty-eight hours: the temperature then became so elevated as to reach, or sometimes to surpass the normal temperature by half a degree; in some cases the animal became cool shortly before death, while in others there was no elevation, the decrease continuing regularly from the moment of operation to death. A singular coincidence is thus remarked between the elevation and depression of the temperature, and the increase and diminution in the respirations. Thus it is clearly demonstrated by experiment that the pneumogastric nerve does not exercise a direct influence on the production of heat, but an indirect influence owing to the manner in which it affects the frequency of respiration.

XII. *When and in what manner does death occur after the function of the pneumogastric nerve is destroyed?* We have seen that the transformation of black into red blood is very imperfect: that the lungs, the heart, the great vessels, both veins and arteries, are filled with blood often coagulated, and therefore conclude that the cause of death is to be found on the one hand in the stagnation of blood in the lungs and heart disturbing their functions, and on the other to the arrested transformation of the blood. The animals then die by suffocation, and this suffocation is slow, if no obstacle prevents the air entering the lungs, but it is rapid if the glottis is closed, or if liquids or blood are effused into the trachea.

Archives Générales de Médecine. Août, 1840.

On the Origin and Mode of Development of Zoospermata. By M. LALLEMAND.

THE zoospermata secreted by the testicles are susceptible, as all the products of secretion, of modification by all severe diseases, and by any cause greatly or protractedly disturbing the economy. Thus they diminish in number, in volume, in density, in vitality, according to the severity of the affection, and resist more or less putrid decomposition. They may become very scarce, and even be replaced by pyriform, ovoid, or spherical bodies. They are very mobile in their living state, and remarkable after death by their brilliant aspect, and the regularity of their forms. At the age of puberty these globules precede the appearance of the complete zoospermata, and replace them in old age and in certain pathological states, by which it appears that they are incomplete unformed secretions. The same phenomena are observed in healthy animals at each periodical return of heat. The zoospermata are most numerous in the secretory canals of the testicles, where they are almost dry, and heaped one upon another, most frequently in groups, of which all the heads are directed towards the epididymis, and the tails towards the surface of the testicle. In the vasa deferentia they separate and become more mobile and perfect. The fluids furnished by this canal, by the vesiculæ seminales, the prostate, and Cowper's glands have no other function than to favour their dilution and movements. The most perfect are always found the nearest to the excretory orifice. The most mobile, those which live the longest, are those which are voided during copulation. The ovules, secreted by the ovaries, are also according to M. Lallemand, living bodies before fecundation; they attain perfection in the oviducts after their separation from the ovary, and comport themselves exactly as the zoospermata.

[From the last proposition the editor of l'Expérience dissents, stating that the zoospermata are complete beings endowed with a peculiar independent life: but the ovules, on the contrary, are fragments of the ovary, which only participate in the general life of the individual, as all other parts of the organism, and do not acquire the independent life of spermatic animalcules until after fecundation.]

L'Expérience. 12 *Novembre,* 1840.

On the Use of Chromic Acid in Microscopic Investigations. By DR. ADOLPH HANNOVER.

AFTER trying a variety of means the author found none at all to be compared with chromic acid for preserving not only the external form, but also the minute structure of the organized tissues, and for giving them a degree of hardness, such that the finest sections could be cut from them without disarranging their elementary parts. Even the different shades of colour (of the brain and spinal cord for example) were retained for many months; and the yellow colour which was imparted to transparent and very delicate objects was found to be even advantageous.

Dr. Hannover has prepared in this fluid cellular tissue of all kinds, elastic tissue, nuclei, the different kinds of epithelium, blood-globules, muscular fibres, cartilage and bone with their primary cells, nervous fibres from the nerves themselves as well as from the brain and spinal cord, ganglion-globules, eyes, and various other organs and tissues; and he has thus preserved them to all appearance unchanged, (with the exception of the yellow colour that some acquire,) and, by their hardening, admirably adapted for microscopic dissection and display.

To show how little influence the chromic acid has upon such vital actions, as may be observed after apparent death, he mentions that he has seen the ciliary movements continue for hours on the lining of the cerebral ventricles immersed in it, and has watched the repeated contractions of muscular fibres. As a proof also of the accuracy with which observations may be made on objects thus pre-

pared, he describes many appearances confirmatory of the examinations which others have made of the fresh substances.

The mode of using this substance is, in ordinary cases, to mix one part of acid with 16 or 20 of water; but either stronger or weaker solutions may be safely used. If the substance is to be hardened, it must lie either for a short time in a strong solution, or for a longer time in a weak one; but the latter plan is preferable, because by the former the outer layers are hardened so soon that the access of the acid to the interior is hindered. For the same reason, to harden brain, it is necessary to divide it into small pieces. Blood-globules generally require a weak solution; muscle, bone, and cartilage, and most nerves will do in almost any.

Müller's Archiv. 1840, *p.* 549.

On Ciliary Motion. By S. PAPPENHEIM.

MAYER of Bonn has, as is well known, given a description of the ciliary motion in the pericardium of the frog, and it has been confirmed by Valentin. I have completely and easily succeeded in observing the fact in tritons recently killed, and in seeing both the ciliæ and the mode in which they are fixed. I have also found them on the mucous membrane on the inner surface of the membrana tympani of frogs that had been dead 24 hours. In the pericardium of the triton, which is composed of parallel tendinous fibres, the ciliæ are placed upon the epithelium, which is transparent, apparently structureless, and containing a great number of very delicately-walled spherical cells. Each of these is surmounted by a diadem of ciliæ. Similar cells are found in the ciliary epithelium of the frog's tympanum, and in that of the pericardium also.

Müller's Archiv. 1840, *p.* 533.

On the Action of the Oblique Muscles of the Eye. By A. W. VOLKMANN.

[WE have separated this portion from the paper on the motor influences of the cerebral and cervical nerves, because it is not immediately connected with the subject there specially treated of, and that it may the more certainly attract the attention of the many who, since the operation for strabismus has been practised, have been engaged in experiments on the muscles of the eyeball. As far as we have seen, none of these observers have detected the mode of action ascribed to the oblique muscles by Professor Hueck and the author of this paper, and now, we believe, generally admitted in Germany. At the same time none of the experiments lately performed are in any important degree opposed to the conclusions arrived at by these authors, and marked to all appearance with every necessary evidence of truth.]

In an admirable but little known treatise by Hueck of Dorpat, (on the rotation of the eye on its axis, Dorpat 1838,) it is shown that the office of the oblique muscles is to turn the eye upon its axis, so as to render it possible for the rays of light from any object to fall on identical spots on the retina, even when the head is inclined downwards; a condition on which, as is well known, singleness of vision depends. The observations (Nos. 2, 3, 4, 5,) related in the preceding paper fully confirm this view. In the dog, calf, and other animals experimented on, the axis round which the eye rotates is the visual axis itself, or at least its position is so little different from that of the latter, that when the oblique muscles act alone no motion of the pupil is discernible. In man a rotatory motion must also be produced by the action of the oblique; for since the muscles wind on the eyeball in the form of a semi-circle, rotation is unavoidable. But in man the axis of this rotation is not coincident with the axis of vision. While in the calf the direction in which the oblique muscles act intersects the visual axis at a right angle,—in man the intersection is at an acute angle. The inferior oblique arising from the lacrymal bone goes outwards and backwards, comes in contact with the eyeball at a point directly beneath the

axis of rotation, and is then prolonged backwards and outwards, and after describing a semi-circle, is attached to the outer and posterior part of the eyeball. The rotation which it produces will therefore have an axis, which may be considered as passing from the outermost part of the iris to the point of insertion of the optic nerve; in short, an axis which, instead of coinciding with the visual axis, cuts it, and passes across it from the front and externally backwards and inwards. The necessary consequence is that when the inferior oblique acts *alone*, the pupil describes a portion of an arc of a circle; and, supposing that the view just advanced is correct, it must turn round the outermost point of the iris (which remains unmoved,) and be thus carried in a curved line upwards and outwards.

Albinus held this opinion respecting the action of the inferior oblique, but Bell and, with him, many modern authors have thought differently. The inferior oblique they think must move the eyeball so as to carry the pupil upwards and inwards, in the direction in which it is carried in wincing, and in which it is fixed during sleep. These motions, when automatic, are ascribed by Bell to the inferior oblique, and not to the combined actions of the superior and internal recti, which preside over voluntary motions exclusively. But the proofs for this opinion are insufficient. Bell divided the superior rectus in a rabbit, and found that the pupil was still moved upwards when the eye was irritated; but this might have been effected by the upper fibres of the retractor bulbi, (as in cap. 6.) He divided also the oblique muscles in a monkey, and it remained capable of all the voluntary motions of the eye, but the pupil on that side on which the inferior oblique was divided was *scarcely distinctly* moved upwards in wincing. But a slight motion must require an organ for its performance not less than a great one does; and Bell has therefore unintentionally proved that the motion of the pupil in wincing may depend on the recti. But, in fact, this motion is never produced by the inferior oblique; for, 1st, that muscle could only move the pupil upwards and *outwards;* 2d, its action must be connected with the axis-rotation of the eye of which no trace can be discerned in the act of wincing; 3d, the experiments already detailed prove that contractions of the inferior oblique are not connected with the twitching upwards of the pupil; 4th, that same twitching upwards continues after division of the inferior oblique. In short the position of the inferior oblique sufficiently indicates its action; it carries the pupil in the arc of a circle outwards and upwards.

In cases in which this rotation of the eye on its axis really takes place, it may be discerned in men as well as in animals, by a mode that Hueck pointed out. By fixing on a small vessel in the white of the eye, which (if possible) is directed transversely and horizontally across it, and then moving the head towards the shoulder, the vessel will be found not to alter its position; and it is therefore evident that the eye turns round in the direction opposite to that in which the head is inclined downwards.

The oblique muscles of the human eye, if they acted alone, would alter the position of the pupil and consequently the visual axis; but they do not act alone. The straight muscles determine the direction of the visual axis, and this being once fixed, the action of the oblique muscles can be no other than that of turning the eyeball round its unchanging axis.

Müller's Archiv, 1840. *Heft* iv. *p.* 480.

On the Vis Nervosa of Haller. By Dr. MARSHALL HALL.

THE object of the present letter (the first of a series which, we presume, the author intends to publish in Müller's Archiv,) is to prove that what Haller called *vis nervosa*, Müller *vis motoria*, and Flourens *excitabilité*, is a power resident, not only in the nerves called motor, but in a whole system including centripetal and centrifugal nerves and the true spinal marrow; and that it is capable of acting along the spinal cord not only downwards but upwards. This

doctrine is supported by precisely the same arguments as those which Dr. M. H. has elsewhere adduced in its behalf; and none of these meet the objection we have more than once brought against it, that there is *no proof whatever* that *a motor influence* ever travels along the *incident* nerves. All we know of the matter is, that the incident or afferent nerves, when excited at their peripheral extremity, produce a change in the central ganglion (the true spinal cord of vertebrata), by which a motor impulse is propagated along the efferent nerves; and, for anything that we know, or that can be proved (in our present state of ignorance as to the real mode by which these impressions are transmitted) to the contrary, the nature of the changes taking place in the incident and motor nerves may be as different as the direction of their propagation.

Müller's Archiv. 1840, *p.* 45.

PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Observations and Experiments on the Employment of Platina in Medicine. By M. FERD. HOEFER, M.D.

THIS is a lengthy but valuable contribution to medical therapeutics. The author first treats of platina in the metallic state, and of its principal compounds, and then examines its physiological action by experiments on animals, and on man in a state of health, and when suffering from disease. Several cases are given to illustrate the statements of the author; but our space prevents us giving more than his general conclusions, which are as follows:

1. The preparations of platina (chlorides) are poisons; the perchlorides in the dose of a scruple; the double chloride of platina and sodium in the dose of two scruples.

2. The chlorides of platina, (perchloride and double chloride of platina and sodium,) are less dangerous than the salts of gold, and the corrosive sublimate.

3. The perchloride of platinum in concentrated solution, produces acute itching on the skin, followed by a cutaneous eruption in the situation where the solution has been applied. Taken internally, it at first irritates the mucous membrane of the stomach, occasions cephalalgia, reacts on the nervous centres, and thus exercises a particular alterative action on the fluids of the economy.

4. The double chloride of platina and sodium does not produce local irritation on the skin. Taken internally, it does not react on the nervous centres in a manner so sensible as the simple perchloride. It more particularly augments the urinary secretion.

5. The perchloride of platinum is a remedy very efficacious in the treatment of syphilitic diseases; and particularly of those which are old, inveterate (constitutional).

6. The double chloride of platina and sodium is well suited to the treatment of recent or primary syphilitic diseases. It is also very efficacious in the treatment of rheumatic affections.

7. Platina appears to rank in the class of medicines called alteratives, with gold, iodine, and arsenic. It differs from mercury in acting after a previous excitation, and as its administration does not entail any of the evils with which mercury is charged. The salts of gold, which appear to be poisonous in much smaller doses than the salts of platina, are, according to writers, only efficacious in certain cases of constitutional syphilis.

8. Platina, as an alterative medicine, is preferable to mercury and to gold.

Gazette Médicale de Paris. 28 *Nov.* 1840.

Complete Paralysis of the fifth pair on one side, with complete Abolition of Sight, Hearing, Smelling, and Taste on the same side, cured by Galvanism. By M. James, Interne at La Charité.

At the sitting of the Royal Academy of Medicine on the 21st of October, 1840, M. James presented a young man who had been affected for two years with paralysis of the fifth pair on one side, which had been cured by the aid of galvanism.

The patient was in the wards of M. Andral, and stated that after neuralgia of the side of the face corresponding to the paralysis, which lasted eight days, the paralysis supervened. On his admission there was absolute loss of sensibility of the whole of the skin receiving the three sensitive branches of the fifth pair. It was proved that the conjunctiva, the nasal and lingual mucous membrane, and that of the cavity of the tympanum on the affected side, had no tactile sensibility. The special sensibilities of these four organs were also completely abolished on the affected side. M. James was convinced that the base of the tongue did not perceive tastes on the affected side, and was not sensible of the contact of a body. The muscles receiving the nerves of the motor branch were also paralysed, as proved by the unequal depression of the jaw, with an evident displacement outwards on the paralysed side, and impossibility of closing the jaws on that side. All these functional disorders yielded to galvanism applied, in a considerable number of sittings, to the different points affected. The organs of sense first recovered their sensibility; the tactile sensibility returned afterwards.

Gazette Médicale de Paris. Octobre 24, 1840.

On the Employment of the Sulphate of Alumina in the Ulcerations and Inflammations of Mucous Membranes. By M. Delmas.

The author of this paper concludes, from numerous observations, that alum dissipates the inflammation of mucous membranes ;and that, applied to solutions of continuity of these membranes, it accelerates their cicatrization, while its action is never injurious when carefully applied. It also rapidly accelerates the cicatrization of cutaneous ulcerations, though in this case it is apt to induce pain and reaction. In syphilitic ulcers it is a most valuable topical application.

Bulletin Général de Thérapeutique, 15 *et* 30 *Novembre,* 1840.

Fibres in the walls of the Gall-Bladder. By M. Barth.

M. Barth has presented to the Anatomical Society of Paris, a dilated gall-bladder, the walls of which offered very apparent fibres, interlaced, and having a great resemblance to muscular fibres.

L'Expérience. 19 *Novembre,* 1840.

On Acephalocysts in the Brain. By Dr. Michéa.

Acephalocysts are so rarely found in the brain, that we record a notice of two cases which we observed in the practice of M. Martin Solon. The symptoms in this first case, in a man 53 years of age, resembled those of sanguineous effusion into the right hemisphere of the brain, followed by hemiplegia. The dura mater was found very vascular, with slight serous effusion into the arachnoid cavity, and injection of the pia mater. Opaline vesicles were found in both hemispheres, but mostly in the anterior lobe of the left, of the size of large peas, containing a diaphanous liquid, the centre of which was occupied by a small, globular, opaque milky-white body, without head or tail visible by the microscope. The second case was an epileptic patient, aged twenty-three, in whom the acephalocysts were found in smaller numbers.

Gazette Médicale de Paris. 21 *Novembre,* 1840.

SURGERY.

Partial Injury of one of the Halves of the Spinal Marrow. By M. Begin.

L., a robust old soldier, aged fifty-nine, received a wound from a sharp-pointed cutting instrument in the back of the neck. He fell immediately, and being unable to raise himself, was obliged to be carried to an adjacent house. He said he had been knocked down by some heavy weapon, and that he had fallen not in consequence of the wound in the neck, which he thought insignificant, but from the violent stunning which he felt at the instant he was struck. He fell *backwards* and on his right side, and there was a slight bruise on the right elbow, but no other sign of external violence, although he attributed the impossibility of moving his right limbs to the shock and the bruises which he had received in his fall, or more still to the blows which he affirmed had been given him while on the ground.

The wound was united by plaster, and the patient, regarding his injury as a slight matter, refused to be bled. On the next day he was taken to the Val-de-Grace, where M. Begin first saw him, in the evening. He then said that he felt nothing but a little numbness of the right side. His pulse was slightly excited; the wound had healed, and he would not be bled. Cold was applied to the head and neck, and warmth to the feet, and antiphlogistic diet was ordered. He passed the night quietly. Next day, on examining the united wound more closely, it was found to be in a transverse direction, half an inch long, at the level of the fifth cervical vertebra, and nearly an inch from its spinous process. Its angles were nearly equally acute, rendering it probable that it had been made with a double-edged instrument; there was neither ecchymosis, nor swelling, nor heat, nor pain, nor hardness about it; the movements of the head and neck were perfectly free, and unaccompanied by pain, even when carried to a considerable extent. The patient felt a kind of weight in his right arm, and had creeping sensations in the hand; with a little difficulty, however, he could raise the arm, and move the fore-arm; but the fingers half-flexed could neither be opened nor closed completely. The right leg was absolutely motionless. A vague pain was felt along the right side of the chest, and there was perfect sensibility in every part of the body. The mind was clear: there was slight fever. The patient was bled freely both from the arm and locally; stimulants were applied to the feet, and cold to the head, and strict abstinence was observed.

On the third day after the accident there was a slight power of moving the right great toe. But there was much more fever, and the patient was again freely bled. On the fourth day, after a very quiet night, he was a good deal better, but at night he became feverish, excited, and delirious. This condition was somewhat relieved by leeches; but next day he had a violent shivering-fit, followed by incomplete reaction; at night he had low delirium, with an unequal pulse, difficult, hurried respiration, and hiccup, and with these increasing he died early in the morning of the sixth day after receiving the wound.

At the examination of the body, the wound externally was scarcely discernible, but more deeply its course became more and more evident by the effusion of blood in the adjacent tissues. In the middle of the right lamina of the sixth cervical vertebra there was a fragment of a knife, which projected backwards for about a line, and had its back directed towards the median line. Having removed all the cervical portion of the vertebral column, and cleaned its fore-part, the point of the same portion of knife was found projecting about one tenth of an inch between the sixth and seventh vertebræ. It had broken the upper edge of the body of the latter, and had stuck into, but not passed through, the posterior wall of the pharynx. The left side of the column was cut away, and the vertebral canal being thus opened, the medulla spinalis was found surrounded by pus mixed with the spinal fluid, and its surface was softened above and below the wound. It had been struck by the back or rather by the blunt edge of the blade of the knife, and the cut extended obliquely on the right side, from the

level of the origin of the posterior roots of the nerves to the anterior median furrow. The posterior column was uninjured from the line of origin of the posterior roots to the posterior median furrow, while the anterior column was divided from the same point to the groove occupied by the anterior spinal artery.

The portion of the weapon that remained in the wound was an inch and three quarters long, and three fifths of an inch broad at its base; it was part of a short and very sharp dagger-knife.

The shock that must have accompanied the violent penetration of the arch of the sixth vertebra, and the intervertebral cartilage, as well as the part of the body of the seventh vertebra, perfectly explains the sensation of bruising and the stunning which the patient felt at the instant of receiving the wound. Neither during life nor after death was there any erection of the penis.

[This case is interesting in every way, but especially in its obvious bearing on the functions of the columns of the spinal marrow, and the nerves respectively connected with them.]

Bulletin de l'Académie de Médecine. *Dec.* 15, 1840.

On the Removal of Foreign Bodies in the Joints by means of Subcutaneous Incisions. By Dr. Goyrand, of Aix.

This new method of operating was suggested by the fact, that incisions made into joints through such small external openings that the air cannot obtain admittance to the cavity, are never productive of that severity of inflammation which follows wounds with wide external apertures, and which, in the operations hitherto performed for this disease, has so often terminated fatally. The proceeding recommended by the author consists in pushing the loose body (if in the knee-joint,) into the synovial pouch above and to the outer side of the patella, beneath the vastus externus muscle, and while an assistant holds it fixed there, passing a narrow knife through the skin at some distance above the joint, and through all the intermediate tissues down to the foreign body. Without enlarging the opening in the skin, the synovial membrane and adjacent tissues over the loose substance are now to be freely divided, till, by the pressure on the latter, it slips out of the joint through the wound, and lodges itself in the subcutaneous cellular tissue, or in some of the other tissues between the skin and the joint. After this the patient must remain at rest for several days, (the small external aperture being merely covered by sticking-plaster,) till all chance of inflammation occurring has passed away. The foreign body dislodged from the interior of the joint, will form a cyst for itself, and remain in its new position without producing any annoyance; but, if it should be deemed necessary, it may easily be removed by a single incision through the skin over it, which will no longer be likely to excite any inflammation of the joint itself.

The author details a case in which this method of operating was adopted with perfect success. Two loose cartilages were dislodged, and pushed into the tissues adjacent to the joint. One was subsequently removed by an incision through the skin; the other was allowed to remain, and it produced no inconvenience whatever. Through the whole course of the treatment the patient had not a bad symptom, although less precautions were adopted than are usual in in this description of cases.

[We understand that Mr. Syme, of Edinburgh, performed this operation, and with complete success, without previous knowledge of M. Goyrand's proceeding.]

Annales de la Chirurgie Française et Etrangère. *Janvier*, 1841.

Successful performance of Tracheotomy in a case of Croup. By M. Saussier.

The case occurred in the hospital of la Pitié, in a boy two years old, who had been operated on for hare-lip, by M. Sanson. The first croupy symptoms appeared ten days after the operation, and after the unsuccessful employment of

leeches and emetics the child seeming to be in imminent danger of suffocation the operation was performed. White patches had formed upon the tonsils and arches of the palate, afterwards on the tongue and internal surface of the cheeks, and deglutition had become impossible.

The operation was followed by instant relief. M. Saussier cleaned out the trachea with a sponge, which he introduced several times, and upon which he withdrew a transparent pellicle, about two centimetres broad. Ten minutes afterwards, the canula was introduced, and the child almost immediately fell asleep.

During the three following days, eleven distinct, thick, yellow, false membranes were either expelled naturally by the patient, or withdrawn from the trachea by means of the sponge. The trachea was cauterized four times with a solution of sixty centigrammes of nitrate of silver in four grammes of water. The canula was changed thrice.

The canula was finally removed on the thirteenth day, and the teasing cough and frequent expectoration of thick mucosities which had continued till then, immediately ceased. On the sixteenth day the wound was healed, and at the end of August the child was discharged, having been operated on on the 3d of June.

L'Expérience. Dec. 3, 1840.

On the Efficacy of Chloride of Zinc in the Treatment of Acute and Chronic Gonorrhœa. By M. GAUDRIOT.

AFTER a large number of observations the author of this memoir concludes that the chloride of zinc, properly diluted, has a remarkable power in curing simple gonorrhœa of the urethra and vagina, also in dilating the urethra, and thereby ameliorating strictures. He believes that it is always by an inflammation *sui generis* that the discharge is cured, and that injections of the chloride of zinc determine a series of phenomena not produced by other caustics. It is only necessary to use a few drops of injection, as the extremity of the urethra is the only part to which it should be applied. To combat vaginal gonorrhœa in women M. Gaudriot proposes a new proceeding, which consists in the use of solid vaginal suppositories, composed of a paste made to melt easily, and containing a certain quantity of liquid chloride of zinc and sulphate of morphine.

In men the author employs the following formula:

Liquid chloride of zinc, 24 to 36 drops.
Distilled water, four ounces.
Agitate and filter through paper.

A small quantity of this solution should be injected about an inch along the urethra, two or three times a day.

The vaginal suppository which he employs is formed of

Liquid chloride of zinc, 5 drops.
Sulphate of morphine, half a grain.
Mix with three drachms of the following paste!
Mucilage of gum tragacanth, 6 parts.
Powdered sugar . . . . 3 „
Starch powder . . . . 9 „

To cure radically a gonorrhœa in man, it will ordinary suffice to apply two injections a day for two or three days. The first injections are almost always followed by more or less swelling of the glans penis, but this does not prevent their continuance.

In women four or six suppositories, one being introduced every day, or every second day, suffice to obtain a cure. The first introductions frequently occasion a swelling with more or less heat of the vulva, but these phenomena are soon completely dissipated. Emollient baths have in most of the cases been employed for this purpose.

Journal des Connaissances Médicales Pratiques et de Pharmacologie. Sept. 1840.

On Ligature of the common Carotid Artery, and Experiments on the Ligature of both Carotids in Animals. By M. JOBERT, of Lamballe.

THE author addressed a memoir to the Royal Academy of Medicine, containing a case where he tied the common carotid to cure an erectile tumour of the orbit, and also the result of his experiments on living animals, to determine the influence that ligature of both carotids would have upon them.

Ligature of the common carotid on either side does not stop the course of the blood through the ramifications of the tied artery; how then can it effect the cure of erectile tumours of the face and head? M. Jobert replies, 1st, by the sudden subtraction of a large quantity of blood from the tumour; 2, by giving an obstacle to the transmission of the impulse of the heart, in its full energy towards the tumour. He has convinced himself by his experiments, that beyond the ligature, the blood runs in a continual jet, waving, and without jerk (*saccade*). His conclusions are, 1st, that erectile tumours of the orbit, destroy the bones after the manner of aneurisms; 2d, they have the characters of aneurismal tumours, and are cured by ligature of the common carotid of the same side; 3d, the cure is not owing to obliteration of the artery beyond the ligature, but to diminished impulse of the column of blood arriving in the tumour; 4th, the vertebral arteries suffice for the cerebral circulation after ligature of the carotids; 5th, dogs, sheep, and rabits do not experience ill effects after this operation; 6th, horses on the contrary do not survive it, dying with pulmonary apoplexies; 7th, bloodlettings before or after the ligature, diminish the intensity of the pulmonary lesions; 8th, perhaps, in man, the loss of a certain quantity of blood after the operation, would have good effect.

Revue Médicale. Septembre, 1840.

MM. Bérard, Gimelle, and Larrey, who were appointed as a committee to examine the memoir of M. Jobert, consider it of importance; 1st, as adding two cases of cure of erectile tumours, one in the orbit by ligature of this carotid, to four other cases previously recorded; 2d, as the experiments prove the harmlessness of ligature of both carotids, in those animals whose vertebral arteries enter the cranium of a caliber sufficiently large to keep up the cerebral circulation and avoid pulmonary congestion; 3d, as it proves that the brain and the organs of the senses preserve their functions entire after this ligature; 4th, as it places beyond doubt, that animals who from their anatomical construction survive the ligature of the common carotid arteries, are not affected with lesions of the organs of the senses as described by authors; 5th, as these experiments and their results will exercise a great influence on the surgical therapeutics of diseases, for the cure of which ligature of the common carotids might be proposed.

Bulletin de l'Académie Royale de Médecine. 15 *et* 30 *Octobre,* 1840.

Stone in the Tonsils. By Dr. WIESNER, of Heydekrug.

A MAN seventy-six years old had for seven years often suffered from inflammation and swelling of the right tonsil. The affection was palliated by the means that were employed, but it often appeared again at different times after being heated and chilled, and at last a hardness of the tonsil remained, which gradually increased, becoming uneven and knotty to the touch, and, on the slightest contact causing severe pain, and rendering it difficult to swallow.

One morning, full seven years after the first occurrence of the angina tonsillaris, the patient on waking had, on attempting to eject some mucus that was collected in his mouth, felt a loose body in it, and drew out with his fingers a porous stone, like a urinary calculus and as big as two hazel-nuts. No bleeding occurred, and the opening in the tonsil soon closed after using an astringent gargle; at a later period, however, an inflammatory swelling of the left tonsil took place, and presented every appearance of a stone being in process of formation there.

Casper's Wochenschrift. August 15, 1840.

Radical Cure of Varicocele by means of Invagination and Shortening of the Scrotum. By Dr. LEHMANN.

THE mode of operating here recommended is very similar to that which the author has in several cases adopted successfully for the radical cure of hernia. A portion of the relaxed and elongated scrotum is to be pushed up on the fore-finger of the left hand, and *invaginated* into the part above it, till the finger reaches the external abdominal ring. Holding the parts in this position, a broad curved needle, with a double thread passed through an eye near its point, is to be carried along the left finger, through the bottom of the inverted portion of the scrotum, and to be made to penetrate it and the integuments immediately over the external ring. The thread is then to be removed from the eye, and the needle drawn back and again carried through the inverted portion of the scrotum and the integuments at a distance of about half an inch from the parts previously penetrated. The threads passed through the two apertures being now drawn, the invaginated portion of scrotum may be pulled up to any desired height; in general it must be drawn very nearly to the external ring; for in the subsequent progress of the cure the relaxation of the parts is so great that the folds and wrinkles thus produced are almost entirely obliterated. The threads then having been drawn must be knotted, and the parts thus tied up must be left for eight or nine days, under a soothing and cooling regimen, by which time adhesion will have taken place between them at the cut edges, and, by the excoriation which the discharge produces between the opposed surfaces of the inverted portion of the scrotum and that into which it is pushed.

The author relates six cases in which his mode of operating was adopted with success; but in none of them had sufficient time elapsed before the publication of the paper to enable one to say whether this measure was likely to be more permanently beneficial than the others which have been proposed for curing varicocele by bringing on the same change in the scrotum. Of all these the simplest and, as far as we have seen, the most successful, is that recommended by Mr. Wormald. (See Brit. and For. Med. Rev., vol. VI., p. 274.)

Med. Zeitung. Dec. 2, 1840.

Mode of Action of Cubebs and Copaiba. BY M. RICORD.

AT the sitting of the Royal Academy of Medicine on the 8th of September last, M. Ricord showed a design of a case of accidental hypospadias, resulting from a urinary abcess. The patient affected with this infirmity having contracted a gonorrhœa, it gave rise to some curious observations. The discharge first showed itself in the vesical portion of the urethra afterwards the part situated before the solution of continuity was invaded in its turn. Treated by copaiba, the vesical portion was soon cured, but the disease continued in the other part, and afterwards communicated it to the portion already cured. Cubebs was administered, and the discharge again ceased in the posterior portion of the canal. These facts show, according to M. Ricord, that cubebs and copaiba cure syphilitic discharges by the principles or properties which they communicate to the urine, and of which the urethra receives the influence by the passage of that fluid.

Archives Générales de Médecine. Octobre, 18 40.

Practical Considerations on the Treatment of White Swellings. By Dr. FORGET.

THE only novelty in this paper is an account of the employment of the muriate of baryta (chloride of barium,) in cases of chronic arthritic disease, as used in the hospital of La Pitié. The author considers it to be a most useful medicine, both in the acute and chronic state of white swellings, especially in scrofulous patients, and believes that in some cases it has alone sufficed to effect a cure, while usually great amendment has followed its use, though the cure may not

have been complete. The usual dose has been six decigrammes, continued for a month. The circulation is very generally lowered, the pulse being depressed from sixty or eighty to forty or even twenty-five [?] in the minute. Local bleeding and compression have been advantageously combined with the employment of the baryta.

Bulletin Général de Thérapeutique. 15 *et* 30 *Septembre,* 1840.

Extraction of a Prune-stone, which had remained eleven days in the Air-passages. By M. BONNET, Surgeon to the Hôtel Dieu, at Lyons.

THE symptoms in this case were convulsive cough, with *complete* remissions. The cough increased in severity and continuance; and eleven days after the accident death appeared so imminent that the parents objected to an operation as useless. The chest was clear on percussion, but there was such an absence of the respiratory murmur as could only be attributed to some mechanical obstacle to the entrance of air into the lung. The signs of elevation and depression of the foreign body could not be recognized. The operation of tracheotomy was performed in the usual manner, but six ligatures had to be applied to vessels of the thyroid gland. The stone was removed by forceps, and complete recovery followed. The child was eleven years of age.

Bulletin Général de Thérapeutique. 15 *et* 30 *Septembre,* 1840.

MIDWIFERY.

Cæsarean Operation saving the Lives of both Mother and Child. By Dr. KÖNIGSFELD, of Aachen.

AT noon on the 28th March I was called to a woman in labour who, in consequence of a remarkable narrowness of the pelvis, had had already for several days severe but ineffectual pains. I found that she was small and bore various signs of having suffered from rickets. The pains were very severe; the woman could scarcely stand; the liquor amnii had not yet escaped. External examination discovered a remarkable bending inwards of the sacrum, and a furrow, nearly an inch and a half deep, formed by the projecting forwards of the two last lumbar vertebræ. The ossa pubis diverging downwards were so much pressed in upon the abdomen above, that all the walls of the abdomen above them hung over them like a sack. The vagina was hot and very narrow; the os uteri as large as a dollar, and very hard; the membranes were moderately tense, and the child's head could be obscurely felt behind them, in the same position as, according to the midwife, it had occupied for the last four days. An accurate measurement showed that the conjugate diameter of the pelvis was only between two and two inches and a half long, and I therefore determined on the Cæsarean operation as the only means of effecting delivery; for, even if perforation and embryotomy could have been performed, they were forbidden by the child being still alive, and the patient rapidly sinking.

The incision was commenced half an inch on one side of the umbilicus, was then turned to the linea alba, and, extending for six inches, terminated close to the os pubis. The uterus presented at the wound, and was divided in the same direction as the external incision, and opened for an inch at its upper part. The membranes were now broken somewhat lower down, carrying the finger along on the knife, and the waters were allowed to flow *per vaginam;* the wound in the uterus was then carried on, and the membranes separated lower down. The right shoulder of the child now pressed into the wound, and was slowly pushed on, bringing after it the right arm, the chest, the buttocks, and the lower extremities; the head stuck in the wound, and required great care in removing it. The intestines, which were forcibly pushed forwards, were held back by large sponges, and the placenta was removed with the hand as soon as

possible. The hemorrhage, throughout the operation, was inconsiderable; and after as much as possible of that which had flowed, had been pressed out of the abdomen, the wound was united by suture and plaster. The operation lasted twelve minutes. Four hours after, bloodletting was found necessary to calm the violent inflammatory excitement that had commenced; but after this, with only mild treatment and perfect abstinence (for hunger, says the author, is the best antiphlogistic,) the case went on perfectly well; the wound was healed, and the patient entirely recovered four weeks after the operation. The child also was healthy from its birth, and grew very rapidly.

Casper's Wochenschrift. December 5, 1840.

Singular Case of Tumour in the Pelvis. By Professor von D'OUTREPONT, of Würzburg.

A WOMAN, 26 years old and well made, gave birth when 25 years of age to her first child without difficulty. Towards the end of her second pregnancy she again applied at the hospital in consequence of experiencing pain in the pelvic region. Vaginal examination discovered a hard and painful tumour, extending from the inner surface of the left ischium nearly to the corresponding point on the opposite side. It was hard, globular, even on its surface, and occupied the ascending ramus of the ischium and the descending ramus of the pubis, and extended over the obturator foramen. It was impossible to reach the lower segment of the uterus, or to feel any part of the child.

The size and hardness of the tumour seemed to leave no chance of the birth of a living child, even by the induction of premature labour. Professor D'Outrepont, who doubted whether the tumour was fibro-cartilaginous or a true bony exostosis, asked the opinion of many eminent men who saw the case. They did not express themselves with certainty as to its nature, and the patient refused to allow an experimental incision to be made into the tumour.

A short time before labour began, the tumour was thought to have become slightly compressible. When labour commenced, the professor called a consultation in which it was determined that unless a great change had taken place in the character of the tumour, an attempt should be made to remove it, or to cut away the bone if that should be found to be implicated, and as a last resource to perform the Cæsarean section.

On an examination being made, the right foot of the child was found to present, the cord was prolapsed, and did not pulsate. The tumour, however, was found to be so much softened that it was possible to pass three fingers through the outlet of the pelvis. Professor D'Outrepont brought down the foot, in doing which he found that the hips had compressed the tumour still more. The chief difficulty was experienced in extracting the head by means of the forceps, which gave the patient considerable pain. The child was still-born but was speedily recovered. After the birth of the child the tumour regained its former size, so that the placenta could not be expelled by the natural efforts, and it was necessary to introduce the hand in order to remove it.

The patient recovered rapidly, and returned ten weeks after her delivery, in order to have the tumour removed, which operation was performed by Professor Textor. The growth was found to be fibro-cartilaginous, and was connected neither with the bone nor the periosteum. It weighed 11¼ ounces, and was so hard that none but they who were present at the patient's delivery, could have believed its previous softening possible. The patient was completely cured.

Neue Zeitschrift für Geburtskunde. Band ix, *S.* 1.

MEDICAL POLICE

On Kyanised Timber. By Dr. Schweig, of Carlsruhe.

The discovery made in England by Mr. Kyan, namely, that wood might be preserved from dry-rot and the destructive attacks of insects, has now, we are told by the author, come into such general use on the continent, as to require an examination into its influence on public health. Kyan's process consists in saturating wood thoroughly, by long immersion, with a solution of corrosive sublimate, in the proportion of two pounds of sublimate to one hundred measures of water.

In regard to the influence of this process on public health, the following points require attention:

1st. From the ready absorption of this poisonous liquid through the skin, the mere manipulation of the timber exposes the workmen to much danger.

They should be warned not to dip their hands in the liquid more than can be avoided. They should wear linen which ought to be frequently washed; the water used in washing either their persons or their linen should have muriate of ammonia dissolved in it, so as to precipitate and separate the poison more effectually. Lastly, the earth on which any of the poisonous liquid may fall should after a time be dug up and buried in a deep hole.

2d. As a matter of medical police, it is proper to enquire whether under certain circumstances mercury may not be volatilized from wood thus prepared, and produce its usual dangerous effects upon those exposed to the vapour.

The occurrence of prejudicial effects from this cause has been denied, because the crews of vessels built of Kyanised timber have returned healthy after long voyages, even in tropical latitudes. Where there is a free access of air, as in the wood used in railways (sleepers), no danger from this source is to be apprehended; but when the wood is employed in the construction of close apartments, in damp or ill-ventilated places the case is different. Experience may not have hitherto shown that dangerous consequences have followed; but if there be reason to suspect the possible occurrence of these on physical grounds, this is sufficient to justify a government in interfering to prevent the use of such timber for the construction of houses.

3d. It need hardly be said that wood thus impregnated with corrosive sublimade, is wholly unfitted for the making of vessels to hold articles of food or drink, either for the use of man or animals.

4th. A fourth and very important consideration is, that Kyanised wood when used up cannot be safely employed for fuel like ordinary wood; the mercury contained in old wood might be thus volatilized and spread in vapour through the apartments of a house, leading to the slow destruction of life by poison, by giving rise to protracted illness. It could only be safely burnt in close stoves or vessels, where there was a free current of air to carry off the mercurial vapour.

The chips or cuttings of wood of this description, might unknowingly be employed as fuel by labourers and others engaged in working the timber.

From the preceding remarks it may be inferred, that some precautions should be taken by a government in allowing the application of this process; and it is at the same time a duty to ascertain whether some harmless substitute equally preservative may not be found for the deadly drug at present used. The author says that a substitute of this kind is now well known and much employed, namely, the sulphate of copper. This effects the same chemical changes on the soluble principles of wood as corrosive sublimate; and although being a poison the wood cannot be used for the manufacture of domestic utensils, yet the metal copper is fixed, and when the wood after use is burnt, there is no fear of its giving out poisonous vapours. Owing to this it might also be safely employed in the construction of houses or in buildings for the use of man and animals. It is then a fair question whether a government ought not to prohi-

bit the use of timber thus saturated with sublimate, and recommending as a substitute a solution of the sulphate of copper.

[We consider this to be a very important paper, and deserving the attention of a board of medical police, if there were such in this country. Fortunately the process in England is patented, and this circumstance has, to the great benefit of the community, restricted its application. Should the use of Kyanised timber spread after the expiry of the patent, which we believe there is little fear of, as the process is likely to be superseded by a more effective one, we may expect that cases of poisoning will be frequent, not so much in consequence of the fragments of wood being used for fuel, as from its being ignorantly used in the construction of barrels, tubs, cisterns, spouts for the holding of water, milk, wine, or other liquids. It is absurd to say that its use should be restricted to out-of-door purposes; since you cannot provide knowledge for the lower classes of artificers; and whether by accident or through ignorance, Kyanised wood will be frequently used for purposes not contemplated by the inventor of the process. The bare possibility of a danger of this sort occurring is, as Dr. Schweig observes, at once a sufficient justification for a government to interfere.

We believe that the author is mistaken in thinking that sulphate of copper is equally effective as corrosive sublimate, imperfect as even this is, and he seems quite ignorant of the more recent discovery of Sir William Burnett, that *chloride of zinc* is greatly superior to both in the preservation of wood, and yet more in the preservation of sail-cloth and cordage. The great superiority of this agent is yet further enhanced by its want of poisonous qualities. We believe Sir William Burnett has obtained a patent for this discovery, and that it is deservedly taking the place in this country of the more dangerous process of Mr. Kyan.]

Henke's Zeitschrift, f. d. S.A.; 1, 1840.

CHEMISTRY.

On a peculiar Substance occurring on the Human Teeth. By F. BUEHLMANN, of Bern.

THOSE bodies, which occur amongst the *tartar* of the teeth, mixed with earthy matter and epithelium cells, were described by Leeuwenhoeck. (Op. omn. i, t. ii, p. 40.) They are filiform, and are found in three different conditions. 1st, beautiful delicate fibres, which seem to sprout from a mass of granular yellowish substance, as the stems of plants do from bulbs, and which are usually collected in tufts; 2d, the same fibres broken and scattered among the epithelium and mucus; and 3d, masses of fibres like those first mentioned, mixed up with the yellowish granular matter.

The first of these forms is the most beautiful, and perhaps the original one. At first the fibres look very like seminal animalcules, as they are sometimes collected in tufts, or like some kinds of fungi. They are about 0·00006 of a Paris inch broad, and their length varies considerably, from about one tenth to one half a line, judging by the author's plates of them. At their free extremities they are gradually acuminated: they are smooth, yellowish white, somewhat transparent, beautifully arched or wavy, and elastic. They break straight across their length, and appear homogeneous.

These fibres occur only on the teeth; they are most numerous where the tooth adjoins the mucous membrane, and especially where a piece of membrane dips into a gap between two teeth; and they are most abundant about teeth that are covered with a tartarous deposit, though not absent on the cleanest. Neither the strongest nitric, sulphuric, nor hydrochloric acid, nor caustic potash produces any change in their appearance beyond that of making them rather more transparent: combustion chars the substance around them, but leaves them quite unaltered.

Müller's Arkiv. Heft iv. 1840, p. 442.

II. THE BRITISH JOURNALS.

(FOR THE QUARTER ENDING MARCH 1841.)

List of the principal Medical Papers published in the British Journals during last Quarter.

THE EDINBURGH MEDICAL AND SURGICAL JOURNAL.

No. CXLVI. JAN.—On the Anatomical Relations of the Blood-vessels of the Mother to those of the Fœtus in the Human Species. By John Reid, M.D.

On some points in the Anatomy of the Medulla Oblongata. By John Reid, M.D.

The Properties and Influence of Arteries on the Circulation of Blood. By G. Calvert Holland, M.D.

An Inquiry into the Mechanical Functions of the Ear. By James Sym, M.D.

Case of Aortal Aneurism, which opened into the Vena Cava Superior, opposite the entrance of the Vena Azygos. By William Young, M.D.

Two Cases of Impracticable Labour arising from Malcosteon of the Pelvis, in which the Cæsarean Operation was performed; accompanied with Practical Observations. By Thomas Radford, M.D.

A Description of a Double Monocephalic Human Monster, which was transmitted to this country from South America, by Robert Mackay, Esq., British Consul at Maracaylis, Venezuela. By Eric Mackay, M.D.

Malta, considered with reference to its eligibility as a Place of Residence for Invalids. By Francis Sankey, M.D.

On the Diurnal Variations of the Pulse in Disease. By William Augustus Guy, M.B. Cantab.

Observations on the Use of Piperine in the Treatment of Intermittent Fever. By Robert Hartle, M.D.

[In cases where quinine failed, Dr. Hartle found (in Trinidad,) the piperine succeed, in doses of 3 grs. every hour.]

Observations on the Therapeutic Action of Croton Oil in certain Nervous Disorders. By P. S. K. Newbigging, M.D.

Case of Amputation of the Neck of the Womb followed by Pregnancy; with Remarks on the Pathology and Radical Treatment of the Cauliflower Excrescence from the Os Uteri. By James Y. Simpson, M.D.

On Sanguineous Tumours on the Scalp of New-born Children. By Francis Black, M.D.

Two Cases of Rupture of the Carotid Artery from Sphacelus. By Mr. C. J. Mill.

[In cynanche maligna.]

Cases Illustrative of the Division of Tendons. By Mr. W. Rhind, Surgeon.

Appendix to Dr. J. Reid's Paper on the Anatomical Relations of the Blood-vessels of the Mother to those of the Fœtus.

EDINBURGH MONTHLY JOURNAL OF MEDICAL SCIENCE.

No. I. JAN.—Surgical Cases and Observations. By James Syme, Esq. F.R.S.E. &c.

On the Pathology of Laryngismus Stridulus. By W. Henderson, M.D. Edinburgh, &c.

Contributions to Forensic Medicine. By John Reid, M.D. Edinburgh, &c.

Case of Scirrhus of the Stomach, probably Congenital, with Remarks. By Thomas Williamson, M.D. Edinburgh, &c.

Case of Luxation during a Fit of Epilepsy, with Remarks. By W. A. F. Browne, M.D. &c.

On the Preparation of Protiodide of Iron. By T. and H. Smith, Chemists.

On the Cause of Ciliary Motion. By Edward Forbes, M.W.S. &c.

No. II. FEB.—On the Varieties observed in the Symptoms, &c. of Poisoning with Opium. By David Skae, Esq. F.R.C.S. Lecturer on Forensic Medicine, &c.

On Bleeding in Mania. By W. A. F. Browne, M.D. Superintendent of the Crichton Lunatic Asylum, Dumfries.

On the Changes of the Colour of the Iris, produced by Inflammation. By James Hunter, M.D. Surgeon to the Edinburgh Eye Dispensary.

THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

[This plan, taken from a method recommended by Dr. O'Beirne in cases where difficulty is experienced in introducing the elastic tube into the intestinal canal, consists in injecting warm water through the catheter while it is being gently forced past the obstruction.]

"The injecting apparatus which I have employed is very simple; it is formed by merely attaching a small bladder to the end of the catheter, after the manner of a mounted enema pipe; the bladder need not be large, one that holds about half a pint of water will be sufficiently so; and the catheter must be furnished with a small cork, having a piece of twine fixed to it, just like the cork of an enema pipe. The largest sized catheter the urethra will admit of should be used, that the impetus of as large a jet of water as possible may act on the obstructed part of the canal, and also that the urethra being filled by the instrument, the fluid may be easily prevented from returning by the side of the catheter. The eye must be large, and very near the end of the tube; or, what is better, the tube should have a large orifice within the circumference of its anterior extremity, with the edge rounded in so as that it shall not hurt or catch the lining membrane of the passage. Also it must be a silver or permanently curved elastic catheter: for the injecting apparatus would prevent the withdrawal of the stilet necessary to maintain the curvature of a common elastic one. The instrument, having the attached bladder properly adjusted, is to be passed down to the obstruction: if it cannot now, on further trial, be made to enter the patient's bladder, it must be held by an assistant, while the small cork is being inserted into its outer end: about six ounces of lukewarm water are to be poured into the injecting apparatus; the latter must then be closed, and tied as near as possible to the fluid, so as to exclude every portion of air. The operator is next to encircle the penis with the finger and thumb of his left hand, making gentle circular pressure to close the urethra round the catheter; he will then, having first withdrawn the cork, embrace the bladder of water with his right hand, so as to be able to apply strong and uniform pressure on as much of its surface as possible, in doing which, its contents being continuously and

forcibly propelled into the urethra, and the handle of the instrument being at the same time depressed, the latter at once passes with facility into the patient's bladder."

[This method has been successfully employed by Dr. Patterson in some cases of enlarged prostate, and also in one case where the difficulty originated in inflammation of the urethra.]

On Asphyxia, resulting from Wounds in the Neck. By Gabriel Stokes, M.D.

Cases of the Venereal Disease, illustrative of the Introduction of the Venereal Poison into the System, through other Channels than by Sexual Intercourse. By Clement Hamerton, Surgeon to the Castletown Dispensary, Nobber.

An Attempt to estimate some of the characteristic Marks, by which to judge of the Cause of Perforations of the Stomach. By Thomas Williamson, M.D. one of the Physicians to the Leith Dispensary, &c.

Clinical Report of Sir Patrick Dun's Hospital. By Charles Lendrick, M.D. T.C.D. Queen's Professor of the Practice of Medicine, &c.

THE LANCET.

No. XI. Dec. 5.—Observations on the Effects of Dividing the Tendons of the various Orbital Muscles in Animals. By E. W. Duffin, Esq. Surgeon, London.

On the Proper Mode of Treating the Insane. By Arthur Stilwell, Esq. Surgeon.

[Advocates mild restraint in certain cases.]

On the Tincture of the Muriate of Iron in Discharges of Blood from the Urethra, Leucorrhœa, and Dysmenorrhœa. By Charles Clay, Esq. Surgeon, Manchester.

[Says he has cured many cases by the tinct. ferr. ℳ viij. and tinct. opii ℳ xj. every four hours.]

On the Application of Strapping and Pressure in Orchitis. By J. Dixon, Esq. Surgeon, Coleford.

[Fricke's method. (See Br. and For. Med. Rev. vol. II. p. 253.) Mr. D. wrongly gives the merit of the plan to Mr. Cross.]

No. XII. Dec. 12.—Injection of Nitrate of Silver in Gonorrhœa. By W. H. Foster, Esq.

[Recommends an injection of a solution of nitrate of silver, gr. iij. to the oz. in the primary stage of gonorrhœa.]

Medical Jurisprudence. Inquests in Middlesex, held before Mr. Wakley, M.P. Recorded by Mr. George I. Mills. Sudden Deaths—Proximate Causes.

Hemorrhagic Diathesis: Cases in Illustration. By David Burnes, M.D. London.

[Good examples of this not very uncommon diathesis in three brothers: one case fatal, with the dissection.]

Retained Placenta after Premature Labour. By Charles Ray, Esq. Surgeon, London.

[Single case. Placenta retained thirty-one days.]

Additional Observations on Sulphur Lotum in Rheumatic Affections, and the Operation for Varicose Veins in the Legs. By Charles Clay, Esq. Surgeon, Manchester.

Case of Stricture of the Œsophagus. By Charles Gravenor, Esq. London.

No. XIII. Dec. 19.—Medical Jurisprudence. Inquests in Middlesex, held before Mr. Wakley, M.P. Recorded by Mr. George I. Mills.

On the Humane System of Treating the Insane. By Andrew Blake, M.D. Physician to am General Lunatic Asylum.

Effects of Restraint on the Insane. By Arthur Stilwell, Esq. Surgeon.

No. XIV. Dec. 26.—Medical Jurisprudence. Inquests in Middlesex, held before Mr. Wakley, M.P.

Operation for Lateral Curvature of the Spine. By G. B. Childs, Esq. London.

[Single case.]

Case of Epileptic Catalepsy. By James Thomas, Esq. Newcastle Emlyn.

No. XV. Jan. 2.—Case of Strangulated Femoral Hernia; Operation; Successful Result. By Charles Braddon, Esq. Surgeon, Crediton.

Case of Hydatids in the Brain, unattended with Symptoms during Life. By William Sturton, Esq. Surgeon, Greenwich.

On the Injurious Employment of Emmenagogues in Vicarious Discharges. By Edward Boulger, Esq. Surgeon, Reading.

[Single case.]

Objections to the Use of Nitrate of Silver as an Internal Remedy. By Charles Ray, Esq. Surgeon, Eaton square.

On the newly-discovered and speedily-forgotten Instruments, and the new Plans and the old Plans, for performing the simple Operation for the Cure of Squinting. By Charles Clay, Esq. Surgeon, Manchester.
On the frequent Occurrence of Caries in the Temporal Bones. By William Hughes, Esq. Surgeon, Holborn.
On the Non-existence of Infection in Gonorrhœa. By Mr. Chippendale.
On the Erroneous Supposition that a Cicatrix after Vaccination is Proof of a Successful Operation. By John Epps, M.D.
On the Advantages of the Sling Fracture-bed in Cases of Fracture, with Illustrative Cases. By H. G. Potter, F.L.S. Surgeon, Newcastle-upon-Tyne.

No. XVI. JAN. 9.—On the Humane System of Treating the Insane. By Andrew Blake, M.D. Physician to the Nottingham General Lunatic Asylum.
On the Treatment of the Insane in Private Houses. By George Stilwell, Esq.
Statistics of Multiple Births. By R. H. Mackenzie, M.D. M.R.C.S. Chelsea.
Case of Retained Placenta and Polypus of the Uterus. By P. L. Burchell, Esq. Surgeon.
On the Poisonous Effects of Laburnum Seeds. By T. A. B. Bonney, Esq. Surgeon.
Double Uvula; from Median Arrest of Development. By George Bolster, Esq. Surgeon, London.

No. XVII. JAN. 16.—On Acute Rheumatism; its Causes, Symptoms, and Mode of Treatment. By David D. Davis, M.D. Professor of Midwifery in UniversityColl. London.
On the Action of Sulphuric Acid on the Cornea. By R. Dundas Thomson, M.D. London.
Collection of Fluid in the Substance of the Right Lobe of the Thyroid Body. By J. Massey, Esq. Surgeon, Nottingham.

No. XVIII. JAN. 23.—Cases of Aneurism of the Thoracic Aorta. By Robert Boyd, M.D. Resident Physician to the St. Marylebone Infirmary, &c.
Case of Suspected Empyema. By J. Gorringe, Esq. University College Hospital.
On the Employment of Oxymuriatic Acid as a Remedy for Disease. By F. d'Alquen, Esq.

No. XIX. JAN. 30.—On the Minute Pathology of Pneumonia. By Thomas Williams, M.B. Surgeon, Demonstrator of Anatomy in the Webb-street School of Medicine.
A New and Successful Method of Treating Disease of the Prostate Gland; with a Wood Engraving. By W. Henderson, M.D. Perth.
[The new method consists in the application of leeches near the affected part, within the rectum, by means of a particular tube.]
On the Treatment of the Insane. By Arthur Stilwell, Esq. Surgeon.
Cases Illustrative of Hemorrhagic Diathesis. By T. Smethurst, Esq. Surgeon, Ramsgate.
[Two cases (females) not of the same family.]

No. XX. FEB. 6.—Reflections on the Treatment of Variola by the "Ectrotic Method;" principally with a View of Preventing the Formation of Cicatrices. By J. F. Olliffe, M.D. President of the Parisian Medical Society.
[An elaborate essay on the method of treating smallpox by the local application of mercurial plasters, noticed more than once in this Journal. Dr. Olliffe speaks most favorably of the new plan. "Its effect is either to prevent the development of the pustules, or so to modify them that they become mere abortions."]

Case of Aneurism of the Pulmonary Artery. By S. W. Fearn, Esq. Surgeon, Derby.
On the Substitution of the Oxide of Silver for the Nitrate. By Thomas P. Dennett, Esq. Surgeon, Storrington.
[In the treatment of epilepsy, &c. with the view of avoiding the cutaneous discoloration.
On the Humane System of Treating the Insane; Letters from Dr. Blake, of Nottingham, and Dr. Browne, Physician to the Crichton Institution.
On the Employment of Brandy and Salt. By Robert Dick, M.D.
On the Treatment of Sciatica. By H. C. Roods, Esq. Surgeon, London.

No. XXI. FEB. 13.—On the History of the Employment of Cinchona Bark in the Treatment of Acute Rheumatism. By David D. Davis, M.D. Professor of Midwifery in University College.
On Ivory Exostosis of the Fibula; Removal of Eight Inches and a Half in Length of the Diseased Bone. By J. Massey, Esq. Surgeon, Nottingham.
On a Peculiar Form of Infantile Convulsions. By W. J. West, Esq. Surgeon, Tunbridge.
Epistaxis; Cured by Plugging the Nares with Putty.
On the Employment of the Unguentum Æruginis in the Treatment of Burns and Scalds. By Edwyn G. S. Gurney, Esq. Surgeon, Cumborne.

Removal of a Pendulous Tumour from one of the Labia Pudendi. By William Howitt, Esq. Senior Surgeon of the Preston Dispensary.

No. XXII. Feb. 20.—An Outline of Chinese Anatomy. With illustrative Wood Engravings. By G. Tradescant Lay, Esq. Surgeon, London.

On a Case of supposed Aneurism of the Aorta, relieved by appropriate Treatment. By John Gay, Esq. Surgeon, London.

On the Employment of Ergot of Rye in Premature Labour. By Joseph Hodgson, Esq. Surgeon, London.

[A single case.]

No. XXIII. Feb. 27.—New Method of Treating Cases of Purely Functional Neuralgy. Exhibition of the Galvanic Factors. By Robert Dick, M.D.

[This method consists in "the simultaneous administration (internally) of the ordinary *galvanic factors*, zinc, copper, or nitric acid."]

On the Application of the Sling Fracture Bed to the Treatment of Compound Fractures of the Leg. By William Morrison, Esq. Durham.

On the Employment of Burnt Rhubarb in Diarrhœa. By F. P. Hoblyn, Esq. London.

[Strongly recommended. The rhubarb is burnt in an iron crucible until it loses two thirds of its weight. The dose is gr. v–x.]

THE LONDON MEDICAL GAZETTE.

No. XI. Dec. 4.—On the Results of Amputations. By J. A. Lawrie, M.D. Professor of Surgery in Anderson's University, Surgeon to the Glasgow Royal Infirmary, Glasgow Lock Hospital, &c.

[A valuable statistical paper.]

No. XII. Dec. 11.—On Congenital Opacity of the Cornea. By Mr. S. Crompton, of Manchester (with Engraving).

On the Detection of Albumen in the Urine. By G. O. Rees, M.D. &c

Observations on the Advantages presented by the Employment of a Stethoscope with a flexible Tube. By Golding Bird, M.D. A.M.

On a New Method of Cure for Shortsightedness. By Aug. Franz, M.D.

[Berthold's method, mentioned in our last Number.]

On Lateral Curvature of the Spine; and on Strabismus. By James Braid, Esq. M.R.C.S.E. of Manchester.

On the Division of the Muscles of the Back in a Case of Lateral Curvature of the Spine. By T. Laycock, Esq. York.

Dissection of a Club-Foot. By Philip B. Ayres, M.B. M.R.C.S. of Thane.

No. XIII. Dec. 18.—On the Colourless Globules in the Buffy Coat of the Blood. By Mr. Addison, of Great Malvern.

Case of Poisoning with Binoxalate of Potass. By John Jackson, Esq. M.R.C.S.

No. XIV. Dec. 25.—On the Treatment of certain Diseases of the Brain. By E. Copeman, Esq.

Cases of Cynanche Tonsillaris. By Joseph Bell, Esq. of Banhead.

No. XV. Jan. 1.—Case of Œdema of the Face and Tongue, Ulceration, &c. By James Syme, Esq. L.R.C.S. Edin. of Alloa.

On the Position of the Placenta. By Joseph Bell, Esq. of Banhead.

On Chyle and Lymph. By G. O. Rees, M.D. Member of the Royal College of Physicians of London, and Physician to the Northern Dispensary.

No. XVI. Jan. 8.—Note on the present Epidemic of Smallpox, and on the Necessity of Arresting its Ravages. By William Farr, Esq.

On the Treatment of Ulcers of the Lower Extremities. By J. Bell, Esq. of Barrhead.

On the Structure of Cartilage. By James Douglas, Esq. of Glasgow.

Case of severe Injury of the Head. By Edmund J. Furner, Esq. of Brighton.

Case of Dislocation of the Tibia inwards, with the Astragalus, and Fracture of the Fibula. By R. G. Coombs, Esq. of Newcastle, Staffordshire.

No. XVII. Jan. 15.—On Phlebitis of the Cerebral Sinuses as a Result of Purulent Otorrhœa. By James Bruce, M.D. Liverpool, formerly House-Surgeon to the Glasgow Royal Infirmary.

On the Coagulation of the Blood after Death. By J. Paget, Esq. M.R.C.S. Demonstrator of Morbid Anatomy, and Hon. Curator of the Museum, at St. Bartholomew's Hospital.

Case of Double Uvula. By George Bolster, Esq. of Tullerboy, Croom, Ireland.

THE DUBLIN MEDICAL PRESS.

No. CVII. Jan. 20.—Case of fatal Asphyxia from covering the face in bed. By R. Cranfield, M.B. Enniscorthy.

No. CXII. Feb.—On Diseases of the Genito-Urinary Organs in Childhood. By Chr. Fleming, Esq. M.D.

Some Observations on Hooping Cough. By Henry Kennedy, M.B. L.R.C.S.I.

PROVINCIAL MEDICAL AND SURGICAL JOURNAL.

No. X. Dec. 5.—A fatal case of Diabetes, with Remarks. By Charles Hastings, M.D.

No. XI. Dec. 12.—Contributions to the Pathology of Children. By P. Hennis Green, M.D. (Scrofulous Tubercle of the Brain, continued in three Numbers.)

No. XII. Dec. 19.—Cases of Diabetes, with Remarks. By C. A. Bree, Esq. Stowmarket. Dissection of two Cases of Rabies. By Mr. V. Pettigrew, M.D.

No. XIII. Dec. 26.—Case of Ischuria Renalis. By George Fife, M.D. Newcastle.

No. XIV. Jan. 2.—Case of Hydrophobia. By J. Toogood, Esq. Bridgewater.

No. XVII. Jan. 23.—Case of Polypus of the Uterus, and of Viper Bite. By J. Toogood, Esq.

No. XVIII. Jan. 30.—Case of Monstrosity. By S. N. Parsons, Esq. Wincanton.

No. XX. Feb. 13.—Case of Scirrhus of the Optic Thalamus and Corpus Striatum. By J. Waters, M.D.

THE LONDON, EDINBURGH, AND DUBLIN PHILOSOPHICAL MAGAZINE.

No. CVII. Nov. 1840.—Observations on certain Peculiarities of Form in the Blood-corpuscles of the Mammiferous Animals. By S. Gulliver, Esq. F.R.S.

On the Structure of the Placenta. By Dr. John Reid, F.R.C.P.E., Lecturer on Physiology, &c.

It is with great pleasure that we find ourselves again called upon to notice results of Dr. Reid's industry and skill in the prosecution of anatomico-phy logical enquiries; and our pleasure is the greater in the present instance, bec his recent labours have given what is to our minds a very satisfactory sol of a long-disputed question of no slight interest, the connexion betwee fœtus and the uterus of the human female. We shall briefly state the fac served by Dr. Reid, and his inferences from them.

In dissecting the uterus, with the attached placenta, of a woman who h suddenly during the seventh month of pregnancy, Dr. Reid's attention tracted towards a number of rounded bands, passing between the uterine of the placenta, and the inner surface of the uterus. These bands [whi distinct from the utero-placental vessels described by the Hunters] we rally observed to become elongated, thinner, and of a cellular appearan put on the stretch, and were easily torn across; whilst at other times ... more rarely, *they could be drawn out in the form of tufts from the* ... Some of these tufts were found to be much more ... an inch from the open mouth of

the intestines are supported in the abdominal cavity, by the reflexions of the peritoneum.) It is on account of this kind of union, that the tufts cannot readily be withdrawn from the sinuses, the connecting vessel usually giving way in preference.

With this clue Dr. Reid proceeded to examine minutely the structure of the placenta itself; and he succeeded in demonstrating that the greater part of its substance exactly corresponds with the tufts just described; being composed of bundles of filaments interwoven with each other, each filament being composed, like those of the tufts, of a ramification of the umbilical artery and vein. There is no cellular or any other tissue filling up the spaces between the branches of the fœtal placental vessels; the so-called cells being merely the interstices between the interlacing tufts, which tufts do not seem, in spite of their close approximation, to be actually connected with each other. Upon tracing the coats of some of the utero-placental veins, which contained none of the tufts, and of the curling arteries, through the decidua it was found that they were reflected over these prolongations of the umbilical vessels within the placenta; just as the lining of the uterine sinuses is reflected over the tufts which projects into them. The interior of the placenta is thus shown to consist of the ramifications of the umbilical vessels, the blunt terminations of which are ensheathed in prolongations of the inner coat of the vascular system of the mother, or at least in a membrane continuous with it.

If we adopt this view of the structure of the placenta, the blood of the mother when it passes into that organ, through the curling arteries of the uterus, may be regarded as entering a large sac formed by the inner coat of the vascular system of the mother, and this is intersected in many thousands of different directions, by the placental tufts prosecting into it like fringes, and pushing its thin wall before them in the form of sheaths, which thus closely envelope both the trunk and each individual branch composing these tufts. Round this sac the maternal blood is returned by the utero-placental veins, without having been extravasated, or without having left her own system of vessels. Into its cavity, the tufts of the placenta hang, like the branchial vessels of aquatic animals, and are bathed in the blood of the mother, just as are those tufts which project into the sinuses. This sac is protected and strengthened on the fœtal surface of the placenta by the chorion; on the uterine surface by the decidua vera (of the existence of which in this position Dr. Reid has satisfied himself), and on the edges or margin by the decidua reflexa. The blood of the mother and that of the fœtus can readily act on one another through the spongy and cellular walls of the placental vessels and the thin sac ensheathing them; just as the blood in the branchial vessels of aquatic animals is acted on by the water in which they float. According to this view of the structure of the placenta (which, when once understood, is found to be characterized by extreme simplicity, as well as by conformity to analogy), there is no proper division into fœtal and maternal portions, since tufts of placental vessels with their blunt terminations may be found immediately under the chorion covering its fœtal surface, as well as towards the uterine surface.

From a small number of observations made upon uteri and placentæ of different ages, Dr. Reid is disposed to conclude (though he by no means wishes to state it as a positive fact) that the degree in which the tufts of fœtal vessels project into the uterine sinuses increases with the advance of gestation. When the placenta is detached, at the conclusion of delivery, it would seem that most if not all of the tufts are retained within the uterine sinuses, being kept there by the connexions already mentioned. We think it not unlikely, however, that some may be occasionally drawn out, but escape observation; and we would suggest to those of our readers whose practice affords them facilities for so doing, that they take pains to ascertain this fact, by immersing the placenta in water, as soon as may be after its separation, and carefully observing whether any such tufts hang down from the placental surface, when it is placed horizontally and on looking downwards. Whether this be the case or not,

however, the existence of the tufts has been placed beyond a doubt by Dr. Reid, who has satisfied Professors Alison, Allen Thomson, and J. Y. Simpson of their character; and Dr. Sharpey has given the weight of his testimony, based upon independent observation. Feeling, as we do, great confidence in Dr. Reid's caution and sagacity as well as anatomical skill, we have little hesitation in according with the general view of the structure of the placenta, by which he has been led by this interesting discovery; and we trust that it will soon receive full confirmation from the researches of others.

Dr. Reid's paper contains, besides an account of his own researches, a summary of the opinions of others on this *questio vexata*. Of these, Weber's views appear to present the nearest approach to his own. Weber had not noticed any prolongation of the fœtal vessels beyond the placenta; but he had spoken of the continuation of the venous canals of the mother into the placenta, where, according to him, they dilate into large sinuses, upon the walls of which the placental tufts are not only ramified, but also project into the interior, carrying the walls of the sinuses before them, much in the manner described by Dr. Reid. The chief difference between the two sets of views consists therefore in this,—that Dr. Reid considers the interior of the placenta in the light of one large sac, continuous with the uterine vessels, having its cavity divided by the interlacement of the ramifications of the fœtal vessels, which are covered by the reflexions of the lining of the sac, into numerous small cells communicating with each other, whilst, according to Weber, several smaller cavities or sinuses are contained in the substance of that organ.

Those who are curious in Morphological Science will not have much difficulty in perceiving a strong analogy between the mode in which the fœtal blood is thus renewed, by being brought into relation with the maternal fluid, and that in which the blood is aerated in the mollusca. The relative positions of the placenta and of the branchiæ (the structure of which has been shown to be essentially the same) in respect to the heart, rectum, &c., have a very close correspondence.

Edinb. Med. and Surg. Journal. Jan. 1841.

On some Points in the Anatomy of the Medulla Oblongata. By John Reid, M.D. F.R.C.P.E., &c.

The object of this short paper is to correct an error which prevails in all the anatomical works of this country, in reference to the connexion between the spinal cord and the medulla oblongata, which has given rise to much theoretical difficulty, in regard to the origins of some of the nerves. The error is in the idea that the decussating fibres of the anterior pyramids pass down the anterior columns of the spinal cord; the fact being that they pass down the middle or lateral columns. If the course of the pyramidal fibres be traced to their decussation, it will be found that those which cross each other pass backwards as well as downwards, so as to unite with the fibres continuous with the restiform bodies in forming the lateral or middle columns; and Dr. Reid states it as the result of repeated examination, that *none of the decussating fibres pass down the anterior columns.* In this assertion he is supported in part by Rosenthal, Cruveilhier, and Arnold, who all speak of the pyramids as principally derived from the middle columns. But a portion of the pyramidal fibres (the amount of which seems to vary much in different cases) does not decussate; and these unite with those of the olivary bodies, to form the anterior column of the cord. Besides these, there is the *arciform* band of non-decussating fibres, which goes backwards to join the posterior column as it passes into the crus cerebelli.

The decussation of the fibres of the middle column may be examined from behind, by detaching the posterior columns, and has been described as such by Sir C. Bell; but he does not state that the decussating fibres pass into the pyramidal column of the opposite sides, and not into the middle columns.

The olivary bodies are separated from each other at the upper part of the medulla oblongata by the pyramids; but when the main body of the fibres of the latter pass backwards, the olivary columns approach each other, so as to be only separated by the anterior median fissure. On tracing the fibres of the olivary column upwards, they are said by Dr. Reid to inclose the olivary body like a kernel, and then to pass into the pons varolii, where they divide into two bands, of which one proceeds upwards and forwards to join the crus cerebri, whilst the other proceeds upwards and backwards to the corpora quadrigemina.

The olivary columns afford attachment to the anterior roots of the first and second cervical, and of all the other spinal nerves; they may therefore be regarded, without much hesitation, as forming a motor tract. On tracing it upwards, it is found to give origin to the *portio dura*; the *hypoglossal* and *abducens* in part arise from it, and in part from the pyramidal column; the *small root of the fifth*, and the *trochlearis* arise from that portion of it which runs to the corpora quadrigemina. The *large root of the fifth* is traced by Dr. Reid into the restiform or middle column; from which also arise all the fibres of the *glosso-pharyngeal*, and nearly all those of the *par vagum* (which are not connected with the olivary bodies, as Mr. Solly supposed); a few of these last being usually seen to arise from the arciform band of the pyramidal column, when this is well marked. The *spinal accessory* nerve is believed by Dr. Reid to originate from the decussating fibres of the pyramidal columns; but on this point he does not speak with certainty.

Of the function of the decussating fibres, Dr. Reid thinks we have at present no sufficient means of forming an opinion. Their connexion with the roots of the spinal nerves is by no means evident; and their position throws great difficulty in the way of experiment. Dr. Reid observed that in two kittens deprived of sensation and voluntary motion by prussic acid, extensive muscular movements were produced by irritating the pyramidal bodies with the point of a needle; but whether the impression thus produced was conveyed directly to the muscles or by a reflex action, he could not satisfy himself.

Edinburgh Medical and Surgical Journal. Jan. 1841.

On Bleeding in Mania. By W. A. F. BROWNE, M.D. Superintendent of the Dumfries Asylum.

DR. BROWNE not only condemns the use of bloodletting in mania, but goes so far as to say that "in nine cases out of ten, when a professional man even of eminence is called to a case of recent mania, he orders general depletion, the liberal exhibition of the solution of tartar emetic, brisk cathartics, and cold applications to a shaven scalp. If the symptoms, and especially the violent and rapid pulse, continue or return in unabated force, the patient is perhaps bled again from the arm, or if not, he will be cupped and leeched to a certainty. And after these energetic measures are manfully urged for days or weeks, and the tartar-emetic lustration fails in every respect, except in producing a nausea quiet, an asylum is recommended as the last resource." Dr. Browne then states that such treatment is not confined to mania: "The melancholic monomaniac and maniac, and all the species included under these general terms, are its objects or its victims. The subject is too momentous for ridicule; but ridiculous and indefensible must that system be which includes all the foregoing diseases under one category, and draws blood and drenches upon the same principle and with the same degree of discrimination as a leech."

We agree entirely with Dr. Browne in thinking the subject too momentous for ridicule, but we consider it as at the same time too momentous for exaggeration; and we cannot help fearing that in his anxiety to warn his readers against the injury arising from the indiscriminate use of the lancet in mental affections, Dr. Browne has unconsciously and unjustifiably allowed himself to bring a strong general charge against his brethren, which, if true to the extent which he alleges, would be a reproach and disgrace to them. We believe

that in many cases bleeding and tartar emetic *are* used where they had been better omitted. But so far as our experience and information go, we have no hesitation in affirming that instead of nine out of ten intelligent general practitioners following the culpable course here ascribed to them, those who do so constitute only a small minority of the whole. From the strong language used by Dr. Browne, and his charge being specially extended to the intelligent and eminent among general practitioners, we are forced to the conclusion that he must have either overstated his case, or been unusually unfortunate in meeting with so many instances of mismanagement. We concur cordially with him in his objections to general bleeding and nauseating in mania, in his anxiety for the early removal of the insane to an asylum, in the general propriety of a generous diet, and we are as earnest as he can be that no cases of maltreatment should ever occur. But for that very reason we contend that he ought either greatly to have restricted his charge against his professional brethren, or *to have produced proofs to substantiate it.* We know that cases such as he describes are to be met with more frequently than is creditable to the parties concerned, but after twenty years' experience we can honestly aver that instead of their amounting to nine cases out of ten, of those treated by well-informed and sensible practitioners, as Dr. Browne assumes, they have been few and far between compared with those treated without general depletion. And on enquiry among our medical acquaintance, we should say that instead of nine out of ten practitioners, "even of eminence," falling into the error imputed to them, the large majority would condemn it as heartily as Dr. Browne himself. Appearing, as this statement now does, *without proofs* it is calculated to rouse in the minds of Dr. Browne's brethren an unfriendly feeling towards himself, and thereby to diminish his influence when really in advance of them, by diminishing their confidence in his accuracy and judgment.

Edinburgh Monthly Journal. Feb., 1840.

On the Advantages of a Stethoscope with a Flexible Tube.

By GOLDING BIRD, M.D. A.M.

THE end which is applied to the chest consists of a thin cup of ebony, an inch in diameter, and carefully rounded at its edges: a flexible tube, from sixteen to twenty inches in length, formed of a spiral iron wire, covered with caoutchouc, and bound tightly round with silk or velvet, having an internal diameter of about one fourth of an inch, is fixed into the apex of this cup: the other end of the tube is cemented into a perforated ebony ball, on the top of which is screwed a slightly concave plate of ivory, two inches in diameter, also perforated in its centre. When this instrument is used, the ebony cup should be held, between the fingers and thumb of one hand, against the walls of the chest, and thus, with the utmost facility, and without producing unnecessary pressure, a tolerably air-tight approximation is effected. The ebony ball at the other end should be held in the other hand, and the ivory plate closely applied to the ear. In this manner we have a column of air, bounded at one extremity by the vibrating surface of the chest, and at the other by the membrana tympani, which thus becomes placed in the position most favorable for assuming vibrations throughout its whole extent. The cup of ebony has its vibrations *damped* by the pressure of the fingers grasping it, whilst the covering of silk or velvet performs a similar office, with sufficient accuracy, for the spiral wire forming the tube of the instrument, whilst from the convenient position in which the physician can place himself with regard to the patient, the necessarily close approximation of the end of the stethoscope to the surface of the chest may be effected. In addition to this, the intensity of sound does not appear to be sensibly diminished by the flexure of the tube, and thus we can auscultate the sides and back of the chest by a very slight movement of the body of a patient whilst in the recumbent position.

London Medical Gazette. Dec. 11, 1840.

PART FOURTH.

Medical Intelligence.

TABLE OF MORTALITY FOR LONDON,

For the Fourth Quarter of 1840:

Showing the number of Deaths from all causes registered in fourteen weeks, from the 28th September, 1840, to the 2d January, 1841.

Causes of Death.	Total Deaths.
Class I.	
Smallpox	738
Measles	383
Scarlatina	465
Hooping-cough	301
Croup	107
Thrush	69
Diarrhœa	87
Dysentery	19
Cholera	7
Influenza	22
Typhus	346
Erysipelas	110
Syphilis	3
Hydrophobia	0
Total Epidemic, &c.	2650
Class II.	
Cephalitis	159
Hydrocephalus	461
Apoplexy	265
Paralysis	243
Convulsions	738
Epilepsy	62
Insanity	12
Delirium tremens	23
Dis. of brain, &c.	106
Total Dis. of Brain, &c.	2069
Class III.	
Quinsey	17
Bronchitis	160
Pleurisy	27
Pneumonia	1506
Hydrothorax	66
Asthma	470
Consumption	1858
Dis. of lungs, &c.	226
Total Dis. of Lungs, &c.	4330
Class IV.	
Pericarditis	10
Aneurism	18
Dis. of the heart, &c.	270
Total Dis. of Heart, &c.	298
Class V.	
Teething	245
Gastritis, enteritis	221
Peritonitis	13
Tabes mesenterica	80
Ascites	11
Ulceration	23
Hernia	24
Colic or Ileus	19
Dis. of stomach, &c.	87
Hepatitis	23
Jaundice	23
Dis. of the liver, &c.	99
Total Dis. of Stom. &c.	868
Class VI.	
Nephritis	8
Diabetes	7
Stone	2
Stricture	5
Dis. of the kidneys, &c.	45
Tot. Dis. of Kidneys, &c.	67
Class VII.	
Childbed	119
Ovarian dropsy	3
Diseases of uterus, &c.	47
Tot. Dis. of Uterus, &c.	169
Class VIII.	
Rheumatism	39
Diseases of joints, &c.	41
Total Dis. of Joints, &c.	80
Class IX.	
Ulcer	7
Fistula	4
Diseases of skin, &c.	5
Total Dis. of Skin, &c.	16
Class X.	
Inflammation	86
Hemorrhage	44
Dropsy	522
Abscess	50
Mortification	62
Scrofula	27
Carcinoma	68
Tumour	19
Gout	10
Atrophy	89
Debility	388
Malformations	13
Sudden deaths	296
T. Dis. of Uncert. Seat.	1674
Class XI.	
Old age or nat. decay	1047
Class XII.	
Intemperance	9
Privation	10
Violent deaths	334
Total by Violence, &c.	353
Class XIII.	
Causes not specified	64
T. Deaths from all causes Males 6901, Females 6655	13556

ON SECLUSION, AS EMPLOYED AT HANWELL IN THE TREATMENT OF THE INSANE.

[AMONG the numerous misrepresentations that have gone abroad respecting the admirable improvements introduced into Hanwell by Dr. Conolly, there has been none greater than that which has converted the temporary and occasional seclusion of violent patients into an habitual system of restraint and punishment as severe and objectionable as the practices he has abolished. In order to expose the utter inaccuracy of such statements, we lay before our readers the section on SECLUSION, from the Report recently presented by Dr. Conolly to the county magistrates; and we can testify from personal knowledge of the proceedings at Hanwell, that the practice of the Asylum is in strict accordance with the principles there laid down.]

Seclusion. All the substitutes for restraint are, like restraint itself, liable to be abused; but none can be made such instruments of cruelty by abuse. All are also liable to great misrepresentation: and none more so than that which is of all the most useful, the most simple, and the most approved of by the highest medical authorities; namely, seclusion. By seclusion is meant, temporary protection of the maniac from the ordinary stimuli acting upon the senses in the refractory wards of a lunatic asylum. He is abstracted from noise; from the spectacle of a crowd of lunatics; from meeting those who are almost as violent as himself; and from every object likely to add to his irritation. But the mode in which seclusion is effected is also important to securing the benefits of it. If resorted to with violence, if accompanied with expressions of anger or contempt, if stigmatised as a punisment, and if followed by neglect, it may produce all the evil moral effects of restraint itself. If injudiciously persevered in in very recent cases, it exasperates instead of calming. The patient requires freedom of action; is relieved by strong muscular exercise; and this should be provided for by such a subdivision of airing courts as would leave one for the occasional use of a single patient, at least for a few hours in the day. After being indulged in active voluntary exercise for an hour, two hours, or such period as may seem desirable, the patient should be secluded. Calmness and sleep will sometimes follow; or sufficient tranquillity to enable the attendants and officers to talk to the patients with effect.

Under the system of restraints, when a patient became noisy and violent, and particularly when some mischief had been committed by him, it was considered necessary, and it was the usual practice, to overpower him, and to put him in some kind of strait-waistcoat. This was done with great difficulty, and with much danger to the attendants. Observation has convinced the resident physician that this was a useless, and even hurtful, mode of management. It was like endeavouring to smother a fierce fire by heaping very combustible meterials upon it. A maniac in the midst of his paroxysm, like a man in a violent fit of passion, should be interfered with as little as possible. The violence, which if met by violence will become still more aggravated, will often, if left to itself, subside even in the course of five or ten minutes. Whatever the duration of the violent accession, its continuance is a bar to anything but such management as protects the patient and those about him. It is in intervals of calmness that the foundations of moral treatment must be laid, and the confidence of the patient gained. To acquire this confidence is the key-stone of all moral treatment; and nothing will so much oppose its acquisition as brutal or even impatient usage during the paroxysm.

In the meantime, supposing the violent state to continue, the other patients should be removed from the neighbourhood of the one who is excited; all obvious means of mischief should be guarded against; and the attendants, although not directly interfering, should be watchful and ready. A soothing word now and then, or something new to attract the attention, may be admissible; but all with discretion. Supposing, as must often happen, that the violent fit does not immediately subside, and that the patient continues to vociferate, to swear, to abuse and threaten those about him, and to endeavour to strike them, some-

thing more must be done. But in this condition the worst thing that can be done is to struggle violently with him, to overpower him, and to mortify him by putting on bodily restraint. The only reasonable object of any treatment is to tranquillize the violent man: and of all modes of effecting this, surely violence is the most unreasonable. Seclusion effects the object more certainly than restraint, and without any violence at all. A lunatic is seldom, even in his most raving fits, insensible to what is said to him: he will often show, among his wildest and most extravagant expressions, that he is watchful of the conduct of those about him; and when the ordinary observer would expect nothing from him but what indicated savage fury, those who are patient with him, and who, regardless of his wildness, continue to indicate their kind feelings towards him, will find that sometimes his voice falters, and his eyes fill with tears. These symptoms of emotion are very transient; but they show that the sensibilities are not quite suppressed; and they warn the practitioner, in language that ought not to be disregarded, to abstain from everything which can further wound or oppress the feelings of an almost ruined mind. These circumstances are now mentioned as bearing upon the manner in which the seclusion of a violent patient should be effected. Three or four attendants, possessed of courage and good temper, should surround him; and telling him that he would be much better if quiet, and in his own room, should endeavour, by gentle occasional efforts, to induce him to walk into it. It will sometimes be found, that although he protests loudly against the measure, his steps gradually proceed in the direction required. At the same time, steadiness and strength may be required to prevent his retrograding; but well-qualified attendants will not on this account resort to violence. If he strikes or kicks them, they must of course effect their purpose as speedily as possible, and with steadiness, and even with force; but always without passion. As soon as the patient is thus placed in his room, he is not unfrequently found to become quiet; or if he continues to talk loudly, it is not for a long period. In all probability he will soon lie down on his bed, and go to sleep. If he continues violent, he is at all events out of harm's way. He will very seldom attempt to hurt himself; and he can hurt no one else. The window of his room should in all cases be secured by an efficient shutter and lock. The bedstead, which should be of wood, should be fastened to the floor, and remote from the window. Sufficient light should be admitted through holes made in the window-shutter to enable the attendants, by looking through the inspection plate in the door, frequently to ascertain the state of the patient.

The abuse to which this seclusion is liable is that of being too prolonged. A troublesome patient being once locked up, it is natural that the attendants should have no very anxious desire to let him out: but if the superintendent is assisted by officers of proper activity, this abuse should be impossible. The seclusion should in all cases be immediately reported; and after two or three hours, some officer of the asylum should visit the patient, or at least look at him through the inspection-plate, so as to judge of the propriety of the seclusion being continued or put an end to. This should be done with great circumspection. If even the cover of the inspection-plate is moved roughly and noisily, the patient is roused and irritated. Still greater mischief may be done by prematurely going into his room. A daily report should be made to the superintendent of the patients who have been in seclusion, and the number of hours they have been secluded. It is impossible to lay down rules for the general length of seclusion. Three hours, two hours, or one hour, in many cases answer every purpose. In other instances, after four or five hours, although the patient should be brought out, and a trial given of his capacity to behave well, it is not found practicable to have him at large among the other patients; and it is better to keep him in his room till the next day. Many patients liable to periodical excitement, and especially females, are far more comfortable if kept in seclusion during the whole period of their excitement.

When the female patients are put in seclusion in consequence of any sudden outburst of passion, a very usual effect upon them is a vehement fit of crying

and sobbing. In such cases, after a short interval, it is desirable that the patient should be talked to and soothed; and the seclusion should not be prolonged. If such a patient is neglected, seclusion will only produce sullenness. There are other cases in which a female patient, who would strike and kick those about her if at large, will become instantly quiet when put in seclusion; and, when seen through the inspection-plate, will seem so extremely tranquil, as to make it appear to those unacquainted with her, that she might be suffered to be at large without inconvenience: but if the door is incautiously opened, such a patient will be found to spring suddenly upon the intruder. The character of such patients becomes familiar to the attendants; but from perverseness, or obstinacy, they sometimes thwart the intention of the superintendent by letting out the patient to astonish a visitor, or by prolonging the seclusion when they know it to be no longer necessary.

In very young female patients, disposed to occasional violence, seclusion requires to be applied with peculiar precaution. The darkness appears to alarm them; and the solitude seems to aggravate the paroxysm. In all respects these patients require particular attention; the greater number of them being curable; yet so strongly affected, favorably or unfavorably, by all the circumstances around them, as easily to be rendered incurable by neglect or mismanagement.

There are many patients subject to paroxysms of excitement of about a week's duration, who, of their own accord, will keep in their rooms at such a time; and who, although the door is not locked, will seldom offer to come out. There were no patients more injured by the imposition of restraint than these: the character of some of them, even during their most excited state, is improved since its discontinuance; and, at other times, instead of being a terror to the attendants and the officers, they are among the most affectionate and grateful patients in the house.

The resident physician dwells more minutely on seclusion, because he considers it as one of the most important of curative means, and as one of the least objectionable substitutes for every kind of restraint. It is open to no objection which is not doubly applicable to restraint. All the possible evils of seclusion were included among the innumerable evils of bodily coercion. While the patients who were permitted to walk about in restraint were still capable of inflicting injury upon others, they were not protected from causes of irritation, or from the attacks of other patients. When put in seclusion, it was a seclusion which did not tranquillize. The arms or the hands were closely confined to the body; or the arms, or the legs, were strapped or chained to the bedstead; or the head was confined by a strap round the neck. In this state they were left for days or for weeks, in the most miserable condition in which a human being could be placed; and often to the total ruin of all habits of cleanliness. The patients themselves, who now come to us from other asylums, reported, "violent and dirty," sometimes remark, that they could not be otherwise than dirty when they are chained down in a deep bed like a trough. The same patients, being freed from all restraint the moment they arrive at Hanwell, seldom prove dirty, and not always violent.

To obviate every objection to seclusion, all the resources of the non-restraint system must be brought to bear upon it. The destruction of bedding and of clothing should be prevented by bedding properly secured in ticking covers, strongly sewed; and by clothing of the same material, fastened by small locks instead of buttons. If the patient will not lie in bed, warm boots, similarly fastened, should be constantly worn. So important do even trifling matters become as auxiliaries to this kind of treatment, that it may be right to mention, that the ticking should be of the best and strongest manufacture, and carefully sewed with the strongest thread; or, in the case of male patients, with twine. Without proper precautions of this kind, the attendants will very probably report that the dresses and blanket-cases are useless. The attendants should be continually exhorted to watch for all favorable opportunities to get the patient out of his room and into the open air: and the cleanliness of his apartment and

person, and the proper administration of food to him, should be most scrupulously observed.

All proper medical means are compatible with this treatment: and although some cases must be expected to be much more troublesome than others; although, indeed, it is known to all familiar with insanity, that there are cases, in which more or less of maniacal excitement will continue for six, eight, or ten months; yet under this treatment, the management of such cases will be found less distressing; the temper and habits of the patients more controllable; and the return to reason steadier, and made with more gratifying circumstances, than where the confidence of the patient has been shaken, and the excitement of the malady aggravated, by violence of any kind whatever. From the statements then made by the recovering patients, the superintendent will learn, and having learned, ought never to forget, that every act of violence, that every word of irritation, that every injudicious expression, of which the attendants were guilty, or into which he himself was betrayed, during the most excited period of the patient's malady, remains recorded in the patient's mind; and that no act or word of kindness, no remission of severity, no little indulgence, no encouragement held out to the poor sufferer, passed unregarded.

OBITUARY.

SIR ASTLEY COOPER,—HIS LIFE, CHARACTER, AND WRITINGS.*

SIR ASTLEY COOPER was the fourth son of the Rev. Dr. Samuel Cooper, of Yarmouth, in the county of Norfolk. His mother was the daughter of Mr. James Bransby, of Shottisham, a co-heiress descended from the family of Paston, Earls of Yarmouth, a lady distinguished by high intellectual attainments, and known as the author of a work of fiction called "The Exemplary Mother." Astley Cooper was born at Brooke, in Norfolk, on the 23d of August, 1768, where he remained till the age of fourteen, receiving his rudimental education at the village school, and the higher branches of learning from his father and the Rev. Joseph Harrison, a distinguished classic. His mind was early directed to the study of surgery, and he was, when about fifteen years of age, placed with Mr. Turner, who was at that time a general practitioner at Yarmouth. Here he only remained a few months, when he came to London, and was apprenticed to Mr. William Cooper, his uncle, then one of the surgeons to Guy's Hospital. Shortly afterwards, at his own desire, he was transferred to Mr. Cline, at St. Thomas's Hospital. The early part of his pupilage was not marked by that unremitting industry which afterwards distinguished him; and it was not until after he had spent a short time in Edinburgh in 1787, and had been appointed demonstrator of anatomy at St. Thomas's Hospital, under Mr. Cline, that his great natural powers were called forth, and matured by the utmost industry in the dissecting-room and the wards of the hospital. In 1791 he began to give a portion of the anatomical course in conjunction with Mr. Cline. At this time no distinct courses of lectures on the principles and practice of surgery were given in London, the maxims of the day being included in the anatomical course; but Mr. Cooper, with the concurrence of the surgeons of Guy's and St. Thomas's, commenced the lectures, since so well known by the publication of repeated editions, and which very soon became the most popular of the day. Towards the close of 1791 he married Miss Cock, of Tottenham, a relative of Mr. Cline. In 1792 he went to Paris, and attended the practice and lectures of Dessault and Chopart. He commenced practice in the same year, residing for six years in Jeffrey's Square, St. Mary's Axe. He then removed to New Broad Street, where he remained until the year 1815, when he removed to the West-end, having been surgeon to Guy's Hospital since 1800. He lived in the house now occupied by Mr. Bransby Cooper, 2, New Street, Spring Gardens, and

* From No. 22 of the Provincial Medical and Surgical Journal.

appeared in an English school; an enthusiast in his profession, and eloquent in its praise, he poured forth the treasures of his clear understanding with an energy, frankness, and affability, that, combined with his liberal feelings and engaging countenance, completely fascinated his auditors. His class for some years exceeded four hundred in number; and we never yet saw a man who had heard him who was not loud in his praise, and grateful for his instructions. As an hospital-surgeon he was distinguished for the amenity of his deportment, his ready tact in the discrimination of disease, and the no less subtle ingenuity in forming plans for its cure. As an operator, though never particularly neat, he was always expert, rapid, and unusually successful. His industry is probably without a parallel in the history of the art. He was always a very early riser; and even at the time when he was employed from six in the morning till midnight in attendance on his public and private patients, with the duties of his lectureship, he would spend great part of many nights in anatomical and pathological pursuits, as is proved by the statements of his friends, and the fact that his most laborious works appeared while he was in the zenith of his popularity. Even when past the usual age of mental activity, when he had reaped the "otium cum dignitate," so far from luxuriating in the ease of mental repose, he composed his last work on the Anatomy of the Breast, which is founded on upwards of two hundred and fifty preparations in his private museum. During the period he was constantly employed, he maintained the habit of noting down all interesting occurrences in his case-book, which are preserved from 1794 to his latest days, affording the most valuable materials for the illustration of surgical precepts.

Let us now turn to the character of Sir Astley as a scientific man, and consider for a moment the discoveries which have alike established his own fame, advanced the science he professed, and increased the resources of the working surgeon in the daily exercise of his art. The mere enumeration of these is all that is necessary to place the name of Sir Astley high among the most renowned benefactors of humanity, and to afford on the slightest knowledge of the comparative state of surgery fifty years ago and at the present day, the most startling conviction of the immense influence which may be exerted over a class or a nation by the labours and talents of a single individual. It is not half a century since it was doubted in our schools whether the hip-joint was ever dislocated; and those who admitted the possibility of the occurrence, doubted the practicability of its reduction. Cases were constantly met with in the hospitals where dislocations had been treated as fractures until the period had passed in which reduction could be effected; and others, perhaps equally numerous, in which irreparable injury had been inflicted by pulling a fractured limb, under the belief that it was dislocated. Sir Astley cleared up this cloud of ignorance and error; and now, as a result of his researches, almost every fracture and dislocation is readily recognized by the merest tyro, and their treatment rendered more simple and efficacious.

We turn to hernia, and trace similar improvements to the same source. The various species of hernia have been distinguished from each other, and from the different diseases with which they had been, or might be, confounded. The anatomy of the parts through which the intestine might protrude, and its various coverings after protrusion, were carefully investigated, with the effect of rendering our knowledge of the descent far more precise, increasing our means of preventing strangulation, and making the operation after strangulation had occurred far more safe and effectual.

The experiments of Sir Astley on the ligature of arteries, and the collateral circulation subsequently set up, followed by his bold, but strictly warrantable, operations on the carotid and aorta, had an equally remarkable effect on the surgical therapeutics of aneurism. He was the first to demonstrate the practicability and safety of tying the carotid in the living subject; and it is fair to conclude that this operation has been the means not only of curing diseases otherwise fatal, but that it has led to a far more philosophical view of the treat-

ment of aneurism than was received in the time of Hunter. He was the first to tie the aorta; and subsequent facts have shown that this will, in all probability, confer on the surgeon the power of directly saving life in some cases.

The work of Sir Astley on the Anatomy and Diseases of the Testis affords another example of the effect of his labours in advancing surgical science; various diseases being distinguished from malignant growths and depositions, and made readily curable, for which the sufferers were formerly doomed to castration. Indeed, there is scarcely a department of surgery, the practice of which has not been vastly improved by his unwearied industry and practical tact. He was the first to remedy obstruction in the Eustachian tube by puncturing the membrana tympani; the first to let off the fluid of spina bifida by repeated punctures, and thereby cure a disease previously considered beyond the reach of art. He was the first who cut into the membranous portion of the urethra through the perinæum, rather than puncture the bladder, either above the pubes or through the rectum; the first who practised the removal of exostoses by paring off their investing periosteum, thereby removing the medium of their nutrition, causing their death, and exfoliation. His latest work, on the Anatomy of the Breast, is one which will always be the standard authority on the subject, and is only equalled by his volume on the Non-malignant Diseases of the Breast, a work which is distinguished for the diagnosis it contains of the simple from the malignant diseases, and the sound precepts laid down as to the tumours which might be removed without a fear of their return,—points of the first interest to the practical man.

We have purposely abstained from filling our pages with the various anecdotes which have been going the round of the daily and weekly periodicals, knowing many of them to be apocryphal and others decidedly false, and have now merely to add a list of the different contributions for which the profession is indebted to Sir Astley, all of which we hope to see published in a collected form with the life of their author, under the able editorship of his nephew. In answer to some illiberal attacks in the "Chronicle," ascribing *avarice* to Sir Astley, we have only to state that, in addition to his well-known generosity during life, he has freely endowed a perpetual studentship, to be in the gift of the College of Surgeons, as a lasting memento of his devotion to the science he professed.

For the following list of the various published writings of Sir Astley Cooper we are chiefly indebted to the memoir which appeared in Mr. Pettigrew's "Medical Portrait Gallery."

1798. Two papers in the "Medical Researches;" one on a case of diaphragmatic hernia; the other detailing three instances of obstruction to the thoracic duct, and showing, by dissection, the channels through which nutrition was carried on.

1800. A paper in the "Philosophical Transactions," entitled, "Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear."

1801. In the "Philosophical Transactions," an "Account of an Operation for the Removal of a particular species of Deafness." For this paper he obtained the Copley medal of the Royal Society.

1804. His work on hernia appeared, entitled, "The Anatomy and Surgical Treatment of Inguinal and Congenital Hernia." A second edition was brought out in 1827.

1805. In the Edinburgh Medical and Surgical Journal we find a "Case of Malformation of the Genito-urinary Organs."

In the Transactions of the Medico-Chirurgical Society, we find—

1809. Vol. I. Two cases in which the carotid artery was tied, once successfully.

1812. Vol. II. "Dissection of a Limb on which the Operation for Popliteal Aneurism had been performed." Also, "Some Observations on Spina Bifida."

1813. Vol. IV. "History of a Case of Premature Puberty," and "An Account of the Anastomoses of the Arteries of the Groin."

1815. Vol. VIII. "Three Cases of Calculi removed from the Bladder without the use of cutting Instruments." This paper was written to show the extreme dilatibility of the female urethra.

1817. Vol. XI. "Account of a Case in which numerous Calculi were extracted from the Urinary Bladder of the Male without the employment of cutting Instruments."

1818. Vol. XII. Contains the history of a case in which a fatty tumour, weighing upwards of thirty-seven pounds, and measuring eighteen inches around its neck, was removed from the walls of the abdomen, the patient completely recovering.

1818—20. The surgical essays of Sir Astley and Mr. Travers appeared, those of the former being—1. On Dislocations. 2. The well-known Case of Ligature of the Aorta. 3. On Exostoses. 4. On Dislocations and Fracture of the Hip and Knee-joint. 5. On unnatural Apertures in the Urethra. 6. On Encysted Tumours. We have alluded to all these with the exception of the last, which is written to prove that encysted tumours take their origin in obstruction and enlargement of the sebaceous follicles.

1822. His great work appeared, "A Treatise on Dislocations and Fractures of the Joints;" and in

1823. An Appendix was added, on fractures of the neck of the thigh-bone, in consequence of the strictures of Mr. Earle.

1829. We have "Illustrations of the Diseases of the Breast. Part I." And in

1830. "Observations on the Structure and Diseases of the Testis."

1832. "The Anatomy of the Thymus Gland."

1836. In the first number of the Guy's Hospital Reports there are papers on the anastomoses of the femoral and inguinal vessels, and an account of the *post-mortem* examination in 1821 of the patient whose carotid artery he had tied in 1808. In the subsequent numbers we have a minute dissection of an unusually formed placenta and imperfect fœtus, and some observations on the thyroid gland contained in a paper by Mr. King. Also, "Some Experiments and Observations on Tying the Carotid and Vertebral Arteries, and the Pneumogastric, Phrenic, and Sympathetic Nerves." "On Spermatocele, or Varicocele of the Spermatic Cord." "On Dislocation of the Os Humeri upon the Dorsum Scapulæ, and upon Fractures near the Shoulder-joint." And the last paper he ever published, which was in the number for October, 1840, the "Dissection of a supposed Hermaphrodite."

His work "On the Anatomy of the Breast," appeared in 1839. His "Lectures on the Theory and Practice of Surgery" have gone through various editions. The only authorised edition is that of Mr. Tyrrell, of which only three volumes have appeared, in 1824, 1825, and 1827.

Sir Astley was engaged until his death in the completion of other publications and the correction of his older ones; and we understand that most of his papers are in a state which will render their arrangement far from difficult; and it is to be hoped that the public will shortly be favoured with the completion of his work on the Breast, which will comprise the various malignant diseases of this organ.

BOOKS RECEIVED FOR REVIEW.

ENGLISH.

1. On Diseases of the Hip-joint; with observations on affections of the joints in the puerperal state. With plates. By William Coulson, Surgeon to the Magdalen Hospital, &c. Second Edition, with alterations and additions.—London, 1841. 8vo, pp. 211.

2. Practical Hints on the Cure of Squinting, by operation. By F. W. Grant Calder, Assistant Surgeon to the Second Regiment of Life Guards.—Lond. 1841. 8vo, pp. 96.

3. Memoranda regarding the Royal Lunatic Asylum, Infirmary, and Dispensary, of Montrose; with observations on some other institutions of a like nature, and an appendix of documents, partly relating to restraint in the treatment of insanity. By

R. Poole, M.D. Superintendant of the Montrose Asylum.—Mont. 1841. 8vo, pp. 278.

4. Malta considered with reference to its eligibility as a Place of Residence for Invalids. Addressed to the members of the medical profession. By Francis Sankey, M.D. Universitatis Melitensis.—*No date.* 8vo, pp. 23.

5. A Second Appeal to the People of Pennsylvania on the subject of an Asylum for the Insane Poor of the Commonwealth. —Philadelphia, 1840. 8vo, pp. 35.

6. The Retrospect of Practical Medicine and Surgery, No. II. By W. Braithwaite, Surgeon.—London, 1840. 4s. 6d.

7. Medical Reform, in a Letter to Lord Melbourne. By Martin Sinclair, M.D.—London, 1841. 8vo, pp. 36.

8. The Anatomy of the Nerves of the Uterus. By Robert Lee, M.D. F.R.S. With two plates.—London, 1841. Folio, pp. 8.

9. Essays and Heads of Lectures on Anatomy, Physiology, Pathology, and Surgery. By the late Alex. Monro Secundus, M.D. F.R.S.E. &c. upwards of fifty years Professor of Anatomy and Surgery in the University of Edinburgh, with a Memoir of his Life, and copious notes, &c. by his Son and successor. With engravings.—Edinb. 1840. 8vo, pp. 292.

10. The Cause and Treatment of Curvature of the Spine, and Diseases of the Vertebral Column. Illustrated with cases and plates. By E. W. Tuson, F.R.S. F.L.S. Surgeon to the Middlesex Hospital.—London, 1841. 8vo, pp. 283. 10s. 6d.

11. The Domestic Management of the Sick Room, necessary, in aid of Medical Treatment for the Cure of Diseases. By A. T. Thomson, M.D. F.L.S. &c.—London, 1841. 8vo, pp. 506. 10s. 6d.

12. Elements of Physiology for the use of Students, and with especial reference to the wants of Practitioners. By Rudolph Wagner, M.D. Professor of Anatomy in the University of Gottingen. Translated from the German, with additions, by Robert Willis, M.D. &c. Part I. on Generation. —London, 1841. 8vo, pp. 229. 10s. 6d.

13. The Library of Medicine. Arranged and edited by Alexander Tweedie, M.D. F.R.S. Vol. VI. A System of Midwifery. By Edward Rigby, M.D.—London, 1841. 8vo, pp. 314. 10s. 6d.

14. The Mineral Springs of England, and their Curative Efficacy: with remarks on bathing, and on artificial mineral waters. By Edwin Lee, Esq. M.R.C.S.—London, 1841. 12mo, pp. 124. 3s. 6d.

15. Seventh Annual Report of the Trustees of the State Lunatic Hospital at Worcester (U. S.).—Bost. 1840. 8vo, pp. 102.

16. The American Medical Almanack for 1841, designed for the daily use of practising Physicians, Surgeons, Students, and Apothecaries, &c. By J. V. C. Smith, M.D. Vol. III. Continued annually. 18mo, pp. 148.

17. A few Hints addressed to Medical Students about to visit the Parisian Hospitals. By a Physician.—London, 1841. 12mo, pp. 56. 1s. 6d.

18. Annual Report by the Managers of the Royal Edinburgh Lunatic Asylum for the year 1840.—Edinb. 1841. 8vo, pp. 14.

19. The Surgical Anatomy of Inguinal Hernia, the Testis and its coverings. By Thos. Morton, Demonstrator of Anatomy in University College.—London, 1841. Royal 8vo, pp. 330. 9s. Illustrated with lithographic plates and wood engravings, 12s.

20. A Practical Treatise on the Venereal Disease. Founded on lectures delivered at the Aldersgate School of Medicine. By F. C. Skey, F.R.S. Assistant Surgeon of St. Bartholomew's Hospital. With plates. —London, 1841. Sm. 8vo, pp. 195. 4s. 6d.

21. Observations on the Management of Madhouses, Part the Second. By Caleb Crowther, M.D.—London, 1841. 12mo, pp. 104. 2s. 6d.

22. A Series of Anatomical Sketches and Diagrams. Part IV. By T. Wormald and A. M. M'Whinnie, Esq.—London, 1841.

23. A Practical Treatise on the Diseases of the Liver and Biliary Passages. By W. Thomson, M.D. one of the Physicians of the Royal Infirmary of Edinburgh.—Edinb. 1841. 8vo, pp. 306. 8s.

24. A Treatise on the Physiological and Moral Management of Infancy. By Andrew Combe, M.D. Second edition.—Edinb. 1841. 8vo, pp. 380. 6s.

25. Statistical Researches relative to the Etiology of Pulmonary and Rheumatic Diseases. By S. Forry, M.D. &c.—Philadelphia, 1840. 8vo, pp. 44.

26. Medical Communications of the Massachusett's Medical Society. Vol. VI. Part IV.—Boston, 1840.

27. The Principles and Practice of Obstetric Medicine and Surgery, in reference to the process of Parturition. With 100 illustrations on steel and wood. By F. H. Ramsbotham, M.D.—London, 1841. 8vo, pp. 672. 1*l*. 2s.

28. New Remedies: the method of preparing and administering them, &c. Third edition, with numerous modifications and additions. By R. Dunglison, M.D. Sec. A.P.S.—Philadelphia, 1841. 8vo, pp. 541.

29. The Anatomy of the Arteries of the Human Body, with an atlas of plates. By Richard Quain, and Joseph Maclise, Esq. Parts IV. and V.—London, 1841. 12s.

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